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CONTENTS

CONTENTS

CONTENTS

CONTENTS

CONTENTS

CONTENTS

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By H. Spencer-Jones, M.A., B.Sc., Royal Observatory, Greenwich.

Ionisation in Stellar Atmospheres.—If, on the occasion of a total eclipse of the sun, the slit of a spectroscope be set upon the bright limb of the sun and a plate exposed immediately before the total phase commences or immediately after its termination, the bright line or "flash" spectrum of the upper layers of the solar chromosphere is obtained. It has long been known that this spectrum is characterised by those lines which are relatively stronger in the spark than in the arc spectra, lines which were designated by Lockyer as enhanced lines. Moreover, it is only such lines as are enhanced which reach to the highest levels of the chromosphere. The reason why such substances as calcium occur at the highest levels to the apparent exclusion of other and lighter substances has been the subject of much discussion. An explanation could doubtless be given if the cause of the enhancement in the spark of certain lines could be stated. This is connected with the physical mechanisms of the arc and spark. Lockyer supposed that passage from the arc to the spark meant a great though localised increase in temperature. That this could be the explanation of the enhancement of lines in the flash spectrum is not probable, for it would involve the supposition that the outer chromosphere is at a higher temperature than the lower layers or photosphere. It is, perhaps, most convincingly disproved by the experiments of Anderson at the Mount Wilson Observatory, who has recently been studying the spectra produced by fine wires, exploded by the discharge of a large condenser. The light source so produced had an intrinsic brightness of about 100 times that of the solar surface, corresponding to a black-body temperature of 20,000° C., yet the principal enhanced lines were absent.

The more plausible theory has been put forward that the enhanced lines are due to radiations not from a normal atom of the element but from an ionised one, i.e. an element which has lost an electron. Thus, for instance, in the case of calcium

the H_1K lines are enhanced lines and leading members of the principal pair-series, whilst the *g* line, of the normal type, is the first member of the system of single lines. Now Fowler has shown that the spectroscopic constant of the series formula of the double lines is 4N, instead of the usual Rydberg number N, which is that of the single-line formula. On Bohr's theory of the atom it follows that the radiating system has effectively a double charge, so that the system has been produced by the removal of one electron from the normal atom.

Assuming this theory, the highest level of the chromosphere would require to be the region of the most intense ionisation. The quantitative application to the solar chromosphere has been worked out recently by M. N. Saha (*Phil. Mag.*, **40**, 472 and 809, 1920), using a method developed by Eggert (*Phys. Zeitschrift*, December 1919), based upon Nernst's "Reaction-isobar." The ionisation of a calcium atom may be regarded as taking place according to the scheme:

$$Ca \rightleftarrows Ca_{+} + e - U$$

where Ca denotes a normal atom of calcium vapour, Ca_{+} an atom which has lost one electron, and U the quantity of energy liberated in the process. The latter can be calculated from the value of the ionisation potential V of the element, if known, for the amount of energy required to detach one electron from the atomic system is eV/300, V being measured in volts. If U refers to one gm-atom and N is Avogadro's number, J the mechanical equivalent of heat, then

$$U = eV \,.\, N/J \,.\, 300 = 2{\cdot}302 \times 10^4 V \text{ calories.}$$

The thermodynamical equation of gaseous equilibrium is applied to the above reaction. C_p denoting the specific heat at constant pressure, Saha assumes that $(C_p)_{Ca} = (C_p)_{Ca+}$ and that $(C_p)_e = \frac{5}{2}R$, the electron being supposed to behave like a monatomic gas. The chemical constant of the electron is calculated from the Sacker-Tetrode relation, taking for the atomic weight of the electron 1/1836, and is found to be $-6{\cdot}5$. In this way the equation of the reaction-isobar is found to be:

$$\log \frac{x^2}{1-x^2} P = -\frac{U}{4{\cdot}571\,T} + 2{\cdot}5 \log T - 6{\cdot}5$$

In this formula x denotes the fraction of the calcium atoms which are ionised, P the total pressure, and T the absolute temperature. The formula makes it evident to what a large extent the degree of the ionisation is dependent upon the pressure, a reduction in the pressure greatly increasing the ionisation.

It is now possible to calculate the degree of ionisation

corresponding to various pressures and temperatures for any element whose ionisation potential has been determined. Taking, for instance, the case of calcium, Saha finds that the ionisation will be practically complete for a temperature of 14,000° at one atmosphere pressure; for a temperature of 11,000° at 1/10 atmosphere; of 9,000° at 1/100 atmosphere; of 7,000° at 1/10,000 atmosphere, etc. Assuming that the temperature in the photosphere is 7,500° with a pressure of the order of one atmosphere, falling to 6,000° in the outermost layers with a partial pressure of 10^{-11} atmospheres, the extent of the ionisation in the solar chromosphere may be determined. In the case of calcium, therefore, when the pressure falls to 10^{-4} atmosphere, practically all the atoms get ionised. Hence up to this point, the combined emission of the H_1K and *g*-lines is obtained, but above this point there is only that of the H and K lines. In the case of strontium and barium, which have a lower ionisation potential, ionisation is practically complete at 10^{-3} atmosphere, so that the heights shown by the lines of the unionised atoms of these elements are still lower. Hydrogen, on the other hand, is completely dissociated into atoms throughout the chromosphere, but is not appreciably ionised except at the very highest levels. For helium to have appreciable ionisation, a temperature of at least 16,000° is necessary. In a similar way, Saha shows that the theory accounts for the behaviour of sodium lines in the Fraunhofer spectrum and for their intensification in the spot spectrum; for the faint occurrence of the potassium lines and for the complete absence of the lines of caesium and rubidium. It therefore seems that the theory will be able to account for the presence or absence of the lines of various elements in the Fraunhofer spectrum, a complete explanation not being possible at present on account of lack of information as to the ionisation potentials of many elements. No evidence has been found of the existence in the sun of rubidium, caesium, nitrogen, phosphorus, boron, antimony, bismuth, arsenic, sulphur, selenium, thallium, praseodymium. The following elements are doubtful: radium, the inert gases (except helium), osmium, iridium, platinum, ruthenium, tantalum, thorium, tungsten, uranium. The following elements are represented only by faint lines: potassium, copper, silver, cadmium, zinc, tin, lead, germanium. Certain elements such as chlorine, bromine, iodine, fluorine, tellurium, have not been investigated. A few elements, such as calcium, iron, vanadium and titanium, are unusually prominent. The theory confirms these facts where it can give any information, and strongly supports the view that the varying records of different elements in the Fraunhofer spectrum may be regarded as arising from the varying

response of these elements with regard to the stimulus existing in the sun.

Saha applies the same methods to the problem of temperature radiation in gases (*Phil. Mag.*, **41**, 267, 1921). The vapours of some elements become luminous at moderate temperatures, whilst permanent gases remain non-luminous at the highest laboratory temperatures. The higher the ionisation-potential of the element, the more difficult it becomes to cause it to emit its line-spectrum. Now, the transition from a neutral state to the ionised state is not abrupt, but proceeds through a series of successive stages of equilibrium of the system with an increasing number of quanta of angular momenta. It follows that the radiation of normal lines will precede ionisation, and that the temperature of emission of a certain group of lines bears a definite relation to the temperature of ionisation. The higher therefore the temperature of complete ionisation, the higher will be the temperature of luminescence. In this way—and by considering the second stage ionisation of the elements—light is thrown upon the nature of spectral variation with increasing age of a star, and the conclusion is reached that the continuous variation of stellar spectral types can mainly be ascribed to the varying value of the temperature of emission of the stellar atmospheres. Thus the following phenomena are related with the temperatures assigned and with the spectral class stated :

Phenomena.	Temp.	Remark.	Spectral Type.
H luminescence begins . .	4,500°	Appearance of (2*p*) orbit .	M*b*
Appearance of K line . .	5,000°	Ca ionisation commences .	M*a*
Appearance of Mg line(4481) .	7,500°	Mg ionisation commences .	Go
Mg luminescence a max. .	11,000°		A2
H luminiscence a max. . .	12,000°		Ao
Disappearance of *g* line .	13,000°	Ca ionisation complete .	B8
Appearance of He . . .	16,000°	He ionisation commences .	B3
Maximum luminiscence of He	17,000°		B2
Disappearance of K line .	19,000°	Ca^+ ionisation complete .	O*d*
Disappearance of 4481 line .	23,000°	Mg^+ ionisation complete .	O*a*
Disappearance of He . .	30,000°	He ionisation complete .	P*e*

As far as the theory can be tested by observation, it appears to be confirmed. Additional determinations of ionisation potentials will enable it to be further tested. At present, it may be regarded as a reasonable working hypothesis for explaining the differences between the Fraunhofer and flash spectra, and the progressive change in the nature of stellar spectra with increase in temperature.

Stellar Parallaxes.—In SCIENCE PROGRESS, **11**, 97, 1916, an account was given of a method of determining the parallax

of a star by spectroscopic methods, developed at the Mount Wilson Observatory. The method depends upon the discovery that amongst the stars of the same spectral type (and therefore of approximately the same temperature), differences in the relative intensities of certain lines in the spectra can be detected which can be utilised as criteria for the determination of absolute or intrinsic magnitudes. In conjunction with a knowledge of the apparent magnitude, the absolute magnitude enables the distance of the star to be assigned.

An important list of the parallaxes of 1,646 stars has recently been published by Adams and his collaborators at Mount Wilson (*Astroph. Journ.*, **53**, 13, 1921). The values of the parallaxes obtained by this method are found to have probable errors of about 20 per cent. On the face of it, this appears to be a large error, but it must be compared with the errors to which the direct trigonometrical determinations are liable if it is to be properly appraised. The probable errors of the most reliable trigonometrical determinations are not much less than $''{\cdot}01$ and therefore exceed 20 per cent. in the case of all parallaxes less than $''{\cdot}05$, i.e. for stars whose distances are less than about 65 light years. For the nearest stars, therefore, it appears that the trigonometrical method is the more reliable, but for the more distant stars, the probable error of a spectroscopic determination becomes less than can be obtained by the best photographic methods. It is for these stars, therefore, that the spectroscopic method will prove of the greatest value.

A careful examination reveals that these parallaxes are not affected by systematic errors of any magnitude, and they can therefore be used in stellar investigations without prejudice and in conjunction with directly determined parallaxes. The potency of the new method is sufficiently indicated by the list of parallaxes now published—the output of a single observatory. These have all been derived in the short period of about five years, much of which time has been occupied in fixing the bases of the method. The number of reliable trigonometrical parallaxes cannot exceed about a couple of thousand, although at present results are being published with great rapidity from several observatories : many of the spectroscopic results refer, of course, to stars whose parallaxes have also been trigonometrically determined, but the list includes, in addition, new parallaxes for several hundred stars. It therefore provides a very important contribution to our knowledge of stellar distances, a knowledge which is of the utmost importance in many investigations.

The development of this new method, from which further important results may be anticipated, together with the perfection of photographic methods of determining parallaxes

trigonometrically, has entirely revolutionised our knowledge of stellar distances. To realise the magnitude of the recent advance in knowledge in this direction, it is only necessary to recall that several years ago, the number of reliable parallaxes was only about a couple of hundred.

The following is a selection from recent papers:

Sampson, R. A., Theory of the Four Great Satellites of Jupiter, *Memoirs, R.A.S.*, **63**, 1921.

Martinez, H. A., Determinación de la órbita del planeta (796) Sarita, *La Plata Obs. Pub.*, **6**, i, 1920.

Hnatek, A., Spektrum und Bahnelemente von δ Orionis, *Ast. Nach.*, **213**, No. 5090, 1921.

Stebbins, J., A Photo-electric Study of Algol, *Astroph. Journ.*, **53**, 105, 1921.

Stratton, F. J. M., The Spectrum of Nova Geminorum II, *Solar Physics Obs. Cambs.*, **4**, i, 1920.

Brill, A., Die Helligkeitsschwankungen im Spektrum der Nova Geminorum II, *Ast. Nach.*, **212**, Nos. 5086–7, 1921.

Wright, W. H., On the Occurrence of the Enhanced Lines of Nitrogen in the Spectra of the Novae, *M.N., R.A.S.*, **81**, 181, 1921. The Displacements of the Hydrogen Absorption Lines in the Spectrum of Nova Geminorum in March 1912, with remarks upon their interpretation, *ibid.*, **81**, 191, 1921.

Shapley, H., Note on Changes in the Period and Light-curve of the Cluster Variable S.W. Andromedae, *M.N., R.A.S.*, **81**, 208, 1921.

Brackett, F. S., An Examination of the Infra-red Spectrum of the Sun, λ 8900 — 9900, *Astroph. Journ.*, **53**, 121, 1921.

Bernewitz, E., Ueber die Dichten der Doppelsterne, *Ast. Nach.*, **213**, No. 5089, 1921.

Tapia, N., Medidas micrométricas de Estrellas dobles y vecinas, *La Plata Obs. Pub.*, **6**, ii, 1921.

Henroteau, F., The Interstellar Clouds of Metallic Gases, *J.R.A.S.C.*, **15**, 62, 1921.

Russell, H. N., Radiation Pressure and Celestial Motions, *Astroph. Journ.*, **53**, 1, 1921. Note on Sobral eclipse photographs, *M.N., R.A.S.*, **81**, 154, 1921.

Plummer, H. C., On the Question of Stationary Radiants, *M.N., R.A.S.*, **81**, 131, 1921.

Fotheringham, J. K., A Solution of Ancient Eclipses of the Sun, *M.N., R.A.S.*, **81**, 104, 1921.

Mitchell, S. A., Parallaxes of 260 Stars derived from Photographs, *Leander McCormick Obs. Pub.*, **3**, 1920.

Van Maanen, A., and Wolfe, Coral, On the Systematic Differences in Trigonometrically Determined Parallaxes, *Mt. Wilson Conts.*, No. 189, 1921.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

"Investigations on Lightning Discharges and on the Electric Field of Thunderstorms," by C. T. R. Wilson, F.R.S., *Phil. Trans.*, Series A, vol. 221, pp. 73–115.—This is an account of experiments made at the Solar Physics Observatory at Cambridge, mainly during the summer of 1917. For details of the apparatus used, in the designing of which much ingenuity

has been shown, the reader is referred to the original memoir, for space does not permit of more than a brief summary of a few of the results obtained. Thunderstorms in which lightning discharges took place immediately overhead were not experienced, but several took place in the immediate vicinity and were studied in considerable detail. It was observed that the sudden changes of potential gradient near the ground that accompanied each flash of lightning were more often + than −, that is to say a + gradient before the flash was increased, while a − gradient was either diminished or changed into a + gradient. The ratio of + as compared with − changes was 3 to 2. The electric moment of a discharge varied very much, the mean value found being of the order of 100 coulomb-kilometres; assuming that the discharge took place across a distance of 2 km. on the average, there results the value 20 coulombs as representing the amount of electricity discharged in a typical flash. The writer concludes that under these conditions the potential difference across the two kilometres may be considerably less than 6×10^9 volts, but is probably greater than 10^9 volts. The energy dissipated in a flash ($\frac{1}{2}$ QV) would be of the order of magnitude of 10^{10} joules, and assuming that such a flash occurs every 10 seconds, it is interesting to compare this rate of dissipation of energy with the power available, assuming that the rain of the thunderstorm is caught at a height of 1 kilometre. Equality between the two is obtained by assuming that rain falls at the rate of 10 cms. in an hour over an area of 3 square kilometres, which is probably not in excess of what often takes place in actual storms.

The *Quarterly Journal of the Royal Meteorological Society* for January 1921 contains an interesting paper by Captain C. K. M. Douglas, dealing with the variations of temperature in the lowest four kilometres of the atmosphere. The author made numerous observations of the temperature and humidity aloft when flying in Northern France in 1918 and 1919, and found that the temperature was very much affected by the previous course of the current in which the observations were made. An investigation made by W. H. Dines some years ago showed that whereas there exists a close connection between the temperature and pressure in the free air, especially above 4 kilometres, the connection between the temperature and the direction of the wind is insignificantly small. Douglas confirms this result by a statistical treatment of his observations, but finds that if the air is traced back along its course there is seen to be a very considerable connection between the temperature of the air and its position some days previously, currents from the north being cold and those from the south warm. The latter often curve round an anticyclone and so reach France as N.W.

winds, whereas those of polar origin very commonly follow a curved path and arrive as S.W. winds. It is for these reasons that there is such a small connection between the temperature and the direction of the wind at any one time. The matter is not one that lends itself very well to treatment by the method of correlation, and the most convincing arguments are those based on a consideration of the extreme values of observed temperature in relation to the weather maps of the previous few days. The notably low temperatures were all with a current which evidently came from regions north of the Arctic Circle; the highest temperatures were with winds from between south and west (occasionally from N.W.) when there was an anticyclone over France.

The author passes on to consider what bearing his observations have on Bjerknes's theories about cyclones and anticyclones. According to Bjerknes there is in every cyclone a warm sector, where a current of air of equatorial origin forms a sloping surface of discontinuity with the surrounding air (which is of polar origin), this surface meeting the ground so as to form two lines of discontinuity in front of and behind the warm sector, which he designates the "steering" and "squall" lines respectively. Discontinuities such as those postulated were actually met with in the course of flying, at various heights and on several occasions. The very strong winds from S. or S.W. often observed at great heights above the "squall line" of a depression are strong confirmation of the existence of a warm sector of considerable height, for the contrast of temperature would produce a strong pressure gradient high up, due to the more rapid falling-off of pressure with height in the cold dense air behind the warm sector. In front of a depression very strong currents from a northerly direction are well known, and are the counterpart of those already mentioned. They are due to the opposite kind of pressure gradient found immediately in front of the warm sector. The author mentions three points in the paper which he considers to be of special importance, namely:

(1) Both troughs of low pressure and wedges of high pressure normally lie farther west in the upper air than at the surface.

(2) The pressure in the upper air may be regarded as being partly a consequence, and not purely a cause, of the temperature of the underlying column.

(3) Very powerful wind currents are observed at great heights between the polar and equatorial air at those levels.

Bjerknes's Theory of the Polar-Front.—The *Meteorologische Zeitung* of January 1921 contains a short article by Dr. F. M.

Exner which adversely criticises this theory, in so far as it postulates a continuous impenetrable line separating the warm westerly winds of middle latitudes from the cold circumpolar circulation of easterly winds in the northern hemisphere. He points out that the idea of a polar front is not new, but that it has never before been so strongly insisted upon in meteorological circles as recently by Professor V. Bjerknes. Margules, in his well-known work on the energy of storms, has deduced the conditions required for stability at the surface of separation of warm and cold currents. Bjerknes takes the stability for granted in the case of the cold and warm winds mentioned above, and suggests that all cyclones are merely waves at the surface of separation of the two. This very attractive theory is rejected by Exner, who points out that the changes which take place in a depression are not reversible, and therefore cannot be part of a wave motion. In particular Margules has shown that the winds in cyclonic depressions can only have their high velocity explained as a result of the vertical displacement of masses of air of unequal density, in the course of which the centre of gravity of the system is lowered. This is an irreversible process. Moreover, friction at the earth's surface, mixing, and eddy-motion are also irreversible. A depression cannot therefore be part of a reversible oscillation. Looking at the matter from yet another point of view, the motive power of the atmospheric movements being due to unequal heating at the poles and the equator, there must be a circulation of air between middle latitudes and the polar basin ; we therefore cannot suppose that the cold polar air is enclosed, as it were, with an impenetrable membrane. Exner regards the " cold burst " behind the depression as a " break-through " of polar air. He had some years previously advanced the view that this phenomenon is due to the checking of the easterly wind at a particular point in the circumpolar circulation, which accords with Helmholtz's views on the subject.

The Louth " Cloudburst " of May 29, 1920.—The disastrous floods that wrecked numerous houses in Louth (Lincolnshire) and drowned more than twenty of the inhabitants has been the subject of an investigation by the Meteorological Office [Professional Note, No. 17, by E. V. Newnham]. The flooding began so suddenly and assumed such extraordinary proportions that it is difficult to believe that mere rainfall, even of the heavy kind associated with thunderstorms, could have been its cause, yet this appears to have been the case. The rainfall records showed that a considerable area of the Wolds to the west of Louth experienced over 100 mm. of rain in about three hours, and a large proportion of this had to pass through Louth along the course of the Lud (normally a mere brook) before it

could spread out over the flat country between the town and the sea. The evidence all seems to show that the suddenness of the flood was not due to a climax in the rate at which the rain fell, but rather to the breaking-down of various dams that had been formed by drifting débris, which were holding up a great accumulation of water on the western outskirts of the town. Some damage was, however, done along the course of every brook within the area of heavy rain and even in the open fields on the hilltops, while Horncastle, which is on the opposite side of the watershed, suffered severely. The storm was an interesting one from the purely meteorological point of view. A large area of light variable breezes extended over most of Europe on the morning of May 28, and considerable heating of the air occurred on the Continent, the temperature rising to 94° F. at Clermont-Ferrand and 86° F. at Lyons. A depression approached the English Channel from a southerly direction in the course of the day, and during the following night the heated air over France penetrated to the south of England, causing an exceptionally warm and muggy night. The southerly breezes advanced northwards during the following morning, converging upon Lincolnshire by early afternoon. North of the Humber there was a relatively cold wind blowing in from the North Sea, the contrast of temperature amounting to nearly 20° F. The warmer of the two currents appears to have ascended over the colder, and being very damp the dynamical cooling gave rise to exceptionally heavy rain and severe thunder and lightning. Thunderstorms occurred in many parts of the Midlands and Northern England, but the storm over the Wolds was the most severe. At its height it covered an area measuring fully 60 miles from south to north, and was accompanied by funnel-shaped clouds in some places, which, however, do not appear to have reached the ground so as to form veritable tornado clouds. Violent squalls occurred south of the centre, but in the devastated area there was little wind near the ground. A southerly wind was not established near the ground in Lincolnshire until some hours after the rain had ceased. The evidence strongly supports the theory that the whole phenomenon was due to the passage of the one wind over the other, accompanied by much convectional ascent of moist air and consequent precipitation.

PHYSICAL CHEMISTRY. By W. E. Garner, M.Sc., University College, London.

The Lewis-Langmuir Theory of Valency.—The structure which is ascribed to nitrogen, carbon monoxide, nitrous oxide, and carbon dioxide by Langmuir has received support recently from several directions. The nitrogen molecule on this theory

consists of an outer cell of eight electrons, surrounding two nitrogen nuclei and two electrons. Since the number of the outer electrons is the same as that in the rare gases, certain points of resemblance should exist between these gases and nitrogen. The nitrogen molecule should behave as an elastic body approximately spherical in shape. Rankin (*Proc. Roy. Soc.*, 1921, [A], **98**, 360) from measurements of viscosity comes to the conclusion that whereas the kinetic behaviour of oxygen, chlorine, bromine, and iodine is in accord with a two-celled structure for these gases, this is not the case for nitrogen, which is abnormal. J. J. Thomson (*Phil. Mag.*, 1921, **41**, 542), in a theory of the structure of the molecule and chemical constitution, points out that the results of Rayleigh (*Proc. Roy. Soc.*, 1920, [A], **98**, 57) on the scattering of polarised light by gaseous molecules, show a marked difference between the shape of the nitrogen and oxygen molecules. The oxygen molecule deviates to a greater extent than nitrogen from sphericity. The resemblance between the rare gases and nitrogen is shown by the remarkable fact that whereas hydrogen, chlorine, carbon, and oxygen readily take up negative charges, neither neon nor nitrogen has ever been observed with a negative charge. The work of Franck and Herz supplies another point of resemblance. The mobility of electrons in gases is determined by the nature of the outer cell of electrons, and in neon and nitrogen, where it is assumed that there is a complete octet, the electron is rarely retained by the molecular system. The mobility of the electron in these gases is therefore considerably greater than in oxygen and chlorine, where the bicellular arrangement is readily broken down by the addition of electrons.

A three-cell structure is ascribed to carbon dioxide and nitrous oxide by Langmuir, the three cells being produced by the sharing of electrons in pairs between the three atoms. According to Rankin (*Proc. Roy Soc.*, 1921, [A], **98**, 369) the molecules of CO_2 and N_2O behave not merely as if they had the same size and shape, but also as if each had an external electronic arrangement like that of three neon atoms with their centres in a line. Such a hypothetical molecule composed of three neon atoms would have a mean target area of $0{\cdot}895 \times 10^{-15}$ cm^2. On the basis of viscosity results Rankin obtains the values $0{\cdot}870 \times 10^{-15}$ cm^2 for CO_2 and $0{\cdot}867 \times 10^{-15}$ cm^2 for N_2O. These data are a striking confirmation of the Langmuir theory.

Metallic Hydrides.—Within the last few years the number of the known gaseous hydrides of the elements has been materially increased. Polonium, bismuth, tin, and lead all yield hydrides if brought into contact with hydrogen in an active form (Paneth, *Zeit. Elektrochemie*, 1920, **26**, 452, and *Ber.*, 1920, **53**b, 1693). Paneth found that thorium C and thorium B when deposited on

magnesium and treated with acid gave very small quantities of volatile hydrides, which were readily decomposed by heat. Since these elements are isotopes of bismuth and lead respectively, it was concluded that the latter two elements were also capable of giving hydrides. An alloy of bismuth with magnesium when treated with 4 n. HCl gave bismuth hydride in sufficient quantity to be liquefied with liquid air and to be evaporated through a heated Marsh tube. A bismuth mirror was obtained. A tin magnesium alloy gave tin hydride in the same way, whereas the experiment with lead gave a doubtful result. Numerous electrical methods also failed, but by a modification of Bredig's dispersion method, using a lead cathode and dilute sulphuric acid as an electrolyte, the lead hydride was obtained. Paneth concludes that the formation of the gas takes place between the negative lead ions and the positive hydrogen ions.

It is of interest to consider the relationship between the atomic number of an element and its ability to form gaseous hydrides. Of the twenty elements forming gaseous hydrides, all except boron possess atomic numbers which are less than those of the corresponding rare gases by 1–4 units. In the Staigmüller system of classification of the elements, all of those in the last four groups form gaseous hydrides.

Nernst (*Zeit. Electrochemie*, 1920, **26**, 323), in an experimental and theoretical investigation into the properties of lithium hydride, has shown that this substance is an analogue of lithium chloride. The crystal form, heat of formation, atomic heat, atomic volume, colouring by ultraviolet light, etc., of these two substances support this analogy. This replacement of hydrogen by chlorine, without any marked change in the character of the compounds, resembles the change from acetic to chloracetic acid. Nernst has applied the heat theorem to the properties of lithium hydride and calculated the dissociation of H_2 into positive and negative ions according the equation

$$H_2E_2 = H^{\cdot} + HE_2(= H')$$

and finds $K = 0{\cdot}55 \times 10^{-39}(C_0)$.[1]

This method of dissociation may also apply to the oxygen molecule, which would thus dissociate into positive and negative ions. This would provide an explanation of the behaviour of oxygen in autoxidation.

Periodic Phenomena in Electrolysis.—An interesting contribution to this subject has been made by Liebreich (*Zeit. Elektrochemie*, 1921, **27**, 94) on the electrolysis of a solution of chromic acid between iron and platinum electrodes. The periodic evolution of gases or the deposition and solution of oxides and metals during electrolysis has long been observed, especially with iron

and chromium. These phenomena, which occur at both the anode and the cathode, are usually explained as due to an alternation between an active and a passive state of the electrode. As was shown by Nernst, the potential between the electrodes could in many cases be altered by shaking or moving the electrodes.

Kistiakowski had previously investigated the electrolysis of chromic acid using irone lectrodes and obtained periodic phenomena both at the anode and cathode. In order to eliminate the anodic disturbance and to study the cathodic processes, Liebreich used a platinum anode. He finds that there are four distinct processes which occur at the cathode :

I. $Cr^{\cdot\cdot\cdot\cdot\cdot\cdot} \rightarrow Cr^{\cdot\cdot\cdot} + 3 \odot$
II. $2CrO_3 + 6H^{\cdot} \rightarrow Cr_2O_3 + 3H_2O + 6 \odot$
III. $Cr_2O_3 + 2H^{\cdot} \rightarrow 2CrO + H_2O + 2 \odot$
IV. $2H^{\cdot} \rightarrow H_2 + 2 \odot$ and $CrO + 2H^{\cdot} \rightarrow Cr + H_2O + 2 \odot$

No hydrogen is evolved in stages I and II, a slight evolution occurs in III, and a vigorous evolution in IV. To each of these processes corresponds a definite decomposition potential curve, which can be realised between certain limits of current density and voltage. Which of these processes is taking place at a given time is determined partly by the applied E.M.F. and partly by chance. Within certain ranges of current density, the voltage suddenly changes from one curve to another, and it is the repetition of this process which gives rise to a periodic change in the voltage, which is accompanied by a corresponding change in the products liberated at the electrode. The order of the voltage change is 0·3 V. In addition to these large changes pulsating phenomena were observed of the order of 0·005 V.

These periodic occurrences correspond with a reduction or oxidation of the electrode deposits, and the duration of the period depends on the nature and the thickness of these deposits. No steady conditions of current density and voltage were found except those corresponding to points on the four curves.

Ultramicroscopic Structure of Soaps.—W. F. Darke, J. W. McBain, and C. S. Salmon (*Proc. Roy. Soc.*, 1921, [A], **98**, 395) have applied the cinematograph to the study of the "life-history" of the ultramicroscopic structures which are formed on cooling soap solutions, and have extended the work of Bachman and Zsigmondy in this field. Both soap sols and soap gels were found to be transparent under the ultramicroscope. The ionic micelle is invisible, but on cooling slowly Brownian particles appear. These are followed, in the sodium soaps, by long thin fibres of characteristic curvature, which grow slowly into the field, and in many cases it appeared that the fibres had attained their full length before becoming visible.

The fibres were composed of hydrated neutral soap. Their cross section was in nearly all cases ultramicroscopic, and those of apparently macroscopic size were shown to be bundles of fibres. The fibres were shorter in the case of potassium soaps, and on standing these salts gave crystals, which were distinguished from the fibres by their higher melting-point.

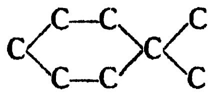
Surface Tension of Solutions of Fatty Acids.—Traube and Somogyi first suggested the use of surface tension methods in alkalimetry and acidimetry. A base which lowers the surface tension of water may be used as an indicator in the titration of an acid by a stronger but an inactive base. Windisch and Dietrisch (*Kolloid Zeitschrift*, 1920, **26**, 193), in the course of an investigation into the suitability of the nonylic, caprinic, and undecylic acids as indicators in acidimetry and alkalimetry, found that solutions of these acids show anomalous properties. A freshly made solution of undecylic acid in water gave a low value for the surface tension, which increased to that of water in the course of a few days. Addition of hydrochloric acid to the freshly prepared solution produced a lowering of the surface tension, which is not, however, permanent. After several days the solution became cloudy and the surface tension rose to that of water. The results were independent of the nature of the containing vessel. The change in the activity of the undecylic acid on addition of mineral acid was accompanied by the appearance of colloidal particles visible in the ultramicroscope, which finally became visible to the naked eye. The authors conclude that the molecular disperse phase of the acids is without influence on the surface tension of the water, whereas the colloidal particles, visible in the ultramicroscope, are active. On the other hand the coarse suspension is inactive. It is suggested that both the active and inactive forms of the acids occur simultaneously in aqueous solution.

ORGANIC CHEMISTRY. By O. L. Brady, B.A., D.Sc., F.I.C., University College, London.

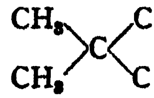
Baeyer's Strain Theory.—In a paper on the formation and stability of spiro-compounds (*Trans. Chem. Soc.*, 1920, **117**, 1579) Becker and Thorpe adduce further evidence in support of the ideas enunciated in 1915.

In the earlier paper (*Trans. Chem. Soc.*, 1915, **107**, 1080) Beesley, Ingold, and Thorpe investigated the formation and stability of spiro-compounds in the light of Baeyer's strain theory. This theory stipulates that the normal angle (109° 30′) between the valency directions of the carbon atom has to be altered to form the cyclohexane ring; it is possible therefore that the angles between the remaining valency directions will be modified accordingly. If the two remaining valency

directions of a carbon atom in a cyclohexane ring distribute themselves equally in the remaining space the angle between them will be 107° 15′—that is, the groups attached, as side-chains, to such a carbon atom will be closer together than in a corresponding open-chain compound. In these circumstances there should be a marked difference in the reactions involving further ring formation between compounds of types *a* and *b*,



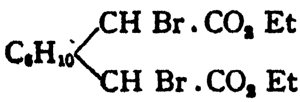
(*a*)



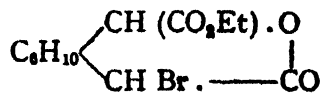
(*b*)

and the spiro-compound containing the trimethylene ring prepared from type (*a*) should be more stable than the corresponding derivative from type (*b*).

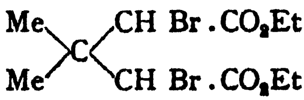
Experimental evidence has shown that this is the case. For example, the dibromo-ester of cyclohexane—1 : 1—diacetic acid [I] is unstable, readily eliminating ethyl bromide to give the bromo-lactone ester [II] unlike the corresponding dibromo-ester of $\beta\beta$-dimethyl glutaric acid [III]



[I]

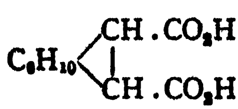


[II]

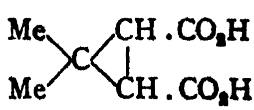


[III]

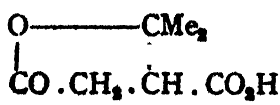
Again, cyclohexane-spiro-cyclopropane-dicarboxylic acid [IV] resists the action of concentrated hydrochloric acid at 240°, whereas caronic acid [V] is transformed into terebic acid [VI] by 5 per cent. acid at 200°.



[IV]



[V]

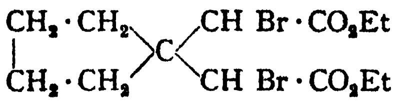


[VI]

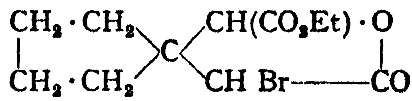
In the latest paper on this subject the derivatives of cyclopentane are considered. In the case of a carbon atom in

a cyclopentane ring, making the same assumption as before, the angle between the remaining valency directions which do not take part in ring formation is 109° 46′—that is, differs by but a few minutes from the normal angle. It would be expected, therefore, that derivatives from cyclopentane would bear a closer resemblance to those in which there was no ring present than to the corresponding cyclohexane compounds, although the latter more closely follow them in molecular weight. Here again the experimental facts are in accordance with the prediction.

Ethyl *aa′*-dibromo-cyclopentane – 1 : 1 – diacetate [VII] like the corresponding derivative of dimethyl glutaric acid, can be distilled under reduced pressure practically unchanged, only some 10 per cent. being converted into the bromo-lactone ester [IX] though in similar circumstances the cyclo-hexane-bromo-ester [I] is completely changed to the bromo-lactone ester [II].

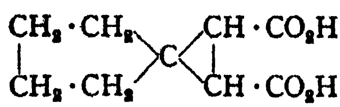


[VII]



[IX]

Similarly cyclopentane-spiro-cyclopropane – 1 : 2 – dicarboxylic acid [X] resembles caronic acid [V] in its action with 5 per cent. hydrochloric acid at 200° and differs from the corresponding cyclohexane derivative, which is unaffected.



[X]

More recently, Ingold (*Trans. Chem. Soc.*, 1921, **119**, 305) has suggested an explanation of the failure of Baeyer's strain theory to account for the ease of formation and for the stability of some carbon rings. The heats absorbed in ring formation of similarly constituted compounds by removal of two atoms of hydrogen from the open-chain compounds are, according to Stohmann and Kleber:

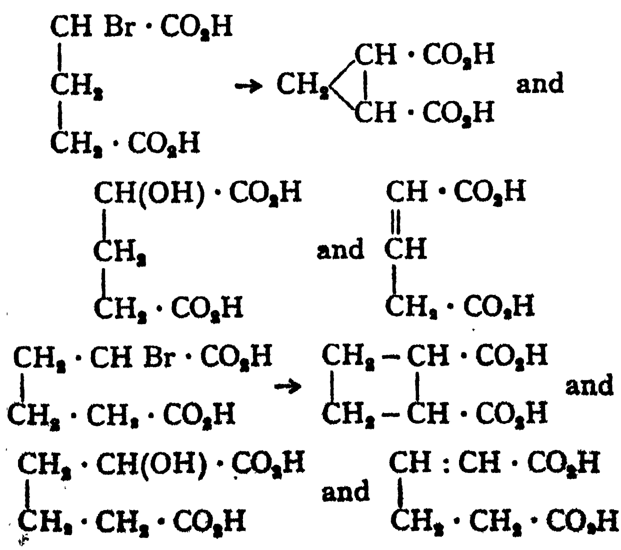
	Cyclo-propane.	Cyclo-butane.	Cyclo-pentane.	Cyclo-hexane.
Heat absorbed	38·1	42·6	16·1	14·3
Angle of strain	24·7°	9·7°	0·7°	5·3°

The thermo-chemical data indicate that cyclobutane is the least stable ring, and cyclohexane more stable than cyclopentane, a state of affairs quite at variance with the predictions of Baeyer's theory, but in accord with a number of experimental observations.

The angle assumed by Baeyer between the valency directions (109·5°) is the angle at the centre of a regular tetrahedron subtended by a side and it is now suggested that the tetrahedron representing the carbon atom is regular only when four similar groups are attached. In the polymethylenes, where two carbons and two hydrogen atoms are attached to the carbon atom, account has to be taken of the relative sizes of the atoms, and the corresponding effect on the regularity of the tetrahedon. If it is assumed that the central carbon atom is a sphere touched by four spheres representing the hydrogen and other carbon atoms, these being in contact with each other and their volumes proportional to the atomic volumes of carbon and hydrogen, by joining the centres an irregular tetrahedron is obtained. It can be shown that in this tetrahedron the angle between the carbon to carbon valency directions is 115·3°. Using this angle the relative stability of the various rings as shown by the strain present is in accord with the thermochemical results.

In order to test his assumptions the author proposes to study quantitatively the ease of formation of the various methylene rings. In order to do this reactions are being investigated in which one or more side reactions can compete with ring formation. In these circumstances the relative amounts of products formed under standard conditions will be a measure of the ease of ring formation.

The example chosen is the removal of hydrobromic acid from *a*-bromo-glutaric, *a*-bromo-adipic, etc., acids. The ring formation and the competing reactions in these cases will be:

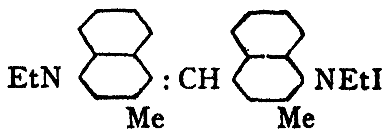


The present paper deals with the first acid and indicates that ring formation takes place comparatively readily. On the other hand, the investigation of bromo-adipic acid, of which the author promises details in a subsequent paper, showed a marked difference in the ease of formation of the tetramethylene ring. The author's results, therefore, are so far in accordance with the thermo-chemical results and his own hypothesis.

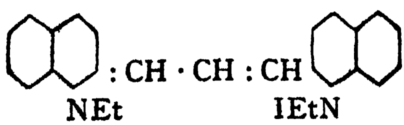
The Constitution of Pinacyanol.—Although pinacyanol (diethylcarbocyanine iodide), prepared by the action of alkali and formaldehyde on a hot alcoholic solution of a mixture of quinoline ethiodide and quinaldine ethiodide, was patented as a photographic sensitiser as early as 1905, its structure has been a matter of doubt.

Two formulæ have been suggested, [XI] by O. Fischer and [XII] by Wise, Adam, Stewart, and Lund. Mills and Hamer (*Trans. Chem. Soc.*, 1920, **117**, 1550) prefer, however, that pinacyanol should be represented as [XIII].

[XI]



[XII]

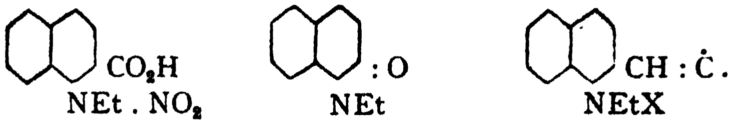


[XIII]

There seems to be no doubt that pinacyanol contains but one monovalent acid radicle, and from careful analyses of the iodide and bromide the molecular weight has been shown to be 479 ± 1. This rules out Fischer's formula.

Although better yields of the pinacyanol are obtained when a mixture of quinaldine alkyl iodide and quinoline alkyl iodide is used rather than the quinaldine compound alone, the quinoline alkyl iodide plays no direct part in the condensation. This was discovered by Fischer, and confirmed by the present authors by showing that by the action of alkali and formaldehyde on a mixture of p-toluquinaldine ethiodide and quinoline ethiodide the dye produce was homogeneous, and consisted of 1:1′-diethyl-6:6′- dimethylcarbocyanine iodide. The untenability of the formula [XII] is shown by the oxidation of pinacyanol. Oxidation of the bromide with dilute nitric acid yields quinaldinic acid ethyl nitrate [XIV] in amounts corresponding to about 90 per cent. of the calculated quantity

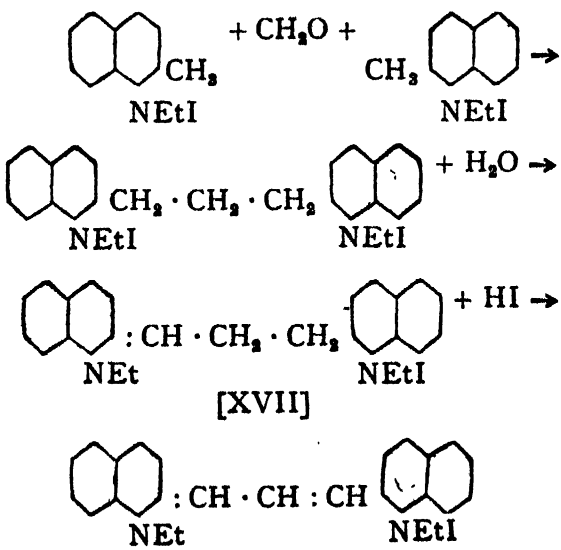
if one quinaldine residue is the source. Further oxidation with alkaline ferricyanide of the residue after removal of this compound gave 1-ethyl-2—quinolone [XV] in quantities which indicated that it must be derived from the remaining part of the molecule.



[XIV] [XV] [XVI]

The smoothness and ease of the oxidation with nitric acid suggests that the quinaldine residue which gives the quinaldinic acid ethyl nitrate is attached to the rest of the molecule by a double bond, and it is inferred that the carbocyanine contains the grouping [XVI].

It is suggested that the mechanism of the reaction is as follows :



[XVII]

It is not considered possible for a compound of the structure [XVII] to be as strongly coloured as pinacyanol, and the oxidation with nitric acid suggests the presence of the ethylene linking. The formation of pinacyanol therefore involves oxidation or dehydrogenation, and it is suggested that the greater yields obtained by bringing about the condensation in the presence of quinoline ethiodide may be due to this substance giving rise to compounds which serve to take up hydrogen. It may be suggested that the quinoline ethiodide condenses

with the formaldehyde to give $C_9H_8N(EtI) \cdot CH_2OH$, which is reduced to quinaldine ethiodide.

Synthesis of Ephedrine and ψ-Ephedrine.—The investigation of naturally occurring organic bases is being actively pursued. One of the most recent syntheses is that of Spath and Göhring (*Monatsb.*, 1920, **41**, 319), who have confirmed the previous views of the structure of ephedrine and ψ-ephedrine by synthesising these compounds from propyl aldehyde as follows:

$$CH_3 \cdot CH_2 \cdot CHO \xrightarrow{Br} CH_3 \cdot CH\,Br \cdot CHO \xrightarrow{HBr + CH_3OH}$$

$$CH_3\,CH\,Br \cdot CH\,Br \cdot OCH_3 \xrightarrow{MgC_6H_5Br}$$

$$CH_3 \cdot CH\,Br \cdot CH(C_6H_5)\,OCH_3 \xrightarrow{NH_2CH_3}$$

$$CH_3 \cdot CH(NH\,CH_3) \cdot CH(C_6H_5) \cdot OCH_3 \xrightarrow{HBr.}$$
$$CH_3 \cdot CH(NH\,CH_3) \cdot CH(C_6H_5) \cdot OH$$

A 45 per cent. yield of racemic ψ-ephedrine is thus obtained which was resolved by crystallisation of its *d*-tartrate. The ψ-*d*-ephedrine is identical with the natural product. By heating with hydrochloric acid the *d*- and *l*-ψ-ephedrines have been converted into the *d*- and *l*-ephedrines, and the latter found to be identical with natural ephedrine.

MINERALOGY.—By Alexander Scott, M.A., D.Sc.

Synthetic Mineralogy.—In the last account of this branch of the subject in these reports (Science Progress, **15**, 29, 1920), mention was made of the relations between the substance $2CaO.MgO.2SiO_2$, one of the ternary compounds found in the system $CaO - MgO - SiO_2$ and the natural mineral åkermanite, and the facts in favour of the probable identity of the two substances were indicated. With a view to the elucidation of the problems raised by the variable composition of the mineral and its probable existence as a constituent of melilite, an investigation of the binary system gehlenite-åkermanite has been made. The former was synthesised by G. A. Rankin and F. E. Wright (*Amer. Jour. Sci.*, **39**, 26, 1915) in their work on the system $CaO - Al_2O_3 - SiO_2$, and its optical properties determined. J. B. Ferguson and A. F. Buddington (*ibid.*, **50**, 131, 1920) have now found that the two minerals form a continuous series of solid solutions with well-defined minima on the liquidus and solidus curves, so that the system belongs to Roozeboom's Type III. The minimum melting-point, 1,388° C., is given by the mixture containing 74 per cent. åkermanite and 26 per cent. gehlenite. The curves showing the variation of refractive indices with composition are found to be straight

lines for both the ordinary and extraordinary rays. Since gehlenite is optically negative and åkermanite positive, the two curves intersect, a mixture approximating to 45 per cent. gehlenite and 55 per cent. åkermanite having zero birefringence for sodium light. The density curves for both the crystalline and the glassy series of mixtures are likewise straight lines, but åkermanite itself is abnormal in that the glass has a higher density than the crystalline form.

As a continuation of his well-known work on the equilibria of such complicated systems as those occurring in igneous rock magmas and metallurgical slags, J. H. L. Vogt has published a memoir " Die Sulfid-Silikatschmelzlösungen " (*Norsk Geol. Tidsskr*, 1917 ; *Vidensk. Selsk. Skrift.*, 1918 ; cf. *Geol. Mag.*, **58**, 87, 1921). From the petrographic standpoint, the most important part of the memoir is the discussion of the sulphide segregations which occur in conjunction with norites and peridotites. In this connection, it is notable that sulphide ore-bodies high in nickel are commonly associated with rocks rich in the orthorhombic pyroxenes, such as the norites ; less commonly with the peridotites, which are mainly composed of orthosilicates of iron and magnesium ; and very rarely with such calcic types as the gabbros. From a physico-chemical study of the norites, Vogt comes to the conclusion that while many of the examples of the latter rock are " anchieutectic," the ore-bodies tend to be associated with those types in which the pyroxene is in excess of the eutectic proportions. The segregation of the sulphides is probably to be explained by their comparative immiscibility in the molten condition with silicate magmas, and by their low viscosity. The bearing of the investigations on the problems arising in the smelting of copper matte is also discussed at some length.

In a paper on " Crystallisation Differentiation in Magmas " (*Jour. Geol.*, **27**, 393, 1919 ; *Proc. Nat. Acad. Sci.*, **6**, 159, 1920), N. L. Bowen replies to some of the criticisms of the theory of the origin of rocks by the gravity separation of the early formed crystals (*Jour. Geol.*, **23**, Suppl. 1915). Discontinuous variations in rock masses have been ascribed by various authors (*e.g.* R. A. Daly, *ibid.*, **26**, 117, 1918 ; F. C. Grout, *ibid.*, **26**, 656, 1918) to immiscibility of the two component parts in the liquid state, despite the fact that all the experimental evidence available is in favour of the complete miscibility of all molten silicates. Bowen points out that undercooling cannot be postulated as the reason for the formation of non-consolute magmas on account of the probably limited amount of undercooling which is liable to occur, for example, in batholiths. Further, in the case of abrupt transitions from one rock type to another, such phenomena are even more

difficult to explain on the immiscibility hypothesis, on account of the possible formation of mixtures analogous to emulsions. Rather are such occurrences to be explained by movements in the partly consolidated magma due to "the action of warping stresses, torsional stresses, and in some cases shearing stresses acting at the appropriate stage of crystallisation." In this way, such a structure as the primary banding in the Duluth gabbro may have originated.

The relationships between α-spodumene, the natural mineral, and the β-form, into which the α-variety is transformed on heating to 1,000° C., have been further investigated by F. Meissner (*Zeit. anorg. Chem.*, **110**, 187, 1920). The view of R. Ballo and E. Dittler (*ibid.*, **76**, 39, 1912) that the transformation is irreversible is confirmed by thermal analyses and by density determinations on material annealed at high temperatures. The identity of the γ-variety, formed by the crystallisation of the molten salt, with the β-variety is indicated by a comparison of the optical properties. The behaviour of the felspars on prolonged heating at temperatures just below the melting-points has been examined by H. Leitmeier (*ibid.*, **105**, 69, 1918). It is found that the amount of alkali lost by volatilisation is extremely small, even after seventy-two hours' heating.

In continuation of his work on the problem of the formation of such magmatic felspathoids as nosean, hauyne, and so forth, P. Niggli (*ibid.*, **106**, 126, 1919) has investigated the system $Na_2CO_3 - K_2CO_3 - CaCO_3$ between the temperatures 600° C. and 1,000° C., and under a pressure of 1 atmosphere of carbon dioxide. The binary system, sodium carbonate—potassium carbonate, and that composed of the double salts of each of these with calcium carbonate belong to Roozeboom's Type III, and form a continuous series of solid solutions. In the ternary system are found two series of mixed crystals, one in which there is constantly one molecule of calcium carbonate, that is, mixed crystals of the double salts mentioned above, and another composed of mixed crystals of the three carbonates in varying proportions. The latter are apparently isomorphous mixtures of the alkali carbonates with a small proportion of the calcium salt. Data for the ternary system, sodium carbonate - calcium carbonate - sodium chloride, are also given.

Amongst other papers on mineral syntheses, one, dealing with the hydrothermal formation of silicates, by W. J. Müller and J. Koenigsberger (*ibid.*, **104**, 1, 1918), may be noted. Discussions, from the chemico-geological point of view, of the occurrence of various minerals in some of the German salt-deposits have been published by M. Rózsa (*ibid.*, **101**, 279, 1917; **105**, 167, 1919; *Cent. Min.*, 36, 1917; 121, 1918).

BOTANY. By E. J. SALISBURY, D.Sc., F.L.S., University College, London.

Genetics.—An interesting dwarf mutation of *Portulaca grandiflora* which behaves as a recessive to the normal habit has been investigated by Blakeslee (*Genetics*, July 1920). About 1·25 per cent. of these recessives produced branches which had reverted to the normal type of growth with long internodes. Both the dwarf and reverting branches of these plants were "selfed"; the former bred true, showing that the vegetative mutation only affected the branches exhibiting external change. The "selfed" reverted branches yielded both types in the ratio of three normal to one dwarf, whilst back-crossed to dwarf gave 1 : 1.

In an extensive study of the protein content of maize (*Genetics*, Nov. 1920), East and Jones find that the environmental factors can raise or lower the total per cent. as much as 40 per cent. As a consequence of unit selection from a population during twenty-three years at Illinois Agric. Exp. Sta., high- and low- yield strains were obtained with an average difference of nearly 6 per cent in protein. In five selected strains seeds from the same cob showed variation coefficients ranging from $5·72 \pm 0·33$ to $8·94 \pm 0·47$. The seeds borne by hybrid plants are larger, more numerous, and contain a smaller percentage of protein. Probably the factors involved are numerous, but the protein content appears to be mainly determined by that of the mother plant. By self-fertilisation of selected maize the percentage of protein can be rapidly increased, but apparently at the expense of total yield.

Morphology and Anatomy.—Experiments carried out by A. W. Hill upon *Cyclamen* (*Ann. Bot.*) embryos seem to place beyond doubt the cotyledonary nature of the curved rudiment which, when the cotyledon proper is removed, develops a normal lamina. Boodle, who has studied the mode of origin of adventitious leaves from the tubers of *Cyclamen*, finds three types. Some are strictly exogenous, or if periderm has been formed they arise just below the cork. In cut tubers the adventitious leaves may even arise from deep-seated cortical cells.

The effect of environmental conditions on the sexuality of *Cannabis* has been studied by Schaffner (*Bot. Gaz.*, Mar.), who finds that both carpellate and pistillate plants show reversal in their growing period to the opposite sex, and that their potentialities in either direction are largely influenced by the environmental conditions.

Harris, in collaboration with other investigators (*Amer. Jour. Bot.*, Feb.), has carried out an extensive study of dimerous and trimerous seedlings of *Phaseolus vulgaris*, in which the anatomical variation was studied on statistical

lines. The features studied were the number, of root poles, of primary double bundles, of intercalary bundles, of bundles in the mid-region of the epicotyl and hypocotyl. In dimerous seedlings the root is normally tetrarch, and in trimerous hexarch, whilst the epicotyl typically contains twelve bundles in the dimerous seedlings, and fourteen to eighteen in the trimerous ones. This variation in the latter case is due to failure of one or more of the nine primary epicotylar bundles (six in normal seedlings) to divide. Additional intercalary bundles (0 to 6) may appear, especially in the normal type, as a consequence of division of the hypocotyl strands. The coefficient of variation for the intercalary strands was 182·7 for the normal, and 274·92 for the trimerous seedlings.

In the hypocotyl the variability of bundle number is much higher in the dimerous seedlings than in the trimerous, but the reverse is true for the epicotyl. In the latter the number for dimerous seedlings ranged from ten to sixteen, and for the trimerous from twelve to twenty-two. Statistical constants are furnished for all the data which serve to emphasise the necessity for such methods before drawing any theoretical conclusions.

Economic.—M. Forbin in *La Nature* describes the new rubber industry of Mexico, the source of which is the guayule (*Parthenium argentatum*): this plant covers an exploitable desert area of about 70,000 sq. kilometres, at altitudes of from 1,000 to 2,000 metres. Its demands on the soil appear to be very slight, provided there is a certain proportion of calcium present. The rubber is obtained by maceration in water, and the yield is about 12 per cent. by weight and of excellent quality. A number of factories are now engaged in its exploitation, and the plant may well prove a valuable asset in other desert regions.

Taxonomy.—Col. Godfery describes two new orchid hybrids, in the *Journal of Botany* for March. One from Italy, between *Serapias Lingua* and *Anacamptis pyramidalis*, the second from France, between *Ophrys arachnitiformis* and *O. scolopax*. In the same Journal for April Col. Godfery describes *Epipactis Muelleri*, sp. nov. A new variety of *Stachys sylvatica* is described by Mr. Cutting, and in the same number Miss Lister describes and figures three new species of *Mycetozoa*, one of which is placed in a new genus, *Minakatella*, found in Japan. The other two species are British, *Physarum ovisporum* and *Arcyria carnea.*

Ecology.—Our knowledge of the water relations of the soil in relation to plant growth is comparatively meagre, despite the large amount of experimental work on wilting coefficients, etc. Recently Bouyoucos (*Soil Science Jour.*) has investigated the nature of the water in soils by means of the dilato-

meter with interesting results. It appears that the water present in the soil is not all frozen even when the temperature is lowered to $-78°$ C. It is assumed that this is combined water or water of hydration, and therefore is to be regarded as unavailable to plants. This combined water may amount to as much as 22 per cent. in clays, or as little as 0·9 per cent. in sands. Of the water which can be frozen, part freezes at 0° C. and part only when the temperature is lowered to $-1{\cdot}5$ C. The author therefore classifies the soil water into free water, capillary-adsorbed water, and combined water. The wilting point appears to correspond to the condition where all the free water and perhaps, some of the capillary adsorbed water has been utilised.

The same writer in a further paper (Feb.) reports results of repeated freezing and thawing, which not only results in more water being ultimately frozen, but also has the effect of raising the temperature at which this occurs. If the conclusion, that the additional water thus frozen is the capillary-adsorbed water, is justified, then, in view of the higher freezing-point, we must assume that, contrary to the current conception, the water film in immediate contact with the soil particles is a weaker solution than that constituted by the free water.

An extensive paper dealing with the distribution of the South African flora is contributed by Prof. Bews to the *Annals of Botany*. He divides the vegetation into seven categories, showing increasing favourability for plant life. (1) The driest is represented by the Western Region, with small and irregular summer rainfall, and mostly sandy or stony soil; Xerophytes and *Aristida* veld are important features; (2) Central Karroo, with 3–14 in. of rain increasing from east to west, and more regular than in (1); the soil is hard clay, rich in salts, and bears dwarf shrubs and succulents; (3) S.W. region of winter rains (20–40 in.) and dry hot summers, soils varied, chiefly bearing sclerophyllous shrubs; (4) sand veld of Kalahari, with 10–20 in. of rain, soil mostly sandy, bearing grass or tree veld; (5) E. thorn veld and succulent shrub; summer rainfall 20–30 in., with clayey soils rich in salts and bearing vegetation similar to (2); (6) high veld and Montane areas of E. side with a summer rainfall of 30–50 in., and mist clouds, loose sandy or loamy soils, poor in salts, bearing grass, scrub, and forest; (7) Eastern Coast belt, with a summer rainfall of from 30–40 in., various soils bearing grass, scrub, and sub-tropical forest.

Despite these different types there are in South Africa a number of widespread species which have close allies, regarded by the author as derived types, which exhibit a discontinuity of distribution that suggests their multiple origin.

In South Africa the xerophytic environment is widespread, and the mesophytic one local, the latter representing later stages in the succession. It is suggested that the contact between widespread species and more mesophytic, or sometimes more xerophytic, conditions has been the stimulus for the mutation of these disconnected types. A number of species are cited in illustration.

PLANT PHYSIOLOGY. By R. C. KNIGHT, D.Sc., Imperial College of Science and Technology (Plant Physiology Committee).

The Root System.—Probably owing to the practical difficulties of experimentation, the study of the physiology of the root system had not in the past progressed at the same rate as the study of stems and leaves. Of late, however, the literature contains a relatively large number of reports of work, both observational and experimental, on the relation of roots to environment and on the individuality of the roots themselves. The methods which have been used for investigation of root problems are unfortunately few, consisting mainly of the variation of the water, oxygen, and carbon dioxide supply; supplemented by observation of morphological characters.

It has long been recognised that the observed differences in root characters have a real physiological significance, and definite enunciation of this view is found in a descriptive paper by Markle (*Bot. Gaz.*, 1917, **64**, 177–205) on the root systems of desert plants. He remarks on the constant presence, even in deep-rooted forms, of surface roots, and even distinguishes in some instances between "anchorage" and "absorptive" roots. He recognises the impossibility of determining the causes of root variation without laboratory experiments, but expresses the opinion as the result of his observations that root competition for soil moisture, especially in arid habitats, is a determining factor. Penetrability of the soil is also considered to influence the form of the root system. The practical importance of these root differences and their probable physiological importance is indicated by Hatton (*Journ. Roy. Hort. Soc.*, 1917, **42**, 361–399) in his description of investigations of the paradise apple stocks. He devotes considerable attention to the nature of the root systems, and finds that the botanically distinct types of stock in some instances possess equally distinct types of root system, which are constant even from the young stages of the plants. There are, however, indications that variability of season may influence to some extent the nature of roots produced, and it is recognised that soil differences may also play a part in determining the nature of a root system. The wide range of root characters which can be exhibited by one type of plant is also emphasised by the

results described by Barker and Spinks (*Long Ashton Agric. and Hort. Res. St. Ann. Rep.*, 1917, 43–54), who worked out a general practical classification of the free apple stocks on the basis of root characteristics, which varied from an entirely fibrous system to a tap root without fibres. These authors conclude that it is not possible entirely to correlate differences of vigour with these root differences. J. E. Weaver (*Carnegie Inst. Wash. Publ.*, **286**, 1919), in a comprehensive study of the root systems of plants in their natural habitats, made excavations and examinations of roots of different families, habits, and habitats and found some striking differences. In one habitat, for example, whilst the grasses were rooted generally in the upper two feet of soil, the majority of the roots of dicotyledons were found at depths between two feet and twenty feet. This is remarkable in view of the fact that there is frequently no soil moisture available below the five-feet level in the regions concerned. Contrary to the conclusions of many observers, Weaver considers that, except for a few very stable types, most plants are able to conform their root systems to the habit of the community of which they form a part. He agrees with Markle that soil moisture is a determining factor in the distribution of roots, in spite of his observations on the depth of root penetration and the availability of water. Soil moisture is considered by Waterman (*Bot. Gaz.*, 1919, **68**, 22–53) to be less important than other factors in causing root variation, and this in a dune habitat, where the competition for water is likely to be severe. This writer finds that the response of roots to the peculiar soil conditions varies with different species. He considers that the main variations are attributable to nutrition rather than to the physical characteristics of the situation, such as penetrability of the soil. The extreme variation of the proportion of shoot to root is cited as a warning against the measurement of plant growth by estimations of root production. Other ecological facts with physiological aspect are recorded by Pulling (*Plant World*, 1918, **21**, 223–233), who has investigated the adaptability of root habit in the northern latitudes of America. He shows that a tree with a rigid, immutable, deep root system is automatically excluded from shallow soils. This factor in conjunction with increasing shallowness of thawed soil, probably determines the northern limit of Canadian forest trees, irrespective of climatic environment. All types of root system were found to exist, from tap-root systems to fibrous superficial systems, with varying capacity for adaptation.

Coupin (*Compt. Rend.*, 1919, **168**, 1005–1008) contributes a paper on root absorption. He finds that the root tip is the most important region for absorptive purposes, and that no advan-

tage is gained by the immersion of the whole root in water. The root hairs are relegated to a position of secondary importance. In view of this root-tip activity it is perhaps not so surprising to discover that Knudson (*Amer. J. Botany*, 1919, **6**, 309–310) has been able to demonstrate that root caps sloughed off by plants in water culture may retain their vitality, in so far that their cells are plasmolysable, for at least seventy-one days.

Experimental work on root physiology has been confined almost exclusively to the treatment of root systems, either in soil or in water culture, with gas mixtures containing different concentrations of the atmospheric gases, especially oxygen and carbon dioxide. In experiments on the aeration of soils, it should be borne in mind that the chief difficulty lies in separating the two factors, moisture content and air content. The air content of a soil naturally varies inversely as its moisture content, and under field conditions it is almost impossible to differentiate between effects due to more or less aeration and those following upon a less or greater water content. Another consideration which has been insufficiently emphasised in aeration work is the distinction between lack of oxygen and excess of carbon dioxide in soil atmosphere. Cannon and Free (*Science*, n.s., 1917, **45**, 178–180) and Cannon (*Carnegie Inst. Wash. Year Book*, 1918, **17**, 80–81) have attacked the problem from both points of view, but it is more usual to find that investigators consider the question as one merely of "aeration," no further analysis being attempted. Cannon and Free in laboratory experiments found a progressive retardation of root growth with increasing carbon-dioxide content of the air surrounding the roots. The concentration of CO_2 necessary completely to inhibit growth was not the same in all species, roots of a species of *Opuntia* being unable to grow in 50–75 per cent. CO_2, whilst those of a *Prosopis* were not completely stopped by these concentrations. In general artificial aeration was found to favour root growth in soil. Cannon also approached the question from the viewpoint of oxygen concentration. Roots of the same two species were subjected to the influence of artificial atmospheres, wherein oxygen was replaced to a varying extent by nitrogen. The results were supplementary to those of Cannon and Free, *Prosopis* being less sensitive to low oxygen content than *Opuntia*. The ecological significance of this specific reaction is obvious, and adequately explains the absence of *Opuntia* from fine soils which are badly aerated. The experiments also demonstrate the necessity for analysis of the results of aeration trials.

Cannon (*Carnegie Inst. Wash. Year Book*, 1918, **17**, 83–85) has succeeded by experimental means in inducing shallow-

rooting *Opuntias* to develop long tap roots. The treatment consisted in providing adequate aeration and warmth throughout the length of the root, and Cannon is therefore inclined to regard root development as a process depending on environment rather than as a distinctive characteristic of the type.

Experiments on aeration of soil- and water-cultures have been carried out by Livingston and Free and by Free (*Johns Hopkins Univ. Circ.*, 1917, Contributions to Plant Physiology, pp. 182 and 198), and have shown that the deleterious effect of lack of aeration is generally not wholly due to the poisonous effect of CO_2, but rather to lack of oxygen. Buckwheat, on the other hand, does not benefit from aeration in water culture, nor does it suffer if nitrogen is bubbled through its nutrient solution. Carbon dioxide, however, quickly causes wilting and death. This work again indicates that the response of a plant to lack of aeration may be due either to the resulting increased CO_2 concentration or to the lack of oxygen. Noyes and his co-workers have followed up their preliminary experiments (*Science*, n.s., 1914, **40**, 792) which showed the response of roots of tomatoes and maize to the influence of CO_2, by a more detailed investigation. Noyes, Frost, and Yoder (*Bot. Gaz.*, 1918, **66**, 364–373) have described some pot experiments in which the roots of plants were exposed to a stream of carbon dioxide through the soil. The results showed the importance of soil aeration to plant growth, whether the immediate result is the maintenance of the supply of oxygen or the removal of carbon dioxide. In all cases, the inhibiting effect of CO_2 was more apparent in the development of roots than in the growth of the aerial parts, and in addition the extent to which growth was affected was different in different plants; *Phaseolus vulgaris*, for example, proving indifferent to treatment. The question of the real value of decaying organic matter in the soil is raised, the authors expressing the opinion, as the result of their experiments, that the carbon-dioxide content of garden soils may at times become actually harmful to plants.

Noyes and Weghorst (*Bot. Gaz.*, 1920, **69**, 332–336) have described the results of growing plants in the soils used in the experiments of Noyes, Frost, and Yoder. They report that nine months after the treatment of a soil with carbon dioxide, the soil still retains to some degree the capacity for inhibiting root growth. No analysis of this result has been attempted, but the authors point out its importance from the standpoint of CO_2 production from soil organic matter.

Howard has devoted considerable attention to the problem of soil aeration in India, and in an article (*Agr. J. India*, 1918, **13**, 416–429) on the general aspect of the question has reported some experiments on the opening of a calcareous silt for

aeration by means of potsherds and sand. The results showed an increased yield up to 40 per cent., and the writer points out the importance of aeration in irrigation lands where the caking of soil is so prevalent. Experiments on tree seedlings by Hole (*Agr. J. India*, 1918, **13**, 430–440) showed that in water culture the water alone is not harmful, and that even in sand culture no ill effects result from root submergence if the water used is aerated. In water culture 500 mg. of CO_2 per litre is sufficient to kill roots. Howard also considers soil aeration to be an important factor in disease resistance (*Ann. App. Biol.*, 1921, **7**, 373–389), and is of the opinion that the destruction or asphyxiation of the deeper roots by bad aeration, *e.g.* during the wet season, predisposes the plant to disease as the result of the reduction of root activity. The wilt of Java indigo, rust of wheat and linseed, and red rot of sugar cane are mentioned in this connection.

Stiles and Jorgensen (*New Phytologist*, 1917, **16**, 181–197) found barley and balsam sensitive to aeration in water culture, and their results with buckwheat confirm the statement of Free (above) that aeration in water culture has no difference on the growth rate.

Bergman, in a laboratory study of the ecological significance of soil aeration (*Ann. Bot.* 1920, **34**, 13–33), has recorded some important results. Pot cultures were set up, using a variety of plants from both dry land and swamp habitats. In the case of land plants, the submergence of the roots in water invariably resulted in wilting, death of leaves, and decrease of growth, irrespective of whether the plants were growing in soil, peat, or sphagnum. Examination of the root system showed that the roots, except those near the surface, had died, and any roots developed after submergence were found to have originated near the surface, and were markedly less extensive than in the controls. Day aeration by means of algæ was found to have a palliative effect on plants with roots submerged. Transpiration measurements showed, as was to be expected, that submergence was followed by a decreased transpiration rate, following upon the inability of the roots to continue their absorption functions. On the other hand, plants from a swamp habitat were found to suffer when grown in moist soil, root submergence being apparently necessary for the proper development and functioning of the root system. The difference between the behaviour of these two types of plants is probably to be found rather in morphological than in physiological peculiarities. There is no evidence that hydrophytes generally have a smaller oxygen consumption than mesophytes, but, on the contrary, it appears that the hydrophyte by virtue of the extent of its internal aerating system is able to effect the

distribution of the oxygen necessary to its various organs. In this connection the present writer has some unpublished evidence to show that the aerating system of mesophytes is actually adjusted to some extent in response to different degrees of soil aeration, which is confirmatory of the results of Norris, quoted by Hunter (*Ann. Bot.*, 1915, **64**, 631).

An article quoted from the *Planter's Chronicle* (by the *Agr. J. Ind.*, 1918, **13**, 148–151) enlarges on the importance of aeration by cultivation and drainage, and emphasises the difference in the requirements of different plants. Reference is made to the attractive if impractical possibility of soil aeration by means of liquid air, and to the method of pumping air into the soil by means of a pipe system.

ZOOLOGY. By Prof. CHAS. H. O'DONOGHUE, D.Sc., F.Z.S., University of Manitoba.

Protozoa.—Hayashi has written on the "Etiology of Tsutsugamushi Disease" (*Jour. Parasit.*, vol. vii, No. 2, Dec. 1920), and his paper should prove of use, since it contains, *inter alia*, a summary of a considerable amount of work that he has published previously in Japanese journals. The disease, rain fever, occurs along the river banks in Northern Japan, and resembles Rocky Mountain spotted fever in many respects. The parasite causing the disease has been named by the author *Theileria tsutsugamushi*, and appears to be most closely related to *T. parva.* "Rod," "spheroid," and "ring-shaped" bodies are found in the lymphocytes of the lymph nodes of patients suffering from the complaint. They also occur in the blood-plasma and in severe infections in the erythrocytes. The disease has been transmitted to monkeys, guinea-pigs, rabbits, and calves experimentally. A diagram of the life-history of the parasite is given.

Other papers include:

Andrews, "Alternation of Phases in *Folliculina*" (*Biol. Bull.*, vol. xxxix, No. 1, July 1920); Fantham and Porter, "On the Natural Occurrence of Herpetomonads (Leptomonads) in the Blood of a Fish" (*Jour. Parasit.*, vol. vii, No. 1, Sept. 1920); Hausman, "The Manipulation and Identifications of the Free-swimming Mastigophora of Fresh Waters" (*Amer. Nat.*, vol. liv, No. 633, Aug. 1920), and "The Vibratile Oral Membranes of *Glaucoma scintillans*, Ehr." (*ibid.*, No. 634, Oct. 1920); and Kudo, "Notes on *Nosema apis*, Zander" (*Jour. Parasit.*, vol. vii, No. 2, Dec. 1920).

Invertebrata.—Stephenson has published a paper "On the Classification of Actiniaria, Part I, Forms with Acontia and Forms with a Mesogleal Sphincter" (*Quart. Jour. Micro. Sci.*, vol. lxiv, pt. 4, July 1920). It has long been recognised that the classification of this group of Cœlenterates was in an unsatisfactory condition, and the author had this forcibly brought

to his notice in working out the species in two collections. He therefore set to work to investigate the various anatomical characters of a large number of such forms, and the present paper is a full statement of his conclusions on the subject. His classification necessitates a considerable revision and rearrangement of the families, and he has furnished definitions of new and revised families, and also their contained genera. This comprises the last part of the paper, but in order to make this portion easy to follow, he has prefaced it by a full account of the anatomical characters upon which it is based, and a discussion of a number of other relevant matters. It also includes contributions to the anatomy of various species.

Other papers include:

Göthlin, "Experimental Studies on Primary Inhibition of the Ciliary Movement in *Beroe cucumis*" (*Jour. Exp. Zool.*, vol. xxxi, No. 4, Nov. 1920); Hickson, "On the Occurrence of *Protohydra* in England" (*Quart. Jour. Micro. Sci.*, vol. lxiv, pt. 4, July 1920); and Parker, "Activities of Colonial Animals: I. Circulation of Water in *Renilla*" (*Jour. Exp. Zool.*, vol. xxxi, No. 3, Oct. 1920), and "Activities of Colonial Animals: II. Neuromuscular Movements and Phosphoresence in *Renilla*" (*ibid.*, No. 4, Nov. 1920).

Coe, in "Sexual Dimorphism in Nemerteans" (*Biol. Bull.*, vol. xxxix, No. 1, July 1920), and again in conjunction with Ball in "The Pelagic Nemertean *Nectonemertes*" (*Jour. Morph.*, vol. xxxiv, No. 3, Dec. 1920), has called attention to the fact that some of these worms exhibit true sexual dimorphism, extending to the number and position of the gonads and also to the external form. This is perhaps most strongly marked in *Nectonemertes*, where the two forms were formerly considered as belonging to two separate genera.

Other papers include:

Boyd, "A Possible Intermediate Host of *Fascicola hepatica* L. 1758 in North America" (*Jour. Parasit.*, vol. vii, No. 1, Sept. 1920); Chandler, "A New Record of *Tænia confusa*, with additional Notes on its Morphology" (*ibid.*); Cort and Nichols, "A New Cystophorus Cercaria from California" (*ibid.*); Kamm, "The Development of Gregarines and their Relation to the Host Tissue" (*ibid.*); Leon, "*Dibothriocephalus tæniodes*, Leon, a new Case in Roumania" (*ibid.*); Taliaferro, "Reactions to Light in *Planaria maculata*, with especial reference to the Function and Structure of the Eyes" (*Jour. Exp. Zool.*, vol. xxxi, No. 1, July 1920); Van Cleave, "Acanthocephala Parasitic in the Dog" (*Jour. Parasit.*, vol. vii, No. 2, Dec. 1920); and Yokogawa, "On the Migratory Course of *Trichosomoides crassicauda* (Bellingham) in the Body of the Final Host" (*Jour. Parasit.*, vol. vii, No. 2, 1920), and "A New Nematode from the Rat" (*ibid.*, No. 1, Sept. 1920). Papers on Mollusca include: Crozier, "Notes on some Problems of Adaptation: I. On the Reformation of the Mantle-glands of *Chromodoris*" (*Biol. Bull.*, vol. xxxix, No. 2, Aug. 1920); Kjerskog-Agersborg, "The Utilisation of Echinoderms and of Gasteropod Mollusks" (*Amer. Nat.*, vol. liv, No. 634, Oct. 1920); and Lange, "On the Regeneration and Finer Structure of the Arms of the Cephalopods" (*Jour. Exp. Zool.*, vol. xxxi, No. 1, July 1920).

Papers on Echinoderms include: Crozier, "Notes on some Problems of Adaptation: II. On the Temporal Relations of Asexual Propagation and Gametic Reproduction in *Coscinasterias tenuispina*: with a Note on the Direction of Progression and on the Significance of the Madrepores; and III. The Volume of Water involved in the Cloacal Pumping of Holothurians (Stichopus)" (*Biol. Bull.*, vol. xxxix, No. 2, Aug. 1920); and "The Bionomics of *Melita*" (*Amer. Nat.*, vol. liv, No. 634, Sept. 1920).

The "Phylogeny of the Arthropods, with especial reference to the Trilobites" (*Amer. Nat.*, vol. liv, No. 634, Oct. 1920), by Raymond, is a review of the relationships of the groups of Arthropods when viewed in the light of our knowledge of the detailed structure of the Trilobites. These animals are claimed as primitive and ancestral, and further, it is suggested that the chief modifications in other groups are in the nature of reductions—the loss of whole appendages, of branches, or of segments. It is claimed that the geological record also favours the theory that the other Arthropods were derived from Trilobites. If this be the case, then the search for the ancestral Arthropod must be conducted among the forerunners of the Trilobites. These were floating and swimming animals, and the adoption of a crawling mode of life is to be regarded as a later specialisation. The Copepods are held to be fairly closely related to the Trilobites.

Other papers include:

Dodds, "Entomostraca and Life Zones: A Study of Distribution in the Colorado Rockies" (*Biol. Bull.*, vol. xxxix, No. 1, July 1920); Komai, "Spermatogenesis of *Squilla oratoria* De Hann" (*Jour. Morph.*, vol. xxxiv, No. 2, Sept. 1920); and Stebbing, "The Malacostracs of Durban Bay" (*Ann. Durban Mus.*, vol. ii, pt. 6, Aug. 1920). . . . Adolph, "Egg-laying Reactions in the Pomace Fly, *Drosophila*" (*Jour. Exp. Zool.*, vol. xxxi, No. 3, Oct. 1920); Herms and Freeborn, "The Egg-laying Habits of Californian Anopheles" (*Jour. Parasit.*, vol. vii, No. 2, Dec. 1920); Muller, "Further Changes in the White-eye Series of *Drosophila* and their Bearing on the Manner of Occurrence of Mutation" (*Jour. Exp. Zool.*, vol. xxxi, No. 4, Nov. 1920); Nonidez, "The Internal Phenomena of Reproduction in *Drosophila*" (*Biol. Bull.*, vol. xxxix, No. 4, Oct. 1920); Schrader, "Sex Determination in the White-fly (*Trial eurodes vaporariorum*)" (*Jour. Morph.*, vol. xxxiv, No. 2, Sept. 1920); and Walker, "*Wohlfahrtia vigil* (Walker) as a Human Parasite (Diptera-Sarcophagidæ)" (*Jour. Parasit.*, vol. vii, No. 1, Sept. 1920). . . . Barker, "Further Data and some Corrections on the *Brevicollis* Group of the *Cicindelæ*" (*Ann. Durban Mus.*, vol. ii, pt. 6, Aug. 1920); and Weiss, "The Insect Enemies of Polyporoid Fungi" (*Amer. Nat.*, vol. liv, No. 634, Oct. 1920). . . . Bowen, "Studies on Insect Spermatogenesis: I. The History of the Cytoplasmic Components of the Sperm in Hemiptera" (*Biol. Bull.*, vol. xxxix, No. 6, Dec. 1920).

While much has been written concerning the nuptial flight and the act of copulation of the bee, it has been little studied from the physiological point of view, and this is the standpoint adopted by Bishop in "Fertilisation in the Honey-bee: I. The Male Sexual Organs; their Histological Structure and their

Functioning; and II. Disposal of the Sexual Fluids in the Organs of the Female" (*Jour. Exper. Zool.*, vol. xxxi, No. 2, Aug. 1920). The inability of young drones to fertilise is due to the immature stage of the reproductive organs for the first nine days. Sperms are stored in the sperm reservoir and mucus in the mucus gland reservoir until ejaculated, and then they do not mix to any appreciable extent. The sperms are passed out first to fill the paired oviducts, and then the mucus follows and fills up the vagina, where it hardens to form a plug. Most of the sperms enter the spermatheca, directed apparently by chemotactic stimuli, within six hours of mating, while some of the mucus may remain for as long as eighteen hours.

Other papers include:

Cockerell, "On South African Bees, chiefly collected in Natal," two parts (*Ann. Durban Mus.*, vol. ii, pts. 5 and 6, March and Aug. 1920); Jordan, "Studies on Striped Muscle Structure: VII. The Development of the Sarcostyle of the Wing Muscle of the Wasp, with a Consideration of the Physicochemical Basis of Contraction" (*Anat. Rec.*, vol. xix, No. 2, July 1920); and Thompson and Snyder, "The 'Third Form,' Wingless Reproductive Type of Termites" (*Jour. Morph.*, vol. xxxiv, No. 3, Dec. 1920).

Chordata.—"The Branches of the Branchial Nerves of Fishes, with special reference to *Polyodon spathula*," are dealt with by Allis (*Jour. Comp. Neur.*, vol. xxxii, No. 2, Oct. 1920). Each branchial nerve in *Polyodon* and *Polypterus* possesses a ramus pharyngeus, rami prætrematici externus and internus, rami posttrematici externus and internus, and a ramus dorsalis supratemporalis. The same probably holds good for fish in general. The pits of *Polyodon* are phylogenetically related to the ampullæ of the Selachii, but they do not respond to the stimulus of pressure as those in the Selachii are said to do; they appear to be organs of taste or touch, probably the former.

Other papers include:

Gilchrist, "Ecdysis in a Teleostean Fish *Agriopus*" (*Quart. Jour. Micro. Sci.*, vol. lxiv, pt. 4, July 1920); Leigh-Sharpe, "The Comparative Morphology of the Secondary Sexual Characters of Elasmobranch Fishes: the Claspers, Clasper Siphons, and Siphon Glands," Memoir I (*Jour. Morph.*, vol. xxxiv, No. 2, Sept. 1920); Moodie, "Microscopic Examination of a Fossil Fish Brain" (*Jour. Comp. Neur.*, vol. xxxii, No. 3, Dec. 1920); Olmsted, "The Results of Cutting the Seventh Cranial Nerve in *Amiurus nebulosus* (Lesueur)" (*Jour. Exp. Zool.*, vol. xxxi, No. 4, Nov. 1920); and Regan, "A Revision of the Flat-fishes (*Heterosomata*) of Natal" (*Ann. Durban Mus.*, vol. ii, pt. 5, March 1920). . . . Dawson, "The Integument of *Necturus maculosus*" (*Jour. Morph.*, vol. xxxiv, No. 3, Dec. 1920); Detwiler, "Experiments on the Transplantation of Limbs in Amblystoma: The Formation of Nerve Plexuses and the Function of the Limbs" (*Jour. Exp. Zool.*, vol. xxxi, No. 1, July 1920); and Kampmeier, "The Changes of the Systemic Venous Plan during Development and the Relation of the Lymph Hearts to them in Anura" (*Anat. Rec.*, vol. xix, No. 2, July 1920).

The Tuatara is admittedly a primitive form and holds a unique position among living reptiles. While certain points in both the arterial and venous systems have been dealt with, no account of the whole vascular arrangement had been given until the publication of "The Blood-Vascular System of the Tuatara, *Sphenodon punctatus*," by O'Donoghue (*Phil. Trans. Roy. Soc. B.*, 1920). It is pointed out that the blood-vessels and the heart are distinctly reptilian in character, and on the whole most closely related to those of the Lacertilia. They also show certain resemblances to some of the other groups in points that most probably are to be regarded as primitive, and which have been lost by the Lacertilia. In addition to this, the whole system is more primitive than that of any Lacertilian so far described, and approaches the conditions in the Urodele Amphibia in a number of interesting features. The heart probably possesses the remains of a conus arteriosus. Both a ductus caroticus and a ductus arteriosus are present in a moderately developed condition on both sides. The origin and distribution of the carotids are primitive. The development of the tracheal veins, the situation of the vena cerebralis posterior, the vena azygos, and the supra-renal veins are all in a more primitive condition than in Lacertilia. In addition to the rarity and singular position of *Sphenodon*, no such general account is available in the case of any Lacertilian.

Other papers include :

Black, "The Motor Nuclei of the Cerebral Nerves in Phylogeny—a Study of the Phenomena of Neurobiotaxis" (*Jour. Comp. Neur.*, vol. xxxii, No. 1, Aug. 1920) ; Detwiler and Laurens, "Studies on the Retina: The Structure of the Retina of *Phrynosoma Cornutum*" (*ibid.*, Dec. 1920) ; Gilmore, "Reptile Reconstructions in the United States National Museum" (*Smithson Rep.*, No. 2561, 1920) ; Haughton, "On the Genus *Ictidopsis*" (*Ann. Durban Mus.*, vol. ii, pt. 5, March 1920) ; Latimer, "The Weights of the Viscera of the Turtle" (*Anat. Rec.*, vol. xix, No. 6, Nov. 1920) ; and Ogawa, "Contributions to the Histology of the Respiratory Spaces of the Vertebrate Lungs" (*Amer. Jour. Anat.*, vol. xxvii, No. 3, July 1920).

The question of the inheritance of hen-feathering in the cocks of certain races of domestic fowls has been investigated by Morgan in "The Effects of Ligating the Testes of Hen-feathered Cocks," "The Effects of Castration of Hen-feathered Campines," and "The Genetic Factor for Hen-feathering in the Sebright Bantam" (all in *Biol. Bull.*, vol. xxxix, No. 4, Oct. 1920). In both cases the castration or ligaturing of the testes so that they are subsequently absorbed leads to the same result : the hen-feathered cocks develop a cock-feathering. Crossing with a cock-feathered race shows that the hen-feathering in the Sebright bantam is nearly a complete dominant in the F_1 generation, and the characters are strictly

alternate. It is probable that only one pair of factor differences is involved.

Other papers include:

Duerden, "Inheritance of Callosities in the Ostrich" (*Amer. Nat.*, vol. liv, No. 633, Aug. 1920); Kuntz, "Experimental Observations on the Histogenesis of the Sympathetic Trunks in the Chick" (*Jour. Comp. Neur.*, vol. xxxii, No. 3, Dec. 1920); and Nonidez, "Studies on the Gonads of the Fowl: I. Hematopoietic Processes in the Gonads of Embryos and Mature Birds" (*Amer. Jour. Anat.*, vol. xxviii, No. 1, Nov. 1920).

Hausman has fairly recently published a series of papers on the hair of mammals, of which two fall in the period under review, viz.: "A Micrological Investigation of the Hair Structure in the Monotremata" (*Amer. Jour. Anat.*, vol. xxvii, No. 4, Sept. 1920), and "Structural Characteristics of the Hair of Mammals" (*Amer. Nat.*, vol. liv, No. 635, Dec. 1920). In the Monotremes several different types of hair are to be found upon the body. The characteristic type is flattened, and of the variety encountered in the shield hair of *Ornithorhynchus*. This animal possesses also a fur hair, of the same general type as in other mammals, and this is lacking in *Tachyglossus*. Mammal hairs are either circular or oval in section; the former is usually straight, and the latter kinky or curly. Each mammal is characterised by its own special type of hair. The second paper provides illustrations of a number of the common types, and also a discussion of the methods of investigation. The discussion of the much-vexed question of the islets of Langerhans has been taken up by Saguchi in "Cytological Studies of Langerhans's Islets, with special reference to the Problem of their Relation to the Pancreatic Acinus Tissue" (*Amer. Jour. Anat.*, vol. xxviii, No. 1, Nov. 1920). As a result of studying the specific granules they contain, the mitochondria, lipoid granules, urano-argentophile apparatus, argentophile granules, etc., the author concludes that they are to be regarded as modified acinar tissue, and he distinguishes five types of cell among them which he designates *a*, *b*, *c*, *d*, and *e*, some of which are intermediate between true islet and acinar cells. "The development of the veins in the domestic cat (*Felis domestica*), with especial reference (1) to the share taken by the supra-cardinal veins in the development of the postcava and azygos veins, and (2) to the interpretation of the variant conditions of the postcava and its tributaries as found in the adult" has been worked out by Huntington and McClure (*Anat. Rec.*, vol. xx, No. 1, Dec. 1920). The title explains the contents of the paper, which is fully illustrated by coloured plates of wax reconstructions, and a composite diagram gives the clue to the

interpretation of the seventeen types into which the variants of the postcava found in the cat and man can be classified.

Other papers include:

Arai, "On the Cause of the Hypertrophy of the Surviving Ovary after Semispaying (Albino Rat) and on the Number of Ova in it" (*Amer. Jour. Anat.*, vol. xxviii, No. 1, Nov. 1920); Allen, "Bison Remains from New England" (*Jour. Mamm.*, vol. i, No. 4, Aug. 1920), and "An Insular Race of Cotton Rat from the Florida Keys" (*ibid.*, No. 5, Nov. 1920); Baldwin, "Notes on the Branches of the Aorta (arcus aortæ) and the Subclavian Artery of the Rabbit" (*Anat. Rec.*, vol. xix, No. 3, Aug. 1920); Bayon, "Racial and Sexual Differences in the Appendix Vermiformis" *ibid.*, No. 4, Sept. 1920); Begg, "Absence of the Vena Cava Inferion in a 12-mm. Pig Embryo, associated with the Drainage of the Portal System into the Cardinal System" (*Amer. Jour. Anat.*, vol. xxvii, No. 4, Sept. 1920); Black, "Studies on Endocranial Anatomy: II. On the Endocranial Anatomy of *Oreodon* (*Merycoidodon*)" (*Jour. Comp. Neur.*, vol. xxxii, No. 3, Dec. 1920); Dewey, "A Contribution to the Study of the Lymphatic System of the Eye" (*Anat. Rec.*, vol. xix, No. 2, July 1920); Downs and Eddy, "Effect of Subcutaneous Injections of Thymus Substances in Young Rabbits" (*Endocr.*, vol. iv, No. 3, July 1920); Dunn, "Types of White Spotting in Mice" (*Amer. Nat.*, vol. liv, No. 635, Dec. 1920); Carey, "Studies in the Dynamics of Histogenesis: Growth Motive Force as a Dynamic Stimulus to the Genesis of Muscular and Skeletal Tissues" (*Anat. Rec.*, vol. xix, No. 4, Sept. 1920); Ellis, "Norms for some Structural Changes in the Human Cerebellum from Birth to Old Age" (*Jour. Comp. Neur.*, vol. xxxii, No. 1, Aug. 1920); Gidley, "A Pleistocene Cave Deposit of Western Maryland" (*Smithson Rep.*, No. 2563, 1920); Grinnell, "A New Kangaroo Rat from the San Joaquin Valley, California" (*Jour. Mamm.*, vol. i, No. 4, Aug. 1920); Guyer and Smith, "Studies on Cytolysins: II. Transmission of Induced Eye Defects" (*Jour. Exp. Zool.*, vol. xxxi, No. 2, Aug. 1920); Hanson, "The History of the Earliest Stage in the Human Clavicle," and "The Problem of the Coracoid" (*Anat. Rec.*, vol. xix, No. 6, Nov. 1920); Hartman, "Studies in the Development of the Opossum (*Didelphys virginiana*, L.): 'The Phenomenon of Parturition'" (*ibid.*, No. 5, 1920); Howell, "Description of a New Species of Beach Mouse from Florida" (*Jour. Mamm.*, vol. i, No. 5, Nov. 1920); Jewett, "Notes on Two Species of *Phenacomys* in Oregon" (*ibid.*, No. 4, Aug. 1920); Jordan, "Further Studies on Red Bone-marrow: I. Experimental; II. Cytologic, with Special Reference to the Data suggesting Intra-cellular Hemocytogenic Activity on the part of the Giant Cells and to the Significance of the so-called Mitotic Figures in these Cells" (*Amer. Jour. Anat.*, vol. xxvii, No. 3, July 1920); Kingsbury, "The Extent of the Floor-plate of His and its Significance" (*Jour. Comp. Neur.*, vol. xxxii, No. 1, Aug. 1920); Kuntz, "The Development of the Sympathetic Nervous System in Man" (*ibid.*, No. 2, Oct. 1920); Lima, "Anatomy of a Fetus of a Cyclopean Goat" (*Anat. Rec.*, vol. xix, No. 2, July 1920); Little, "Factors Influencing the Growth of a Transplantable Tumour in Mice" (*Jour. Exp. Zool.*, vol. xxxi, No. 3, Oct. 1920); Matthew, "A New Genus of Rodents from the Middle Eocene" (*Jour. Mamm.*, vol. i, No. 4, Aug. 1920); Nittono, "On the Growth of the Neurons composing the Gasserian Ganglion of the Albino Rat—between Birth and Maturity" (*Jour. Comp. Neur.*, vol. xxxii, No. 2, Oct. 1920); Slonaker, "Some Morphological Changes for Adaptation in the Mole" (*Jour. Morph.*, vol. xxxiv, No. 2, Sept. 1920); Stewart, "On the Origin of the Ganglion Cells of the Nervus Terminalis of the Albino Rat" (*Jour. Comp. Neur.*, vol. xxxii, No. 1, Aug. 1920); Suitsu, "Comparative

Studies on the Growth of the Corpus Callosum: I. On the Area of the Corpus Callosum, measured on the Sagittal Section of the Albino Rat Brain" (*ibid.*); Turner, "A Wax Model of a Presomite Human Embryo" (*Anat. Rec.*, vol. xix, No. 6, Nov. 1920); Taylor, "A New Meadow Mouse from the Cascade Mountains of Washington" (*Jour. Mamm.*, vol. i, No. 4, Aug. 1920); Vaughan-Kirby, "The White Rhinoceros with Special Reference to its Habits in Zululand" (*Ann. Durban Mus.*, vol. ii, No. 5, March 1920); Vincent and Hollenberg, "The Effects of Inanition upon the Adrenal Bodies" (*Endocr.*, vol. iv, No. 3, Sept. 1920); and Wieman, "Observations in Connection with the Early Development of the Human Suprarenal Gland" (*Anat. Rec.*, vol. xix, No. 5, Oct. 1920).

General.—The suggestion is put forward by Swingle in "Neoteny and the Sexual Problem" (*Amer. Nat.*, vol. liv, No. 633, Aug. 1920) that the "oocytes" in Bidder's organ in the Bufonidæ are in reality simply overgrown spermatocytes which are the remnants of a phylogenetically older and more primitive reproductive period, and not to be considered as in any way related to true oocytes. This earlier period corresponds with the larval reproduction met with in certain Urodela. In this case Bidder's organ is to be regarded as the vestigeal remnant of an earlier and larval reproductive organ of the male sex. This point of view bears on the much-discussed cases of hermaphroditism in the Anura.

Other papers include:

Adams, Burns, Hankinson, Moore, and Taylor, "Plants and Animals of Mount Marcy, New York" (*Ecology*, vol. i, Nos. 2 and 3, 1920); Batson, "De-electrification of Paraffin Ribbons by means of High-frequency Current" (*Anat. Rec.*, vol. xix, No. 4, Sept. 1920); Cowdry, "Anatomy in China" (*ibid.*, vol. xx, No. 1, Dec. 1920); Hawkins, "Sexual Selection and Bird Song" (*Smithson Rep.*, No. 2568, 1920); Heilbrunn, "The Physical Effect of Anæsthetics upon Living Protoplasm" (*Biol. Bull.*, vol. xxxix, No. 6, Dec.); Kordenat, "Contamination of Cadavers by *Saccharomyces cerevisiæ*" (*Anat. Rec.*, vol. xix, No. 2, July 1920); Longly, "Marine Camoufleurs and their Camouflage: the Present and Prospective Significance of Facts regarding the Colouration of Tropical Fishes" (*Smithson Rep.*, No. 2569, 1920); Petrunkevitch, "Standardised Microphotography" (*Anat. Rec.*, vol. xix, No. 2, Oct. 1920); Petronievics, "On the Law of Irreversible Evolution" (*Smithson Rep.*, No. 2565, 1920); Thuringer, "A Suggestion for Improvement in Projection and Drawing Apparatus" (*Anat. Rec.*, vol. xix, No. 3, Aug. 1920).

ANTHROPOLOGY. By A. G. THACKER, A.R.C.S., Zoological Laboratory, Cambridge.

THE racial affinities of the representatives of *Homo sapiens*, who lived in Europe before and during the last great glacial period, is a subject of unceasing interest. It has often been remarked that these races do not in any way merge backwards into another species of the Hominidæ. They give us no direct help in the solution of the problem of the origin of true man. Various races still surviving give us more help—though they, too, give us singularly little assistance. These Pleistocene

races were much nearer to modern Caucasians than are numerous peoples living to-day. It is, however, difficult to be sure of their exact affinities. The apparent cultural indications are sometimes almost striking; but they receive little or no support from the osteological evidence, and if we were destitute of the latter, they would undoubtedly be highly misleading. In particular, the resemblance of the Magdalenian culture—which flourished, many of us believe, at the height of the last glacial period—to the culture of the modern Eskimos has often been the subject of comment. It is, however, quite certain that most of the Magdalenian races were entirely unlike the Eskimos. It has long been claimed, however, that one late Pleistocene skull, the well-known Chancelade skull, has a definite resemblance to the Eskimo type. This important question is discussed, among others, by W. E. Le Gros Clark in an article entitled "On a Series of Ancient Eskimo Skulls from Greenland," which appears in the *Journal of the Royal Anthropological Institute*, vol. 50, July to December 1920. The article consists mainly of a report on sixteen adult skulls belonging to St. Thomas's Hospital Medical School. Most of the skulls came from graves near Melville Bay in North Greenland, and the author is confident that they represent an unmixed Eskimo population. The craniological characteristics are set out in detail, but they are, of course, too extensive to quote here. It may be remarked, however, that the author demonstrates that there is a "predominance of frontal over occipital development in the Eskimo skull," and that the lower border of the zygomatic arch shows a curious straightness. At the end of his excellent article Mr. Le Gros Clark deals with the question of the Chancelade skull. He sets out the characteristics of the Chancelade man, of his limb-bones as well as of his cranium; and I think it will surprise most of his own readers that he comes to the conclusion that the inference to be drawn from the comparison of Eskimo and Chancelade characteristics "must be of a negative nature." He is evidently influenced mainly by the dissimilarity of the limb-bones. But it is difficult to read the careful comparison of the craniological characters, point by point, without feeling that the resemblances are something more than accidental. Among other points of likeness, the Chancelade cranium has the excess of frontal development and the straightness of the zygomatic arch mentioned above. The points of resemblance appear to me to be sufficient to arouse a very serious suspicion of relationship, to express it mildly. The Chancelade skull is, after all, very ancient. It antedates all civilisation by thousands of years. And although the Chancelade race evidently differed from modern Eskimos

in certain points, notably in the limb-bones, the relationship between the two types may nevertheless be close.

The same number of the *Journal* contains an interesting article on "palæolithic" implements in South Africa by Neville Jones, entitled "On the Implement-bearing Deposits of Taungs and Tiger Kloof in the Cape Province of South Africa." There is also a most important article by Mrs. Scoresby Routledge on Easter Island, under the title "Survey of the Village and Carved Rocks of Orongo, Easter Island, by the *Mana* Expedition." A magnificent series of photographs is appended to this latter article.

The reader may remember that in my last article in this series I dealt at length with Mr. Reid Moir's correlation of East Anglian Pleistocene beds with Prof. Penck's glacial and interglacial scheme, and indicated that I had not been able to understand what English stratum Mr. Moir had intended to correlate with Penck's Günz. I understand from Mr. Moir that at present he cannot arrive at a definite conclusion on this point. Owing to a slip in the proof—which was, however, obvious from the context—I stated that Mr. Moir correlated the Cromer Till with the Riss, whereas, in fact, he correlates this stratum, of course, with the Mindel.

The following articles may also be mentioned:

On Physical Anthropology. In the *American Journal of Physical Anthropology*, vol. iii, No. 3, "Age Changes in the Pubic Bones," by T. W. Todd; and "Abnormalities and Pathology of Ancient Egyptian Teeth," by Sir A. Ruffer.

And on Social Anthropology. In the *Journal of the Royal Anthropological Institute*, vol. 50, pt. 2, "Migrations of Cultures in British New Guinea," by A. C. Haddon; "The Nilotic Languages," by G. W. Murray; and "Notes on Edo Burial Customs," by N. W. Thomas. And in recent numbers of *Man*: "History and Ethnology in Central Asia," by M. A. Czaplicta (Feb.); and "On a Recent Discovery of Rock-sculptures in Derbyshire," by G. A. Garfitt (March). And in *Annals of Archæology and Anthropology*, vol. viii, No. 1, "Oxford Excavations in Nubia," by F. L. Griffiths.

MEDICINE. By R. M. Wilson, M.B., Ch.B.

The Future of Radiology.—The most important medical event of the past quarter has undoubtedly been the death of Dr. Ironside Bruce, radiologist to Charing Cross Hospital. Dr. Bruce died of aplastic pernicious anæmia, which was induced by the action of penetrating X-rays on the bone marrow. Apart from its tragic character, his end calls attention to a danger in the use of those rays which has not perhaps been sufficiently realised.

Modern X-ray work is being carried on to a great extent with tubes capable of affording a penetrating ray. This ray does not tend to burn the skin or to set up dermatitis. The skin,

as a rule, is but little affected. The ray, however, does tend to inhibit the growth of tissues, and Regaud, of the Biological Laboratory of the Radium Institute, Paris, has gone so far as to suggest that radio-sensibility is a property of the nucleus, and is inherent in certain states or temporary states of cell life, the most important being reproduction. Another period of heightened sensibility, he says, corresponds to the maximum metabolic activity of the nucleus in cells which have a secretory function. Thus, radio-sensibility waxes and wanes in the same tissue, at different periods.

This worker has, therefore, felt himself able to draw up a list of tissues having various degrees of radio-sensibility, and has extended his series to include neoplasms which may also be arranged in order of radio-sensibility.

An indication of the importance of such a conception was afforded by the well-known fact that spermatogenesis is inhibited by X-rays. In Dr. Bruce's case it would seem that the highly penetrating rays were able to inhibit the activity of the bone marrow, and so produce a failure to create the corpuscular elements of the blood. It is evident that this danger is a very serious one, and that it must be guarded against; for the tendency of X-ray therapeutics is all towards more rather than less penetrating X-rays.

The same considerations apply to the use of radium. It is known that the gamma rays of radium are highly penetrating, and several deaths have been reported from pernicious anæmia among workers exposed to radium. Here, also, more light is necessary if we are to enjoy in safety the therapeutic use of the substance. That the penetrating rays do exercise an important influence in checking the growth of cancerous tissues is beyond dispute. The present-day technique of use is, however, evidently capable of great improvement. This will scarcely be attained either for X-rays or radium until greater safety to the worker is ensured. As great a thickness as five inches of steel is said now to be insufficient to exclude certain of the more penetrating rays.

The Nature of Pain.—Medical attention is being focussed more and more on such physiological considerations as the nature and mechanism of pain. Mr. Wilfred Trotter, in *Medical Science*, recently reviewed the evidence and reached the conclusion that sensibility to pain is the survival in us of the primordial mode of sensation. He uses the expression "pain nerves," and suggests that they are constantly exposed to a mild "subliminal excitation," the difference between them and all other nerves lying in the fact that they are normally in a state of continuous excitation. When the insulation of nervous tissue, which is very complete in the human body, is

broken down this nervous tissue is "thrown back," as it were, to a more primitive type and pain supervenes.

Medical Progress and the State.—The retirement of Dr. Addison from the Ministry of Health has this bearing on medical progress, that it seems to suggest a halt in the movement towards a state system of medicine. The greatest achievements of the British school have always been individualistic, and many viewed the tendency to a socialistic conception with misgiving. They believe that it is the proper function of the Ministry to follow rather than to attempt leadership, for they hold that any attempt to influence medical thought or method from outside must prove disastrous.

EDUCATION. By A. E. Heath, M.A., University, Liverpool.

The Educational Conferences in January were overshadowed by fears for the new Education Act. Educationists realise that economy is necessary; but they mean by economy the wise control of expenditure. Refusal to spend what is necessary to develop the nation's resources in capacity is not, however, economy, but waste. It will probably need a wide extension, among ordinary citizens, of a more definitely scientific outlook on educational questions before the community recognises to its full extent the gross waste involved in leaving untilled the vast field of adolescent youth. To the psychologist nothing could be clearer than the vital necessity that none should be denied help, guidance, and training during the important years between 13 and 18. Not only is potential capacity thrown away and previous schooling made of no avail, but also the untilled field is left unprotected against weeds that threaten the very existence of any social order. It was, perhaps, Professor Adams's wise way of reminding us indirectly of these things to choose, for his inaugural lecture to the London Conference, the subject "Instinct and Education." For modern psychology has to a great extent centred round the problems raised by instinct, and in so doing has taught us to look with new and somewhat apprehensive eyes on those critical years of adolescence, as well as those of early childhood. Side by side with the more lively recognition of the dangers and difficulties of these two periods has come a new sense of their importance as factors in the making of the adult creature. The provision of adequate control and guidance is therefore essential at both periods; provided that the control is sympathetic and the guidance well informed.

The study of recent work on instinct can, moreover, give us—in addition to this insight into fundamental necessities—an admirable starting-point for an incursion into current

educational theory. There are two recent books which would form a good introduction to such a study: the welcome second edition of Dr. Drever's *Instinct in Man: A Contribution to the Psychology of Education* (Camb. Univ. Press, 1921); and Dr. Rivers's *Instinct and the Unconscious* (Camb. Univ. Press, 1920). The latter is a work of the highest importance to both psychologists and educationists. Its appearance, taken in conjunction with the work of Dr. Henry Head, marks the beginning of the healing of that breach between psychical and neurological explanations of mental facts which has been hailed with such delight by irrationalist philosophers. Its special importance for education lies in Dr. Rivers's central thesis that suppression is an instinctive mechanism which takes place at all biological levels whenever one set of tendencies in the creature hinders the efficacy of another; and that the consequent dissociation is at the other end of the scale from that integration or bringing into co-ordination of different tendencies which is the work of intelligence. A concrete example may make clear the educational bearings of the distinction here drawn. The work of Freud and his followers has been used by some educationists to justify a glorification of licence. The child must be allowed to "satisfy" his impulses because repression is dangerous. This, however, is to confuse conscious control with an instinctive mechanism. Moreover, it neglects the real truth about the impulses of creatures as high in the biological scale as man. To know the facts about repression is a great advance and throws light on problems of man's behaviour in relation to the primary instincts like sex and self-preservation. But it gives no support to the view that the whole of a child's everyday impulses are sacred. It is true that these impulses are not evil in themselves, and so to be ruthlessly exorcised; for they may well be fused into more expressive processes of wider scope. But neither are they good in themselves, until such fusion has been effected. The truth is that a child who is left free to indulge without guidance a fancy for disorderliness is, strictly speaking, being left to waste his possibilities in the formation of lower-level habits. The point has been well expressed as follows, in a comment on the anarchic device of a teacher who arranged a corner of a classroom for the use of such children as were impelled to make a disturbance: "The child who must go into a corner to throw things about, and smash them and shout, is not really satisfying his impulses. The more noise he makes and damage he does, the less he satisfies them; and he makes the noise and does the damage because he does not know how to satisfy them. It is very like the incessant use of sanguinary and other epithets; they do not really

express, but are symptoms of a failure to do so ; and, the more they are used, the less they satisfy the impulse of expression" (*Times Educ. Supplement*, April 28, 1921, p. 195). Many other examples could be given of the educational applications of Dr. Rivers's view, and of Dr. Drever's account of what he calls the change from "primary meaning" to "significance." In fact, it is not too much to say that a close study of these two books would illuminate almost every branch of educational theory.

The following is a selection of references to recent work :

The paper by Dr. Rivers which forms the title-essay of his book is also printed in the symposium on "Instinct and the Unconscious" (other papers by C. S. Myers, C. G. Jung, Graham Wallas, James Drever, and W. McDougall), *Brit. Journ. of Psy.*, 1919, **10**, 1, pp. 1–42. It is interesting to contrast the behaviourist view of instinct put forward in "A Functional Interpretation of Human Instincts," by J. R. Kantor, *Psy. Rev.*, 1920, **27**, 1, pp. 50–72. It has been well said that the present interest of teachers in psycho-analysis is based upon an anxiety to secure that "the stuff shall not be flawed in the process of moulding the new citizen." The dangerous aspect of this interest has already been pointed out (SCIENCE PROGRESS, April 1920, p. 587) ; but those who doubt the value of the results achieved in special cases should read the interesting and moderate paper by Mr. Cyril Burt, "The Dreams and Day-dreams of a Delinquent Girl," *Journ. of Exp. Ped.*, 1921, **6**, 1, pp. 1–12. Prof. Pear has a short but valuable paper on "Training in Scientific Thinking" in the *School Science Review* for Dec. 1920, pp. 221–223.

The claim that educational experiment can alone decide questions of method was made in a previous article (SCIENCE PROGRESS, July 1920, p. 47). An admirable example of what can be done in this way is provided in Dr. W. H. Wrinch's paper on "'Equal Additions' versus 'Decomposition,' in Teaching Subtraction," *Journ. of Exp. Ped.*, 1920, **5**, 5, pp. 207–220 ; and **5**, 6, pp. 261–270. Many outstanding problems are being hopefully attacked by the way of experiment ; as, for example, the questions raised by the theory of formal training. References to recent work on this subject will be given in a future article.

"The School and the Community Spirit," *Journ. of Exp. Ped.*, 1920, **5**, 5, pp. 250–255, is a suggestive criticism of Prof. Nunn's *Education : Its Data and First Principles*. A reply by Prof. Nunn appears under the same title, *loc. cit.*, 1921, **6**, 1, pp. 38–40.

ARTICLES

THE DIMENSIONS OF ATOMS AND MOLECULES

By WILLIAM LAWRENCE BRAGG, M.A., F.R.S.
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It has long been known that the dimensions of the simpler molecules are of the order 10^{-8} cm. When we try to determine these dimensions accurately, however, we are faced with the difficulty of defining with precision that which we are attempting to measure—a difficulty which is due to our ignorance of atomic and molecular structure. Even so, various lines of reasoning have agreed in assigning diameters to the molecule, regarded as a spherical body, which lie between one and four Angstrom units (1 A.u. = 10^{-8} cm.). It is the purpose of this article to review the evidence on which these estimates are based, and to give examples which will illustrate the general concurrence between the results obtained by different methods.

The dielectric constant of a gas is greater than unity, and we must therefore suppose that the molecules become polarised in an electric field. A rearrangement of their structure takes place which has the same effect as that of opposite electrical charges induced at either end of the molecule. According to Mossotti's hypothesis, the molecules of the gas may be regarded as small conducting spheres, and with this assumption the diameter of the spheres can be calculated from the dielectric constant. The dielectric constant K and the radius σ of the molecules are connected by the equation

$$K - 1 = 4\pi N\sigma^3 \qquad (1)$$

where N is the number of molecules in unit volume. Values of the atomic diameter for the inert gases calculated in this way are given below:

	2σ (Diameter of atom).
Helium	$1 \cdot 19 \times 10^{-8}$ cm.
Neon	$1 \cdot 46$,,
Argon	$2 \cdot 36$,,
Krypton	$2 \cdot 70$,,
Xenon	$3 \cdot 18$,,

This method must be regarded as giving a lower limit to the diameter of the molecule, since it has been assumed to be a perfect conductor. If the distortion of the atomic structure in the electrical field is not so great as to justify this assumption, the diameters of the molecules must be larger than those given above.

The expressions for the viscosity, thermal conductivity, and diffusion coefficient of a gas, which are deduced by the Kinetic Theory, depend on the free path of the molecule, and so involve the molecular radius σ. The experimental data can be satisfactorily explained by supposing, for purposes of mathematical treatment, that the molecules are hard spheres, which rebound on collision, and which exert an attractive influence when in close proximity to each other. The average length of the free path depends both on the size of the spheres, being inversely proportional to the area $\pi\sigma^2$ of the target which each molecule presents to other molecules, and on the attractive force between the molecules, which tends to bring molecules into collision which would otherwise pass each other. If the attraction were non-existent, the viscosity η, free path λ, and molecular radius σ would be connected by the equations

$$\eta = 0{\cdot}499\,\rho\lambda\sqrt{\frac{3p}{\rho}} \qquad (2)$$

$$= \frac{1}{\sqrt{2}\,N\pi\sigma^2} \qquad (3)$$

where p is the pressure of the gas, ρ its density.

The attractive force between molecules increases the number of collisions, and Sutherland has shown that its influence may be allowed for by supposing the effective diameter of the molecule to be increased by an amount which depends on the temperature. The Sutherland correction may be stated in the form

$$\sigma_0 = \sigma\left(1 + \frac{S}{T}\right)^{-\frac{1}{2}} \qquad (4)$$

where σ_0 is the true radius of the spheres, σ the apparent radius at a temperature T. This may be interpreted physically as meaning that, the higher the temperature and consequent average velocity of the molecules, the less important is the part played by the attractive force in increasing the number of collisions. The constant S can be calculated from the variation of viscosity with temperature. From equations (2), (3), and (4) the value of $\pi\sigma_0^2$ may be determined by measuring the viscosity of a gas η and its temperature coefficient.

An estimate of the molecular diameter may also be obtained

from the value of the constant b which appears in Van der Waal's equation

$$\left(p + \frac{a}{v^2}\right)\left(v - b\right) = RT$$

This constant is introduced in the formula because the molecules occupy an appreciable fraction of the total volume, v, of the gas. It can be shown that

$$b = \frac{16}{3} N\pi\sigma^3$$

A determination of b may be made by observing the departure from Boyle's Law in the behaviour of the gas, or from critical data.

In both these last cases the " diameter " of the molecule, regarded as spherical in shape, represents the shortest distance between the centres of two molecules when they come into collision, and we would therefore expect that estimates of the diameters by these two methods would be the same. That this is actually the case is shown by such a table as the following, due to Chapman (*Phil. Trans. Roy. Soc.*, **216** A, p. 347) :

	$\sigma_1 \times 10^8$ (Viscosity).	$\sigma_2 \times 10^8$ (Van der Waal's).
Helium . . .	1·89	1·96
Argon . . .	2·84	2·85
Krypton . . .	3·12	3·14
Xenon . . .	3·47	3·42
Hydrogen . . .	2·36	2·52
Nitrogen . . .	3·10	3·08
Oxygen . . .	2·93	2·89
Air	3·08	3·30
Carbon dioxide . .	3·20	3·20

In these calculations the molecule has been regarded as a spherical body. Although it may be legitimate to do this in the case of a monatomatic gas, a molecule containing several atoms must be regarded as more complex in shape. The quantity $\pi\sigma^2$ which enters into the expression for the free path does not, however, depend for its interpretation on the assumption that the molecule is spherical. It is introduced as representing the size of the target which the molecule presents to other molecules approaching it from all directions, and it has a definite meaning even if the molecule is not spherical in form. This will be referred to later in considering Rankine's results on the dimensions of molecules.

The discovery of the diffraction of X-rays by crystals, and the consequent elucidation of the arrangement of the atoms in a

crystal structure, has made it possible to obtain a clearer idea of the relative positions of the atoms in the chemical molecule. The crystal structure is composed of atoms, or groups of atoms, arranged in a definite pattern, the unit of which is repeated at regular intervals in three dimensions. By means of X-ray analysis, the dimensions of the pattern, and the relative positions of the individual atoms, can all be measured with a high degree of accuracy. We can find the distance between any given atom and the neighbouring atoms, and so get an estimate from a new source of the size of the atomic structure.

In order to realise the significance of the atomic arrangements in crystals, it is necessary to consider them in the light of recent theories of atomic and molecular structure. Rutherford has shown that the positively charged part of the atom consists of a nucleus, whose dimensions are small compared with those of the atom, and with which practically the whole of the mass of the atom is associated. The positive charge on the nucleus is an integral multiple of the unit of electrical charge, the negative counterpart of which is the charge borne by the electron. The place of an element in the periodic series is determined by the charge on the atomic nucleus, the hydrogen nucleus having a single positive charge, the helium nucleus a double positive charge, and so on through the periodic table to the element uranium, whose atom has a nuclear charge of 92 positive units. The number of electrons surrounding the nucleus of the uncharged atom is equal to the number of positive units of charge on the nucleus, and so is identical with the "Atomic Number" of the element, a number the importance of which was revealed in so striking a manner by Moseley's work on X-ray spectra. In passing from one element to the next in the periodic table, one unit is added to the charge on the nucleus and one electron to those in the atomic structure.

The arrangement of electrons in the atom of an inert gas may be supposed to be an exceedingly stable one, since these atoms do not enter into chemical combination with other elements. The chemical behaviour of the three successive elements such as chlorine (atomic number 17), argon (18), and potassium (19), may be understood by assuming this stability of structure in the case of an inert gas. The potassium atom has one more electron than the stable argon atom, and in consequence the additional electron is easily detached from the atom, which then becomes a singly charged positive ion. The chlorine atom has one electron less than the number in the argon atom, and there is a tendency for an additional electron to be absorbed into the structure, forming an anion with unit negative charge. The calcium atom (20) tends to lose two electrons, the sulphur atom (16) to gain two, and so for other elements near one of the

inert gases in the periodic series. The atom tends to lose or gain electrons until the total number is equal to that for one of the inert gas atoms, a relation which has been pointed out very clearly by Kossel (*Ann. der Physik*, **49**, 229, 1916). Without any knowledge of the reason for the stability of those particular arrangements of electrons which constitute the inert gases, the chemical behaviour of the elements may be explained as a tendency of the electrons to take up a more stable arrangement.

It is supposed that this can come about in two ways. A molecule such as that of potassium chloride is held together on account of the electrostatic attraction between the two oppositely charged ions of which it is formed. The more stable arrangement of electrons is realised by the potassium atom parting with an electron and the chlorine atom absorbing one, so that both atoms have the same number of electrons as argon. We thus have two complete stable electron systems surrounding each nucleus, the two being bound together by the residual charge on each.

Another mechanism must be assumed for the combination of two electro-negative atoms, for instance, two atoms of chlorine. In this case both atoms have one electron less than the argon atom, and it is supposed that the more stable arrangement is realised by the two atoms holding a pair of electrons in common, this pair in some way fulfilling the same role in giving rise to a stable structure for both of the atoms. The justification of this view, which has been largely developed by Lewis (*Journ. Amer. Chem. Soc.*, vol. xxxviii, p. 762, 1916) and Langmuir (*Journ. Amer. Chem. Soc.*, vol. xli, p. 868, 1919), lies in the simplification which it introduces into the conception of chemical valency in the case of many compounds of electro-negative elements. One, two, and perhaps three, pairs of electrons may be thus held in common by two electro-negative elements.

Crystalline structures show very clearly that there is a difference in atomic arrangement for these two types of chemical combination which have been postulated. In a crystal of potassium chloride, the most accurate analysis which has so far been possible fails to reveal the existence of anything corresponding to the KCl molecule. The structure consists of an alternate arrangement of potassium and chlorine atoms situated at the corners of a series of cubes which form the basis of the structure. Each potassium atom is surrounded by six chlorine atoms at equal distances from it, and *vice versa*. This is difficult to understand if the potassium and chlorine atoms are bound in pairs by a valency bond, but becomes comprehensible when they are regarded as oppositely charged ions, since in that case the electrostatic attraction of the potassium kation may be

exerted equally on the surrounding chlorine anions. On the other hand, in a crystal such as calcite ($CaCO_3$) the four electro-negative atoms forming the CO_3 group are bound together in a unit with a special relationship to each other, and with a relatively small distance between the atomic centres, a fact which may be explained by the conception of certain electrons being common to pairs of atoms so that their structures have become closely linked together.

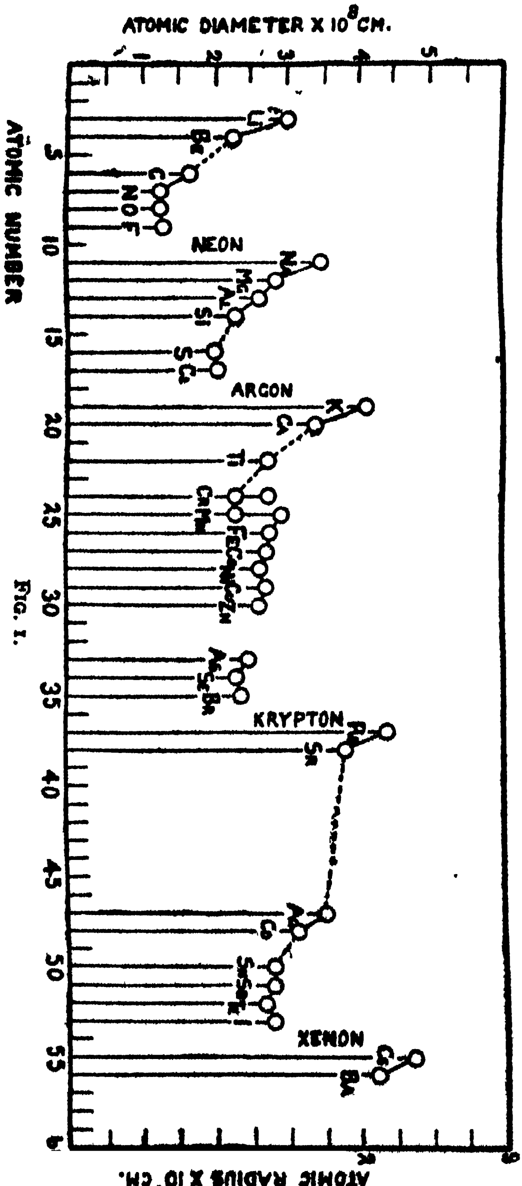

FIG. 1.

The table on the following page will show the relationships between the distances separating atoms of metal and halogen for a series of crystals which have the same structure as potassium and sodium chlorides. This distance is that of the side of the elementary cubes on which the atoms are arranged. All distances are expressed in Angstrom units (10^{-8} cm.).

The substitution of chlorine by bromine, or bromine by iodine, increases the distance between atomic centres by an amount which is approximately the same throughout the series of compounds, as the figures in italics show. The same is true when potassium replaces sodium, or

	Na		K		Rb
F	2·39	*·34*	2·73		
	·42		*·40*		
Cl	2·81	*·32*	3·13	*·15*	3·28
	·16		*·15*		*·16*
Br	2·97	*·31*	3·28	*·16*	3·44
	·26		*·24*		*·22*
I	3·23	*·29*	3·52	*·14*	3·66

rubidium potassium. It has been shown recently that the corresponding cæsium compounds are not isomorphous with the above salts (Davey and Wick, *Proc. Am. Phys. Soc.*, December 1920), so that they cannot be included in this comparison.

Similar relationships hold for other series of compounds. The substitution of sulphur for oxygen, for example, increases by a nearly constant amount the inter-atomic distance in a number of compounds, and further, this amount is the same as that in the case of the substitution of fluorine by chlorine, the elements which follow oxygen and sulphur in the periodic table. An attempt to summarise this relationship has been made by the author, which may be exemplified by the graph in Fig. 1. The abscissæ in the graph are the atomic numbers of the elements. The ordinates represent the " diameters " of the atoms, using this term in a very special sense. The crystal is regarded as composed of a set of spheres (the atoms) packed tightly together, so that the distance between two neighbouring atoms is equal to the sum of their radii. It is found that, by a suitable choice of radii, we can suppose each atom to be of constant size in all the structures of which it forms part. Atoms such as those of the metals potassium, calcium, and so forth have large diameters assigned to them, since they occupy isolated positions in the crystal structure. The electro-negative elements combined together are closely associated, and must be represented by small spheres when building a model of the atomic arrangement. To a very fair approximation, the distance between any two atoms in a crystal structure may be predicted by adding together the radii of the atoms as given by this graph.

In each period of the atomic series, the diameters in the graph approach a lower limit. These limiting values increase from one period to the next, their values being :

Second period (ending in neon) .	$1{\cdot}30 \times 10^{-8}$ cm.
Third period (ending in argon) .	$2{\cdot}05 \times 10^{-8}$ cm.
Fourth period (ending in krypton)	$2{\cdot}35 \times 10^{-8}$ cm.
Fifth period (ending in xenon) .	$2{\cdot}70 \times 10^{-8}$ cm.

It cannot be assumed, *a priori*, that the inter-atomic distances in an isolated molecule will be the same as those in a group of atoms which form part of an extended crystal structure, for the

forces determining equilibrium may be of a somewhat different nature. The work of Rankine, which will now be discussed, indicates that the assumption is justifiable, and that one can predict the distance between the centres of electro-negative elements forming a molecule by using the data represented by Fig. 1.

The length of the free path of a molecule in a gas depends on the size of the target which the molecules present to each other when approaching. From the viscosity of the gas we can calculate the size of this target, the attraction of the molecules being allowed for by means of the Sutherland correction. Previously, in the absence of any knowledge as to its constitution, the molecule has been considered as a small spherical body and its diameter calculated on this assumption. Rankine (*Proc. Roy. Soc.*, **98** A, 693, pp. 360–74, January 1921) has reconsidered this question in the light of the evidence as to molecular constitution afforded by crystal structure.

We may illustrate this by taking the case of the gases argon

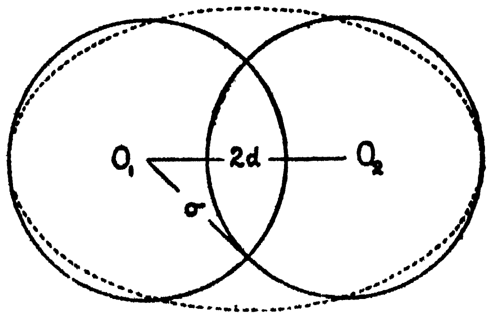

FIG. 2.

and chlorine. Argon is a monatomic gas, and in this case it is almost certainly justifiable to consider the atoms as spherical in shape. The diameter of the atom, calculated from the viscosity of argon, comes to be $2{\cdot}87 \times 10^{-8}$ cm. Chlorine occupies a position just before argon in the periodic table. The chlorine molecule is formed by the sharing of a pair of electrons by two chlorine atoms. If the data given in the graph of Fig. 1 are correct, we would expect the centres of these two chlorine atoms to be at a distance of $2{\cdot}05 \times 10^{-8}$ cm. apart. Rankine, therefore, considers the chlorine molecule to be composed of two atoms each identical with argon as regards their outer structure, except at the point where they come in contact. Here the structures have interpenetrated to a certain extent, since the distance between the centres is less than the diameter of either. The molecule, from the point of view of the Kinetic Theory, may be regarded as the dumb-bell shape structure shown in Fig. 2.

It is possible to calculate the average area of the target presented by such a molecule to other molecules approaching it from all directions. When the molecule is end on, the target will be no larger than that presented by a single atom, but when its longer axis is perpendicular to the direction from which the other molecule is approaching the target presented will be nearly twice as great. Rankine has made calculations of these areas, and in the following table they are compared with those deduced from the viscosity of the gases.

Gas	$\pi\sigma^2$	Ā	Gas	S	Ā/S
Ne	0·435	0·67	O_2	0·69	0·97
A	0·648	1·06	Cl_2	1·07	0·99
Kr	0·797	1·31	Br_2	1·28	1·03
X	0·970	1·61	I_2	1·56	1·03

In this table the area $\pi\sigma^2$ of the inert gas atom is given in the second column. The area Ā in the third column is that calculated by Rankine for two atoms of corresponding dimensions joined together by holding electrons in common, the centres being separated by the appropriate distance deduced from molecules in crystals. The area S in the fifth column is that deduced from the viscosity of the corresponding diatomic gas. In the last column is given the ratio of the calculated and observed areas. The relation between the dimensions of the chlorine molecule and argon atom, which has been already referred to, would also be expected to exist between fluorine and neon, bromine and krypton, iodine and xenon. Since the viscosity of fluorine is not known, Rankine has compared the oxygen molecule with the neon atom in this case. It will be seen that the ratio in the last column is very nearly unity—a fact which confirms the estimate which has been made as to the distance separating the atomic centres. Rankine has further applied the same reasoning in the case of the CO_2 and N_2O molecules, which he has assumed to consist of three neon structures linked together in a row, and again gets satisfactory agreement between calculated and experimental values.

Another method of estimating the distances between the centres of atoms in a molecule is afforded by the absorption spectra of gases. A gas such as HCl has an absorption band in the infra-red, whose frequency, to a first approximation, corresponds with the frequency of the radial vibration of the hydrogen and chlorine atoms, towards and away from each other, under the influence of the forces binding them together. When this band is analysed it is found to consist of a number of regularly spaced narrow bands. Owing to collisions the molecules of the

gas are set in rotation. The linear velocities of the molecules may have any value, the distribution of velocities amongst the molecules being given by Maxwell's laws. On the other hand, the angular velocity of the molecule according to the Quantum Theory must be one of a definite series of values which are determined by Planck's constant h and the moment of inertia I of the molecule. Since these molecular rotations modify the absorption frequencies, this gives rise to the series of bands characteristic of absorption spectra. It can be shown that the frequency difference $\delta\nu$ between successive bands is given by the formula :

$$\delta\nu = \frac{h}{4\pi^2 c I}$$

when c is the velocity of light.

We can measure this frequency difference and calculate the moment of inertia I of the molecule. Therefore, since we know the masses of the hydrogen and chlorine atoms, we can calculate their distance apart. The substitution of chlorine for fluorine, or bromine for chlorine, increases this distance between the atomic centres.

The determination of the size of the atom from the average length of the free path in a gas, and from the Van der Waal's constant b, corresponds to an estimate of the shortest distance between the atomic centres when the strong repulsive forces come into play which cause the atoms to rebound from each other. On the other hand, the determination of inter-atomic distances by means of crystal analysis, or by such information as may be obtained from absorption spectra, tells us the distance between the atomic centres when the atoms are linked in chemical combination. In some cases we suppose the molecule to consist of separate structures held together by electrostatic forces, in others that the fusion of structure takes place which has been termed the holding of electrons in common. The relations between the inter-atomic distances in crystals have been interpreted by supposing that the atoms were spheres in contact with each other, but it is not to be expected that the diameters of the spheres found in this way will correspond with those given by the Kinetic Theory. We are on surer ground, however, in comparing the *increase* in atomic dimension given by all these methods when we substitute for an atom the corresponding atom in the next period of Mendeleef's table. Both crystal analysis and Rankine's work on the viscosity of molecules confirm the view that the atoms near the end of any one period have nearly the same structural dimensions. We can, for instance, compare the difference in σ for argon and krypton given by the Kinetic Theory, with the increase in inter-atomic

distance when bromine is substituted for chlorine in a molecule. This comparison is made in the following table :

	$\sigma_0 \times 10^{-8}$	$\sigma_1 \times 10^{-8}$	$S \times 10^{-8}$
Neon	1·17	0·65	HF 0·93
	0·26	*0·37*	*0·35*
Argon	1·43	1·02	HCl 1·28
	0·15	*0·15*	*0·15*
Krypton	1·58	1·17	HBr 1·43
	0·17	*0·18*	
Xenon	1·75	1·35	HI —

In this table σ_0 is the atomic radius calculated from the viscosity of the inert gas. The quantity σ_1 is the contribution of the atom to inter-atomic distance when holding electrons in common with another atom. It is, for instance, one-half the distance between two halogen atoms in such molecules as Cl_2, Br_2, etc. In the fourth column the distance S is that between the hydrogen and halogen atomic centres found from the absorption spectrum. It will be seen that there is a general agreement between the estimates of the increase in atomic dimensions (figures in italics) in passing from one atom to that with similar properties in the next period.

The evidence on which these estimates have been based is still very incomplete. In the case of crystal structures particularly, those possessing most interest contain complex molecules or ions, and the corresponding complexity of structure makes analysis difficult. An attempt has been made in this article to summarise what has already been discovered, and to show that some advance has been made in the direction of constructing a model of the molecule, with the atoms in their correct relative positions.

NATURAL INDIGO

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Sometime Indigo Research Botanist to the Government of India

Much has been written of the brilliant chemical investigations and their industrial applications which led to the synthesis of indigo blue and to its production in such quantity and at such a price as seriously to cripple the ancient method of preparing it from various species of indigo plant. While great efforts were being made in Germany to produce indigo—efforts which involved much costly scientific research and the employment of distinguished chemists—the Indian indigo industry preserved the even tenor of its way much as it had done for centuries. It is true that the planters, in Behar especially, systematised its production and effected considerable improvements in its manufacture and the purity of the product, but they were just as much in the dark as to the chemical changes involved and the factors influencing these changes as were the natives of India who produced it in small scale concerns.

When, however, the production of synthetic indigo was an accomplished fact, and its price, uniform quality, and convenient make-up in the form of 20 per cent. paste, had seriously cut into the prices obtained for natural indigo, the Behar Planters' Association, subsidised by the Government of Bengal, started investigations into the methods of growing the indigo plant and of the manufacture of the dye. Had such investigations been taken in hand earlier it is quite possible that the synthetic product would never have been able to emerge from the experimental stage. As is well known, however, its position was soon established, and up to the beginning of the war its production and export were steadily reducing or altogether inhibiting the production of the natural dye in India, Java, China, and other countries. The story of the decline—and almost the fall—of natural indigo has been outlined by W. A. Davis (*Agric. Journ. of India*, vol. xiii, Jan. and April 1918).

Sirsiah Investigations.—The Planters' Association started work at Mozafferpur in 1898, and work was continued at other stations under Rawson, Bloxam, Bergtheil, Parnell, and others

till March 1913, when the investigations on the agricultural and botanical aspects of the industry were transferred to the Botanical Section of the Agricultural Research Institute, Pusa, which is directly under the Imperial Department of Agriculture in India.

Much spade-work had to be done at the start, methods for estimating the indican content of the plant had to be worked out, its localisation in the tissues investigated, and estimations had to be made of the percentage of indican in various species of indigo. At the same time numerous manurial trials were carried out, and the progress of the *mahai*, or fermentation of the indigo plant in the vat, was studied especially with regard to the influence of temperature upon its rate. The results were in many cases very conflicting and difficult to interpret, but recent work has explained many apparent anomalies in them. Before going in detail into these and later investigations it may be as well to outline very briefly the fundamental operations in the preparation of natural indigo.

The Indigo Plant.—Of this there are various species, the commonest grown in India being *Indigofera Sumatrana* (Gaertn.) and *Indigofera arrecta* (Hochst.). The latter was introduced from Java in 1899 by H. A. Bailey, and is now the principal indigo crop in Behar and Assam. These are leguminous plants, and nodules produced by nitrogen-fixing bacteria are found on the roots. Sumatrana indigo is an annual with comparatively shallow roots, whereas Java indigo is a perennial, when under favourable conditions, and its root system is much deeper. It is believed that the Java plant arose as a cross between a Natal species and the original Java or Guatemala type of indigo. It is made up of a number of types, early flowering ones with comparatively shallow roots, and late flowering types which as a rule have deeper roots.

In Behar it is customary to sow Java indigo seed for mahai in October. It may be remarked that in the highly calcareous alkaline soils of Behar but few of the properly matured seeds will germinate. This was shown by Bergtheil and Day to be due to the development of a thin cuticular layer outside the cellulose wall of the seed-coat. Such a layer is absent from Sumatrana indigo. In Java, and also in Assam, where this plant is now being grown extensively, the soil conditions are such that the cuticle is not an obstacle to germination. As suggested by Butler, the Java seed is now treated with strong sulphuric acid for a specified time before it is sown in Behar.

The seed sown in autumn rises to a height of a few inches normally, at which stage further growth is arrested during the cold weather. With the return of warm weather development is rapid, and by May or June the first cutting—the

moorhan crop—is gathered. The Java plant grows in Behar to a height of about 3 or 4 feet, more or less according to the soil conditions, and a bushy habit, with a high percentage of leaf, calculated on total weight of the cut plant, is preferred to a more erect woody type. In Assam, however, where the rainfall is plentiful and the soil conditions exactly suit the plant, a height of 8 to 10 feet is attained in the same time of growth. If left in Assam for a whole season the plant grows to a height of 12 to 15 feet.

When cut only about 6 inches of stem is left, but under favourable soil and climatic conditions growth is rapid, so that in a month or six weeks a young plant, with a high percentage of leaf, is ready for a second cutting. This is called the *khoontie* crop. Where the land is liable to be flooded it has been found advantageous to sow as early as the rains permit, in September if possible, and thus to obtain the khoonties before they are destroyed by floods. The Sumatrana plant is usually sown in February, after a period of fallow. Before the seed is sown the ground is hoed and ploughed several times in longitudinal and transverse directions. The loose soil is then compacted by a roller. Correct preparation of the land is a matter of much importance.

The Manufacture of Indigo.—The plant is usually cut as early in the day as is possible, and is carted to the factory. It is then loaded into rectangular vats of about 1,000 cu. ft. capacity, with a depth of about 3 ft. 9 in. to the level of the cross-beams. The vats are brick, covered with Portland cement. They are filled up to the cross-bars, which, when in position, serve to retain the plant when it tends to rise during the fermentation.

It is usual to have six or more of these vats side by side. Running along the ends of these steeping vats is a beating vat, which is at a lower level. Down the middle of this vat runs a brick wall about 3 feet high, but it is not continued to meet the walls at either end. A paddle-wheel at one end throws the liquid of the vat into the air in a fine spray and causes it to circulate round the middle partition.

The vats are loaded with cut indigo plant, the amount used varying according to the species, being approximately 50 per cent. greater with Sumatrana than with Java indigo.

Water from a river, stream, or reservoir is then run in so as to cover the plant, and the latter is allowed to steep in it at a temperature of about 90° F. Bergtheil drew up a table showing the average optimum time of steeping for the two varieties of plant. For Java indigo twelve and a half hours at 90° F. is recommended, the time being decreased twenty minutes for every degree rise in temperature, so that at 102° F.

only eight and a half hours are required. It is considered that a proper fermentation does not take place below 90° F., so some factories have a steam-pipe to heat the water if it is not up to the desired temperature. In Behar, however, this temperature is usually reached or exceeded during the mahai season without the aid of artificial heat.

During the steeping a complicated series of changes occurs. These are induced by bacterial and enzymic actions, and the net result is that, after a brisk evolution of gases, nitrogen followed by hydrogen and carbon dioxide, the glucoside indican is hydrolysed and its important constituent, indoxyl, is liberated from the leaf and passes into the steeping water, to which it imparts a greenish fluorescence. The steeped plant, or *seet*, is used as a manure for cereal and other non-leguminous plants, as it is specially rich in readily available forms of nitrogen.

When the steeping is at an end the liquor is run off into the beating-vat, where it is immediately beaten for an hour to an hour and a half. From time to time samples are removed and tested. When all the indoxyl has been oxidised the beating is stopped, and the indigo blue (indigotin) is allowed to settle. Under normal conditions this requires two or three hours. When, however, settling is not good, a serious loss of indigo occurs, as the seet water retains a minute percentage of the finely divided dye, which imparts to it a greenish tinge. This condition is known as " green vat."

The precipitated indigo tends to collect in the lower end of the sloping beating-vat, the seet-water is drained off through plugs in the side, and the watery precipitate is passed through strainers into a well, from which it is pumped up to a tank. It is again strained and passed to the boiling-tank.

The boiling of the watery precipitate, containing about 0·5 per cent. of indigo, is necessary in order to prevent a destructive type of fermentation and to promote good filtration. The liquid is heated by steam to 160° F., allowed to settle, the supernatant liquid drained off, and more water is added. With this dilute sulphuric acid is mixed, and the liquid is again heated to 160° F. After settling, draining, and renewing the water the liquid is heated almost to boiling. The clear liquid is again run off and the watery indigo, or *mal*, is run on to filtering-tables across which stout cloths are stretched. The mal thus concentrated contains 8–10 per cent. of indigo and is carried to presses and subjected to pressure for several hours. As a result a slab about 3 inches thick is produced. These slabs are cut into small cakes and allowed to dry in special sheds for several months. During this time bacterial action results in the liberation of ammonia and moulds grow on the

cakes. It has been shown by Rawson that these actions do not destroy the indigotin, but only the impurities, so that the indigo-cake becomes slightly richer in its percentage of indigotin.

Lines of Research.—Whereas the production of synthetic indigo blue demands investigations of a chemical nature it is obvious that the preparation of the natural dye necessitates work in a wide range of sciences.

Agricultural problems are met with in connection with the growth of the plant involving both cultural and manurial conditions. With these are closely connected botanical considerations as to the type of plant and its fertilisation and bacteriological problems in connection with the soil and root-nodule bacteria. The place of the indigo plant in a rational scheme of crop rotation is also a point of much agricultural importance.

Indigo is fortunately free from any serious fungal or bacterial disease, the disastrous wilt having been shown to be connected with the soil conditions rather than with any specific organism. Weakly crops, however, suffer severely from an insect pest, *psylla.*

In the mahai, or fermentation process, there is much room for the study of the bacterial and enzymic changes which normally result in the production of indoxyl, but may if allowed to proceed too far result in its destruction.

Finally, the nature of the end-product, indigo cake or paste, has to be considered with special relation to the possible market.

The investigations carried out have naturally been of a complex nature, on the borderland between two or more of the recognised sciences, so they will be considered from the standpoint of the crop or process rather than that of the sciences concerned.

Agricultural Conditions.—Formerly indigo was grown by planters on land specially leased for the purpose, and owing to expiration of leases and new takings the land actually under indigo in Behar was constantly changing. At present indigo is not such a paying crop as it was, and the planters grow other crops as well, the seet forming an excellent manure for cereals such as wheat and oats. Indigo, being a leguminous plant with nitrogen-fixing bacteria in root-nodules, brings about an increase in the soil nitrogen. In order that the bacteria may carry on this fixation, air must have free access to the roots, and the aeration of the soil is accordingly of even greater importance than for other plants. Thus, both on account of the supply of seet and of the roots left in the ground, indigo occupies the place of a nitrogen-enriching crop in the rotation.

It, however, makes a considerable demand upon the available phosphates, and continuous growing of indigo on the same land has in many cases resulted in such a depletion of the soil in this constituent that the crop no longer flourishes. This has been emphasised by Davis in several of the Indigo Publications of the Pusa Research Institute. Abundant phosphate in the soil promotes root growth, and, as has been pointed out by Howard and Howard in publications of the Pusa Botanical Series the wilt which has caused so much damage, especially to khoonties, is accompanied by failure to develop new rootlets after their destruction as a result of the cutting back of the plant.

The wilt disease was first reported in the Sumatrana plant in Behar in 1907, and the khoonties were entirely lost. According to Davis it was first observed in plants grown in soils which were abnormally deficient in available phosphate (below 0·0005 per cent.), and did not show itself at all in soils relatively better supplied with phosphate (above 0·001 per cent.). The Java plant was, however, equally affected. Now, whereas the Sumatrana indigo is shallow-rooting, the Java is deep-rooting and derives its nourishment mainly from below the top six inches of soil. Davis has found that, in cases in which the khoonties failed or in which August sowings for seed were poor, the subsoil was even more deficient in available phosphate than was the surface layer. Thus both on the score of better aeration, which favours nodule activity as emphasised by Howard and Howard, and on that of phosphate supply, surface rooting forms have advantages under these special soil conditions. On the other hand, when first introduced into Behar the Java plant grew luxuriantly, and both in its yield of plant per unit area and of indigo per unit weight of plant it far surpassed the Sumatrana. Davis considers that this was due to the fact that it drew on the deeper layers of the soil which had not been exhausted by cereals or the shallower rooting Sumatrana indigo. Quite apart from this, however, in equally advantageous soil conditions the Java plant is taller and more vigorous than the Sumatrana, and has a higher leaf percentage.

As far back as 1900 Rawson had drawn attention to the deficiency of available phosphates in some of the indigo soils of Behar. In 1907 Leather reported that they were almost uniformly deficient in it. Neglect to remedy this has resulted in still further depletion of phosphates, and Davis has urged the necessity for manuring with superphosphate.

Though phosphate deficiency is doubtless the limiting factor in the growth of indigo in many places, it does not follow that it is the only one that is ever operative. Thus, under

special experimental conditions in a lysimeter, Howard and Howard showed that water-logging of the soil might be of more importance than phosphate supply, and Davis has pointed out the necessity of remedying the deficiency in organic matter which has been noted in many of these soils. This, he recommends, should be dealt with by ploughing in sannai (*Crotalaria juncea*) as a green manure, or by combining this with superphosphate treatment. The remarkable increase in plant and seed yield brought about at Pusa by the addition of leaf-mould has been demonstrated by Howard and Howard. With regard to the development of wilt in khoonties, planters attach special importance to the correct timing of the cutting with regard to subsequent rainfall. This may either drown out the stalks or promote such a rapid growth as to emphasise the phosphate deficiency.

From the foregoing brief sketch it may be seen that the successful cultivation of indigo in Behar is beset with many problems. It may be added, though, that in parts of Assam a sandy, well-aerated soil rich in organic matter and in phosphates, combined with a liberal rainfall and the requisite temperature, gives conditions very well suited to the growth of Java indigo, so that heavy yields of plant rich in indican are obtained ; wilt is unknown, and seed is well formed and matured. Yields of ten maunds per acre can be obtained as against a half to one and a half maunds of seed in Behar. A falling off in seed was one of the first symptoms of soil deficiency observed in Behar with this crop.

Species and Varieties of Indigo.—The Natal-Java indigo plant usually grown in Behar and Assam consists of a number of varieties differing as regards their general habit both above and below ground. Details of these may be found in the final report of the Sirsiah Station, 1913, in which Parnell summarises the botanical work, and in the Pusa Memoir by Howard and Howard, 1920.

It must be mentioned that, after the introduction of the Java plant, seed was obtained both from Indian-grown Java plant and from Java. As indigo went out of cultivation by the Dutch planters in Java, who, owing to the competition of the synthetic dye turned their attention to sugar, the supplies of seed could in time be obtained only from natives of Java. When, owing to the wilt, the Behar crop of Java plant failed to set seed, recourse was had to natives of Java for seed. But, owing to crossing with wild forms in Java, it appears highly probable that the resulting mixture of types was not the same as was originally obtained from the Dutch planters. It has been pointed out by Howard that this mixture contained many of the deeper rooting forms which are not so well

adapted to Behar conditions. At any rate, the mixture of types proved highly successful when grown in Assam, and the greater part of the supply of Java seed now sown in Behar is obtained from Assam, though some comes from near Cawnpore, where it is specially grown for seed. Apart from cultural considerations, the aim of botanical research on the indigo plant at Sirsiah was mainly the selection of a variety specially rich in indican. Parnell devoted much labour to this, but disastrous floods seriously hindered it. Not only must the selected plant grow well and give a high yield per acre, but it must also give a high ratio of leaf to total plant. It has been more or less assumed, that the indican content of the leaf is a factor transmitted by inheritance, and Parnell considered that a considerable amount of evidence had been accumulated to that effect. This, however, requires further examination.

Effect of Cultural and Soil Conditions on Indican Content of Indigo Leaf.—It appears that the above conditions affect the indigo plant so largely as quite to outweigh any improvements in the indican content of the leaf transmitted by inheritance.

For example, the same seed, probably a mixture of numerous types of plant, is sown on two similar plots, one untreated and the other manured with seet. It is found that the yield of plant per unit area is double on the treated plot, but in spite of this the yield of indigo blue is the same approximately on both. Thus, merely the luxuriant growth resulting from the manurial treatment has had the effect of diminishing the indican content by 50 per cent. On the other hand, it has been shown that land treated with sannai and superphosphate may give double the yield of plant per acre as compared with untreated land, and at the same time may give the same weight of indigo per hundred maunds of plant.

Seet is rich in nitrogenous compounds, approximating in this respect to farmyard manure in England. Thus, it appears that forcing the growth by a liberal supply of nitrogenous manures tends to lower the indican content, whereas a supply of other necessary substances under conditions which favour root-nodule formation leads to an increased crop without serious diminution in the indican content. In Assam the indigo plant flourishes in a soil sandy, well aerated, and rich in humus and in necessary salts. Under these conditions nodule activity is great, and the indican content is often as much as to give 0·8 to 0·9 per cent. of indigotin in the leaf, whereas in Behar the plant grows poorly by comparison, and is as a rule poorer in indican, though largely grown from Assam seed.

There was thus a considerable amount of evidence, not

amounting to absolute proof, however, that the indican content is related to root-nodule formation in the sense that high indican accompanies high nodule activity. It is, however, proved conclusively that cultural and manurial conditions may cause very great alterations in the indican content.

More recently Davis has shown that the percentage of indican in the leaf appears to be determined mainly by the amount of readily available nitrogenous substances in the soil. When this is high the indican content is low. If grown on a good soil, with a cover crop of wheat, such as Pusa 4, a plant rich in indican is obtained, as the wheat removes nitrogenous compounds from the soil. This is effected by sowing two rows of wheat between each row of indigo. The wheat is reaped in the spring and the indigo, which grows but little in winter, comes on well after its removal. This method of sowing with a cover crop was previously recommended by Howard and Howard, but in the present poor conditions of planters soils may not be practicable, though successful at Pusa.

Effect of the Variety of Indigo Plant upon Indican Content.—Though large alterations are induced in the indican content by external conditions, yet Bergtheil showed that each variety of the Sumatrana plant studied at Sirsiah had an average indican content in which it might differ from other varieties. Differences also occur during the growth of each individual plant, as the indican content increases with age up to a certain point, but decreases during seed formation. It has been found that Java indigo has a higher indican content than other species, yet on one occasion, at Pusa, Java indigo gave 0·6–0·7 per cent. indigotin and Sumatrana gave 0·8. It may be remarked that, while the plant contains the glucoside indican, the results of analyses are usually given as indigotin.

Among the Java plants there are certain very obvious differences, such as colour of stem—red, reddish; or green; shape of stem—round or angular; leaf colour—dark or light; number of pairs of leaflets; pulvinus—fine or coarse. Though this is so, Parnell concluded that the "morphological examination gives no indication of indigo content, and all work on this character must be controlled by chemical analysis, in spite of its tedious nature."

Parnell further studied the curves got by plotting indigo content against numbers of individuals. From the positions of the maximum values he concluded that in an ordinary Java crop there would seem to be a number of races differing with regard to their inherent indigo-producing power. "There are two major groups averaging about 10 per cent. higher and lower respectively than the average of the whole crop, and two minor groups averaging about 20 to 25 per cent. higher

and lower respectively." "If the above explanation is correct it is obvious that immense improvement in the plant can be brought about by the selection and breeding of plants belonging to the high content groups."

It seems, however, from the recent work of Davis, that Parnell's results may possibly be interpreted as indicating that the soil on which his plants grew had various values for the nitrogen content, so that his high- and low-value plants correspond to low and high nitrogen contents. Nevertheless, it is quite likely that certain varieties of each species of indigo plant do give higher values for indican content than other plants of the same species, just as the various species differ on the average. To prove this, and to select high-yielding plants, great care would have to be taken to secure absolute uniformity in soil conditions, and, what is more difficult, in subsoil conditions also, for the Java plant is deep-rooting.

At present, therefore, the position appears to be that it has been shown that indican content may be increased or decreased by external conditions such as the poorness or richness of the soil in nitrogenous compounds. Presumably it varies also with inheritable qualities in the plant, which quite possibly are connected with its relation to the root-nodule bacteria, but no evidence free from uncertainty has as yet been obtained on this point.

The Feasibility of obtaining a Pure Strain of Indigo Plant—As pointed out by Parnell and by Howard and Howard, the floral mechanism of the indigo plant is such that cross-fertilisation is the rule. Examination of the stem colour of the progeny of single plants showed how extensively cross-fertilisation took place. It was further found that plants protected from insect visitors set but little seed. Hand-fertilisation resulted in a larger yield. The above-mentioned workers have also drawn attention to the possibility of sterility resulting from continued self-fertilisation. Parnell concludes that the failure of plants, raised by selfing for two generations, to set seed at Sirsiah was quite possibly the result of this selfing. Howard states that, so far as the evidence obtained at Pusa goes, the indications are most definite that Java plants raised from self-fertilised seed, even in a single generation, show a marked falling off in size and general vigour. If an attempt were made to purify Java hybrids and to obtain a plant breeding true with high indican content, the experiment would, in all probability, fail in a few years on account of self-sterility. Thus, in order to try to produce a type of Java indigo specially rich in indican—assuming that this quality is inheritable—it would be necessary to obtain a number of plants of high content and to carry on crossing inside this group, which of

course makes the process far more tedious and complex than it would be, were it possible to deal with plants which are readily self-fertilised. Furthermore, it does not follow that the plants specially rich in indican would be vigorous and give a high yield of leaf. Indeed, the plants raised at Pusa from seed selected at Sirsiah were of a poor nature vegetatively, but low vigour and high indican content are not necessarily linked together, of course; namely, it is unlikely that they are coupled characters in a Mendelian sense.

Percentage of Leaf as affected by External Conditions.—With regard to leaf yield, Davis has shown that well-spaced plants, which were delayed in their growth by a cover crop of wheat, and subsequently grew rapidly after wheat harvest when the rains came, were of a bushy form, and particularly rich in the proportion of leaf to total plant. Thus this factor also is largely modified by external conditions.

From a consideration of the facts and opinions quoted already, and from a general impression gained by perusal of the reports of the research stations, it appears to the writer that the chances are small that results of value are to be obtained from selection work aiming at the production of a variety specially rich in indican. Improvement is to be sought rather in the selection of specially vigorous plants.

Whether the natural indigo industry survives or dies out depends upon soil and cultural conditions, and upon conditions of manufacture. The possibility of improvement in the plant is a secondary factor on account of the length of time which must of necessity elapse before any results can be expected.

As an item of practical interest, it may be pointed out that were a hardy strain of indigo specially rich in indican to be obtained, it would still be a matter of difficulty to grow it on a large scale owing to the deterioration it would undergo in the field through crossing with the type commonly grown, the pollen of which would be carried by bees.

The Indigo Fermentation, or Mahai.—A very brief account of this has already been given, and as yet it is not possible to write a complete one, for much still remains to be discovered as to the mechanism of the process. The recent work of Davis, in conjunction with Hutchinson and Walton on the bacteriological side, has, however, brought to light many points of interest. When the water is run into the steeping-vats a series of changes is initiated. It is necessary to see how far these are due to autolysis in the plant and how far they are induced by bacterial action. The above-named workers have laid emphasis upon the importance of the latter.

It has been shown that the duration of the fermentation process depends upon the bacterial content of the water used

to flood the vats, and upon the number of indican-splitting organisms in particular. Use was made of agar plates with which a small amount of indican had been incorporated, and it was found, on adding small quantities of water used at various factories, and at the same factory at various times, that the number of colonies showing up blue, through production of indigo from the hydrolysed glucoside, was related directly to the time necessary for adequate fermentation. This was brought to light in connection with the mahai on Assam estates, where the Behar standard optimum times at various temperatures were found to give very poor results. On increasing the time from twelve to twenty-four hours, however, better yields were obtained. More recently mahai has been limited to sixteen or seventeen hours, but, owing to variations in the water-supply, which is derived from small streams instead of rivers or reservoirs as in Behar, the time necessary is variable. This is very decisive evidence as to the preponderating influence of bacteria upon the progress of the mahai.

Furthermore, there is a change in the proportions of the gases liberated, first nitrogen, which begins to be evolved three or four hours after filling, then hydrogen, and finally carbon dioxide being the principal constituent. It appears that the last-named renders the protoplasm of the cells permeable and the indican diffuses out and is hydrolysed to glucose and indoxyl by bacteria.

Davis has shown that the final acidity of the vat-liquor is due to carbon-dioxide. No other volatile acids are present, though possibly traces of malic acid, derived from the plant, are in solution. A study of the hydrogen ion concentration by means of indicators has proved that the strongly alkaline river water, $P_H = 8{\cdot}6$, found at Pusa is changed during mahai so that an acid reaction is developed, the acidity being at least equal to that found in the leaf of the plant, $P_H = 5{\cdot}6$. The yield of indigo is very greatly influenced by the duration of the mahai, as over-steeping results in serious loss through destruction of the indoxyl, so that it can no longer yield indigotin. This Davis has shown to be correlated with a high content of carbon-dioxide in the vat-liquor, though it is uncertain whether the acidity is in itself the cause of the destruction or whether the carbon-dioxide is merely an indication of the presence of bacteria which destroy it. The possibility remains that the losses due to over-steeping are brought about by enzymes derived from the plant, though the sudden fall which accompanies over-steeping is rather against this view.

The correct timing of the steeping is therefore of great importance, but no method of gauging this was known till recently. The times given by Bergtheil as average optimum

values were followed throughout Behar. The disturbances induced by the irregularity of the process in Assam, however, led Davis to devise a simple method whereby the termination of the mahai for optimum yield was regulated by a colorimetric test. Ten cubic centimetres of vat-liquor were withdrawn, and, after adding a few drops of ammonia, shaken up with air to oxidise to indigo blue. The whole was then diluted to 250 cubic centimetres and its colour compared with a sample taken half an hour later. When the colour no longer increases with time the process is stopped by drawing off the water for beating.

As previously mentioned, when the bacterial processes have not proceeded in quite the normal fashion, a green vat is obtained after the indigo has been formed. Owing to the large volume of water used in proportion to the yield of dye, namely, by weight, 3,000 : 1, as little as 0·01 per cent. of indigo in the seet-water represents a loss of 30 per cent. of the yield, At Pusa up to 0·015 per cent. of indigo has been found by analysis in the seet-water of green vats. A much increased yield may be obtained by the use of a small quantity of dhak gum as an aid to flocculation and precipitation. This causes but little lowering in the quality of the indigo cake, whereas other substances, such as alum, tried for this purpose caused the subsidence of much extraneous material and resulted in a cake of lower purity. The gum is a ruby-coloured exudate from the dhak-tree (*Butea frondosa*), and is an article of commerce in parts of India.

Final Product.—The issue in this lies between cake indigo and paste indigo, each of which has certain advantages. The German synthetic product, indigo blue, was exported as a 20 per cent. paste. It was thus of uniform quality and purity, and easily mixed for use. The natural indigo of commerce is usually produced as cake, which contains from 70 per cent. to about 50 per cent. or less of indigo blue, together with small quantities of indirubin and other colouring matters which collectively produce (especially on wool) a dyeing effect slightly greater than that due to its indigo blue content. The balance of the cake is made up of organic material derived from the plant (indigo gluten) or from destructive transformation of indoxyl, and a small amount of ash. The cake is valued by its general appearance and colour or by analysis. For use the cake has to be ground up and got into the form of soluble indigo-white by reduction, nevertheless it is a handy form for transport and needs no special container. The English market is used to cake indigo, but the very large amount of natural indigo produced as a 1 per cent. watery paste in China, and utilised locally, was in part displaced by

the German product, though indigo cake has no sale in the Chinese market. It is obvious that Indian indigo must be prepared as paste if this market is to be captured. With this end in view the production of 20 per cent. paste in drums has already received attention in India. Instead of pressing thoroughly the mal is analysed and diluted if necessary. It is then sterilised by steam heating in drums hermetically sealed. Over 27,000 tons of 20 per cent. paste were imported from Germany into China and Japan in 1913. Arrangements are also being made to grind cake indigo to a standard 20 per cent. paste and send it out in a sterilised form.

Economic Considerations.—Before the war natural indigo was rapidly being displaced by synthetic. It may reasonably be asked why this process should not continue now that the artificial war restrictions are removed. Many believe that the displacement will progress to complete extinction of the natural product, but the price of coal, raw materials, and of labour, now prevailing are such as to give natural indigo a good chance of success as a paying concern for many years to come. This success will largely depend upon the capture of the large Chinese and Japanese markets, and upon the retention of the Indian market. In comparison with these the consumption in Great Britain and the United States is small.

Before the war synthetic indigo was sold in Great Britain at 8*d*. per lb. for 20 per cent. paste, equivalent to 60 per cent. cake at 2*s*. a lb.; with good land, such as that of the Assam Indigo Estates, this price could be undersold by natural indigo; but even with exhausted lands which require manuring with superphosphate or sannai, or both, it is quite probable that indigo will continue to be grown, especially on account of its value in crop rotation as a leguminous plant and the value of the seet as a manure. The development of the tobacco industry in Behar was rendered possible by the use of heavy dressings of indigo-seet, 300–400 maunds per acre. After this treatment land which ordinarily would fetch only Rs. 20 per acre could be let out to the ryot at Rs. 120 per acre. This kept the indigo industry alive in Behar. Whether indigo is retained or not may be largely determined in certain districts by whether it is more profitable for the planter to continue to do so or to turn his attention to sugar production. If indigo is left out of the rotation some other leguminous plant will have to be grown instead, or large sums expended on nitrogenous manures.

Taking into account the savings effected by more efficient control of the mahai and by better manurial and cultural conditions, it appears highly probable that natural indigo will be able to compete successfully for a share of the market.

Details of the work of natural indigo may be found in the Reports of the Sirsiah Indigo Research Station and in the Botanical and Indigo Series of the Agricultural Research Institute, Pusa. To these the author acknowledges his indebtedness; he is also indebted to the workers in the various branches who placed their experience at his disposal in his efforts to give a brief general outline of a large field of work. For its incompleteness and for any inadequate statement of their researches the writer can only ask their indulgence.

THE CYTOLOGICAL PROBLEMS ARISING FROM THE STUDY OF ARTIFICIAL PARTHENOGENESIS

PART II

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In a previous paper [1] certain evidence was given in support of the theory of the individuality of the chromosomes, and for the belief that the X chromosomes are intimately correlated with sex-determination. It is our purpose here to discuss artificial parthenogenesis, and to see what light it throws on these and other cytological conceptions. Taking first the question of sex-production, unfortunately, the number of cases in which it has been determined for parthenogenetically raised animals are at present very few and confined to only one or two species.

Loeb [2] reports that out of nine parthenogenetically raised frogs seven were males and one a female. This somewhat unexpected result led him to conduct a second series of experiments, in which, as before, every precaution was taken to ensure that the eggs laid had not been fertilised. Parthenogenesis was induced by the puncture method of Bataillon, and, of the eggs so treated, ten developed into frogs, one being a female. There seems no doubt, therefore, that both sexes may develop from parthenogenetic frog eggs. In the sea-urchin Delage has repeatedly tried to rear adults from parthenogenetic eggs, but at present with only one success. In this case the adult animal was a male.

This exceedingly scanty data is all there is, at the moment, to discuss; consequently, any conclusion that may be reached can only be regarded as provisional, since the accumulation of more facts, as to the sex of animals so raised, will probably have a modifying or even subversive effect.

Nevertheless, we know definitely that in the frogs both

[1] Cutler, D. W., Science Progress, No. 59, 1921, p. 435.

[2] Loeb, J., *Proc. Nat. Acad. Sci.*, 4, 1918, p. 60.

sexes may arise; whereas, in the sea-urchin, although only the one sex (male) has as yet been obtained, further work may show that both males and females can develop. Such results can, however, be paralleled in natural parthenogenesis where it is easy to find examples of dual sex-production, or the development of only one sex, male or female. Thus, many species of the Aphidæ, Cynipidæ, and Cladocera produce both sexes. Among the bees and wasps the unfertilised eggs give rise to males, while among some species of the Tenthredinidæ (Sawflies) only females develop from such eggs. These phenomena easily fall into line with the sex-chromosome hypothesis if the cytological basis is considered.

In the Rotifera and Cynipidæ, where both sexes are produced from parthenogenetic eggs, those developing into males undergo two maturation divisions and the chromosomes are halved in number. The female-producing eggs, on the other hand, have but one maturation division, which is equational, the chromosome number remaining unchanged.

In the Hymenoptera (ants, bees, and wasps), as mentioned above, males are produced from the parthenogenetic eggs, having the reduced chromosome number, as in the Rotifera, whereas females hatch from fertilised eggs, containing the full complement of chromosomes. There is an interesting case described by Jack,[1] where, on rare occasions, worker bees produce females from unfertilised eggs, but until their cytology is known discussion is profitless.

The Aphids offer many interesting and perplexing problems in sex-determination, but a few cases have been investigated by Morgan, Stevens, and others. Here, again, both sexes arise from parthenogenetic eggs, differing, however, in their chromosome complex. Both types undergo one maturation division, but in the male-producing eggs one of the X chromosomes is eliminated, while in the female-producing ones both X chromosomes are retained. It is impossible to deal here with fertilisation and the associated changes in the chromosome complex in the above species; this question is, however, discussed in an earlier paper.[2] Finally, in those species whose parthenogenetic eggs give rise to females only it is found that, whatever may be the number of maturation divisions, the chromosomes are not reduced.

It will be remembered that in the previous article in this Journal the theory of sex-determination was favoured which ascribed sex as dependent on the quantity of sex-determining substances contained in the egg. In the majority of animals

[1] Jack, R. W., *Trans. Entom. Soc.*, 1916.

[2] Cutler, D. W., *Mem. and Proc., Manchester Lit. and Phil. Soc.*, 62, 1917–18.

the females are quantitatively greater in some substance than the males: " femaleness is due to maleness plus something else." It will readily be seen that natural parthenogenesis accords with this view. In all cases of male-producing eggs the chromosomes are halved in number (Hymenoptera, Cynipidæ, Rotifera), or an X chromosome is omitted (Aphidæ); while in all female-producing eggs the chromosome number is complete. How then do the few observations on the sex of artificially produced parthenogenetic animals fit this hypothesis?

Take, first, the case of the sea-urchin. It is found that during and after treatment the nucleus and cytoplasm of the eggs undergo changes, comparable with those occurring at fertilisation. Asters appear at one of the nuclear poles and the centrosomes divide, their halves diverge, and develop a typical spindle between them. During these changes the nucleus enters the prophase of division. The chromosomes are formed, and, on the disappearance of the membrane, take up an equatorial position on the spindle, separate, and pass to each pole. Thus, each daughter nucleus has the half number of chromosomes (haploid), and this reduced number is retained at least through the early succeeding divisions. Though this was at first denied, later investigations have shown beyond doubt that it obtains. Thus, the parthenogenetically produced sea-urchin is, in respect of its nuclei, comparable with the males of the Hymenoptera. If, then, further work shows that only males are produced from treated sea-urchin eggs, the explanation offered for the bee holds equally well here. If, on the other hand, it is found that female sea-urchins can also develop, it may be explained on one of the lines I suggested in an earlier paper for the case quoted by Jack and mentioned above.

(*a*) There may be two maturation divisions, but both equatorial, as in some of the sawflies, in which case there would be no chromosome reduction.

(*b*) There may be but one maturation division, which is not reductive.

(*c*) There may be non-disjunction of the sex-chromosomes, if such exist.

This phenomenon of non-disjunction was suggested by Bridge, as a result of work on Drosophila. He found that occasionally the sex-chromosomes of the egg stuck together at the maturation divisions, both being extruded with the polar body, or both remaining in the female pro-nucleus, in which case the resulting animal would be a female.

Either of these three possibilities would explain the interesting case of female production in the bee or parthenogenetically produced female sea-urchin. Or, finally, if in

this latter case the number of chromosomes doubles during the later stages of development, a female might be expected to arise.

But how can one explain the two-sex production of the frog? Unfortunately, the cytological investigations on Amphibian eggs are very few, and even these are contradictory. Thus, in 1913, Herlandt[1] stated that, after pricking the eggs with a glass rod, the second maturation division was completed, leaving the pro-nucleus haploid. This was asserted earlier by Dehorne, who, however, believed that during the subsequent development of the egg the diploid condition was re-established by division of the chromosomes (auto-regulation). Brachet found the diploid number of chromosomes in the somatic cells of an eighteen-day-old tadpole, raised by means of artificial parthenogenesis, but did not determine the chromosome number before segmentation of the egg. According to Swingle[2] the diploid chromosome number is 26. The latest observations we owe to Loeb and Parmenter[3]; the former, in conjunction with R. Goldschmidt, counted the chromosomes in the testes of one fully developed parthenogenetic frog seventeen months old. Ripe spermatozoa were seen, and over twenty chromosomes found in the developing cells, thus making it probable that in this frog the condition was diploid. Parmenter investigated more fully Loeb's material, and found the diploid condition in three adult males and thirteen tadpoles. Some of the cells were undergoing maturation, and the tetrads, clearly haploid in number, appeared of the same form as in normal material. Unfortunately, the chromosome number in parthenogenetic female frogs has not yet been determined. As Herlant has shown, the nucleus of the egg immediately after treatment is haploid; the subsequent diploid condition, therefore, must arise by division of the chromosomes without division of the cell body, or by retention of the second polar body, which, according to Herlant, is not the case. Hence, one must conclude that auto-regulation occurs. It should be mentioned, in passing, that, according to Nachtsheim, this is a normal phenomenon in the somatic cells of male Hymenoptera.

Having reviewed the chief cytological investigations on the parthenogenetic eggs of frogs, the question may be asked, What light do they throw on sex-production? It must be conceded at once that no clear-cut statement can be made, especially in view of the fact that at present it is undetermined as to whether the normal frog possesses sex-chromosomes or not.

[1] Herlandt, M., *Arch. de Biol.*, **28**, 1913; C. R., *Ac. des Sci.* **158, 164**; 1914, 1917; *Bull. Scientif.*, **50**, 1917; *Arch. Zool. Exp. et Gen.*, **57, 58**; 1918, 1919.
[2] Swingle, W. W., *Biol. Bull.*, **33**, 1917, p. 70.
[3] Parmenter, C. L., *Journ. Gen. Physiol.*, **2**, 1920, p. 205.

Levy,[1] in *Rana esculenta*, and Swingle, in *R. pipiens*, describe a sex-chromosome in the male, but their accounts are most unconvincing.

Loeb, however, puts forward the following possibility to account for the dual sex-development. He assumes that the female cannot be regarded as homozygous for sex, since in that case the male would not possess the diploid number of chromosomes. Though this seems a likely assumption, it should be pointed out that since the *exact* chromosome number was not determined in the material examined, homozygousity of sex in the female cannot be definitely excluded. Assuming, however, that the female is heterozygous, female frogs would develop from eggs possessing the constitution 12 + X. Until, however, exact cytological evidence is available regarding the eggs from which females develop speculation is useless. Further, the frog is a peculiarly difficult, and in some ways unsuitable, animal for experimental work on sex-determination; for Hertwig,[2] among others, has shown that changed conditions can, to a large extent, affect the sex produced. Thus, in one of his investigations, a frog was allowed to lay a few eggs normally, after which she was removed and kept from the male for sixty-four hours with the remaining eggs in the oviduct. These were then fertilised by the same male. The first batch of eggs gave a normal sex ratio, but the second batch gave a great preponderance of males (700:100). A confirmatory experiment by Kuschakewitsch, where the eggs were retained in the oviduct for eighty-nine hours, gave a culture consisting entirely of males. Control experiments showed that this was not due to selective mortality of female ova or to over-ripeness of the spermatozoa. Hertwig also showed that external conditions could affect the sex ratio. Fertilised eggs kept at 30° C. gave 344 males and 319 females; but eggs kept at 16°–18° C. gave 260 males and 85 females. It was suggested that over-ripeness of the eggs might cause the abnormal extrusion of the female-determining factor—an X chromosome—thus bringing about excessive male production.

Morgan,[3] however, put forward the alternative suggestion that the over-ripe eggs may develop parthenogenetically, either by the egg-nucleus undergoing segmentation, or by the sperm nucleus alone, giving rise to the segmentation nuclei. Such a possibility is shown by the investigation of Oscar and Gunther Hertwig,[4] who showed that sperm nuclei, after treatment with radium, can give rise to the nuclei of an embryo.

[1] Levy, F., *Arch. Mikr. Anat.*, 2te Abt., **86**, 1915, p. 85.
[2] Hertwig, R., *Biol. Zentrabb.*, **32**, 1912, p. 1.
[3] Morgan, T. H., *Physical Basis of Heredity* (Lippencott, London).
[4] Hertwig, O. & G., *Arch. f. Mikr. Anat.*, **77**, 1911.

The comparative ease, therefore, with which the sex ratio of the frog can be affected by external condition renders it difficult to draw deductions as to the cause of dual sex-production from parthenogenetic eggs. There is no doubt but that important experimental evidence as to determination of sex will result from such work, when more animals, obtained from unfertilised eggs, have been raised to maturity, and their cytology completely investigated. At present, all that can be asserted is that the scanty data does not contradict the assumption that the sex of an animal is, at least to a certain extent, correlated with the presence or absence of a particular chromosome or chromosomes.

Before closing this article reference must be made to Herlant's most suggestive work as to the factors underlying artificial parthenogenesis. According to Loeb, it is first due to a cytolytic action on the egg-surface causing nuclear division. Such action, however, leads to disintegration unless checked by a second agent—commonly hypertonic sea-water—inhibiting excessive oxidation. The further assumption is then made that at normal fertilisation the sperm introduces into the egg a cytolytic agent, inducing cell-division ; and that an inhibiting substance is also carried by the sperm.

Delage, on the other hand, believes that cell-division is a series of coagulations and liquefactions of the colloidal protoplasm, and that the agents used to cause artificial parthenogenesis simply set in motion this series of coagulations and liquefactions. One of the weak points of this theory is that such cyclic changes have never been satisfactorily demonstrated—a criticism which, to a less extent, applies to Loeb's speculations.

A third theory, analogous to that of Delage in that no postulation is made as to the introduction into the egg of definite chemical substances, assumes that the mechanism of division is latent in the egg and is set in action by substances altering the permeability of the egg-membrane. Lillie therefore believes that at fertilisation there is initiated altered conditions of interchange of diffusible substances and ions, through rhythmical changes in the permeability of the cell-membrane.

These theories, and others not mentioned, are admittedly highly speculative, and too purely physico-chemical to be completely satisfactory. Herlant's work is therefore particularly interesting in giving a different view-point of the problem, and, though his conclusions are no more completely satisfactory than those of other workers, they are not less important.

Herlant maintains that all methods of inducing artificial parthenogenesis are such that either the effective volume of the cytoplasm is reduced or else the size of the division spindle

is increased, so rendering possible the commencement of segmentation. Working on frog eggs, he found that, after pricking, the second polar body was given off, leaving the nucleus haploid. Nuclear division then occurred, which *in all cases was not followed by cytoplasmic segmentation.* After two or three such divisions, the egg became polynuclear and died. If, however, a little blood was introduced at the time of pricking normal segmentation ensued. Bataillon had also made this discovery, but concluded that the introduced leucocytes acted as catalysers. Herlant, however, finding that around each leucocyte a small aster developed, pointed out that by these asters the volume of the cytoplasm, over which the spindle had to take control before cell-division could occur, was reduced. From such experiments he was led to the view that whenever the nucleus is a " monocaryon " (haploid) the intervention of a second factor, after that causing activation, is necessary for development ; but when the nucleus is an " amphicaryon " (diploid) a second factor is not requisite. Thus, in the frog's egg simply pricked, without the introduction of blood, the spindle becomes deeply implanted in the midst of an enormous " female energid " (centrosome), and is incapable of controlling effective segmentation. But in the egg pricked and treated with blood or lymph, the accessory asters (energids) so produced set up a dynamic change in the egg-cytoplasm rendering normal segmentation possible. Therefore, " in fertilisation the male pro-nucleus (apart from heredity) plays an important rôle by its union with the female pro-nucleus ; for it re-establishes the normal relation between the nuclear mass and the volume of the egg." It will be noticed that Herlant's views are very similar to those put forward by Hertwig, and generalised in the well-known expression " Kermplasmarelation."

Finally, a few facts put forward by Herlant in support of his theory may be cited. It is well known that the egg of the sea-urchin requires two factors to bring about development, and correlated with this the egg-nucleus is a monocaryon, due to maturation having taken place before treatment.

In the starfish, on the other hand, maturation occurs after the egg is laid, and it is possible for the experimenter to treat such eggs either before or after the reduction division. Delage found that treatment with the second agent could be omitted, provided that maturation had not taken place (nucleus diploid) ; if, however, the treatment was delayed until the nucleus was a monocaryon (haploid) then the second factor was necessary.

Again, in those cases where an egg is activated by foreign spermatozoa the general principle appears to hold. Thus, in the sea-urchin egg, activated by Mytilus sperm, the male nucleus takes no part in mitosis. The egg-nucleus is therefore

a monocaryon, and development will only continue if the egg has been treated with hypertonic sea-water (the second factor).

In fertilisation of Sphærechinus egg by Chætopterus sperm the two nuclei unite, but *before* mitosis the paternal chromatin is extruded; here, again, treatment with hypertonic sea-water is necessary for development.

Cases are also known where the action of a second factor is not necessary, as, for example, an Echinus egg fertilised by sperm from Andouinia; where, in spite of the great difference in the parents, the two nuclei remain as an amphicaryon for a long time.

An intermediate condition is found in the egg of Strongylocentrotus fertilised by Sphærechinus spermatozoa: the paternal chromatin is extruded at the metaphase of division, but, as mitosis commences with an amphicaryon nucleus, no second agent is required. Finally, Baltzer showed that when a Stronglyocentrotus female is fertilised by Arbacia sperm development proceeds normally until the amphicaryon nucleus becomes monocaryon by extrusion of chromatin. This stage marks a crisis in development, which, in many cases, leads to the death of the embryo.

It will be obvious from the foregoing that Herlant's conclusions are exceedingly suggestive, and further show that the detailed cytological study of artificial parthenogenetic eggs will yield results of wide interest to experimental biologists.

THE APPLICATION OF PHYSICAL METHODS TO PHYSIOLOGY

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RECENT work on the nature of the atom has obliterated the dividing line between physics and chemistry: for practical purposes the twin sciences will continue to be housed in separate buildings, and discussed in separate text-books, but, as regards the ultimate reality behind them, they rest unquestionably upon the same foundations. In the following pages, therefore, the word "physical" will be held to imply all those methods of observation and reasoning which are common to the exact sciences. On the biological side the trend of botany and zoology towards the study of function, as well as form, the growth of pathology as a laboratory science, and the formal recognition of experimental medicine, have tended similarly to break down the barriers between these sciences and physiology: in the following pages, therefore, we shall use "physiology" to denote the study of all the normal processes of life, of the phenomena attending and of the mechanisms underlying them, and of the co-ordination of those mechanisms.

The word "mechanism" will at once evoke the echoes of a controversy. It is believed by some that the processes of the living creature are unlike those of the non-living world, in kind not merely in degree, and that the use of the word "mechanism" as descriptive of them is a false metaphor involving an unjustifiable assumption. It is believed by others that, with the progress of knowledge, the living creature will be proved to be nothing but a complex system of co-ordinated mechanisms, working according to the exact laws governing the exacter sciences. The dispute between "vitalism" and "mechanism" is one where no conclusion can be reached, but it serves, and has served, a useful purpose in calling attention to the very fundamental nature of the problems with which physiology has to deal, and in showing the necessity to a physiologist of a due acquaintance with the other sciences. The living creature lives in a physical universe, bounded by physical barriers, dis-

obeying no known physical laws, dependent from moment to moment on the continuance of certain physical conditions. Want of oxygen will destroy in a few seconds all the phenomena of consciousness; lack of the chemical secretion of the thyroid gland will turn an intelligent child into an idiot; a 10 per cent. change in the absolute temperature will destroy the labile physico-chemical structure of the medium in which life works; an appropriate salt solution will cause an egg-cell to develop without the intervention of a male; and the means by which the physiological activities are expressed, and the psychical activities are communicated, are exclusively physical in nature. Clearly the physical is a very fundamental aspect of the living creature. On the other hand, it is difficult even to conceive the possibility of an instrument, a mechanism, capable of being designed in the physical or engineering sense, capable so to speak of being described in a patent specification, which would possess in the least degree either the human properties of consciousness or emotion, or the powers of purposive adaptation possessed even by the humblest living cell. To many minds there is something of a different kind, not merely of a different degree, in the living creature. The most fundamental, the most insoluble, the most prolific of all the problems of physiology are these: where (if anywhere) do the physical aspects of life end? where (if anywhere) do we come up finally against the fundamental biological and spiritual aspects?

It is notorious that all attempts to explain the whole mechanism of any characteristically vital process by a physical hypothesis have failed. Physical science, however, is still young; the most fundamental of its theories, the laws of motion, of the conservation of energy, of the conservation of matter, and our whole belief in a simple, infinite, mechanical universe moving along in a steady stream of absolute time, are being successfully assaulted, and shown to be nothing but accurate first approximations to the truth. The concepts of physics are changing, in kind, not merely in degree. Moreover, the practical results, the instruments, and the achievements of physical science are growing daily: technical improvements in electrical instruments have opened up fields of which the greatest of our fathers never dreamed. It is not reasonable, then, to demand that the physics of to-day shall be adequate to explain the phenomena of life. Rather should we learn caution from these failures of the past, and avoid the grandiose method of attack, based upon an insufficient appreciation of the strength of the position occupied by the mysteries of life.

Progress in physiology will come from adopting physical methods of investigating the physical manifestations of life, not from framing physical theories of things imperfectly under-

stood. In the past, mistakenly believing that the concepts of physics had a finality not allowed to those of biology, the physiologist has tended to put forward theories of vital processes based upon an imperfect knowledge both of physics and of the physical accompaniments of the vital processes themselves. When amino-acids had been combined to form polypeptides, some ardent spirits began to acclaim the synthesis of the protoplasm of which the living cell is made, little heeding the fact that all that had been achieved was the synthesis of something not unlike the bodies of which the cell is made *after it has been killed.* Progress has not come in that way, but rather from the laborious process of cleaning up by physical methods, slowly and laboriously, one corner after another of our ignorance of the physical causes, or accompaniments of vital activity. We can safely say that the physics of to-day has not "explained," will not "explain," any of the more characteristic of the phenomena of life; whether the physics of to-morrow will explain them only to-morrow will decide. The attempt, however, has been valuable, in so far as it has made clear where and how physical science can help in elucidating the mysteries of the living creature. Advance will come from investigating the physical causes, the physical accompaniments, and the physical products of vital activity with all the tools, mental and material, of the exact sciences. We do not know where such advance will lead us—if we are wise we shall not pretend to know; but we may be sure that, if we follow the main roads built by the exact sciences, we shall go straighter to our goal than if we take to short-cuts of our own choosing.

The living creature is much more difficult to investigate than the non-living, chiefly because its continued normal existence is possible only between very narrow limits; the living creature, moreover, like an eddy or a wave, is a process and not a thing, and it is difficult to maintain the stability and constancy of a process under any kind of experimental manipulation or procedure. Thus it is that the progress of physiology has depended largely on that of instrument design, by which instruments have been made to suit the living tissue which could not be coerced or adapted to existing ones. Indeed, it is remarkable how physiology has attracted, or produced, such men as Einthoven, Keith Lucas, or Bull, with a genius for designing instruments for specific purposes. Moreover, modern instruments and instrumental methods—the ultra-microscope, the Röntgen rays, the sensitive galvanometer, the high-frequency current, the electrical measurement of chemical, thermal, or mechanical changes, and the systematic employment of photographic methods of recording—these have led to a far higher degree of certainty in following, and in analysing the physical

manifestations of vital phenomena. This is all pure gain—we are beginning to understand more precisely what are, in space and time, in intensity and degree, the physical *accompaniments* of the process we call life.

Side by side with the development of instrumental methods has gone the development of biochemistry. The precise methods of analysis and synthesis employed by the organic chemist have been applied successfully to the ingoings and the outcomings of the living cell and of the chemical bodies of which it is composed. There are still unknown factors in our food, in the air we breathe, and in the chemical bodies we excrete; but, on the whole, the study of the imports and exports of the body is well advanced. Our knowledge, however, of the intervening chemical stages of the process, of the intermediate "metabolism" of the cell, or plant, or animal is as yet in its infancy, and the results achieved are more suggestive than conclusive. The essential difficulty of the study is that the processes of life take place, not in a homogeneous system, not even in a heterogeneous system of a simple kind, but in a tiny complex structure possessing spatial as well as chemical and physical properties—spatial properties which render the ordinary "mass" methods of the chemist incapable of following the finer details of the process. Doubtless great advances are still to come in physiology from such chemical methods, but if we reflect that the living cell is a process and not a thing, and that the sequence of events in a process may well elude the study of the medium in which the process works, we may expect advance to become more rapid with the adoption of micro-chemical or physical methods more capable of coping with the spatial difficulties of the problem. One does not know when or where such methods will arise; there is no doubt, however, that—like the chemical molecule—the living cell and its processes exist in space as well as time, and in a space so small that none of our ordinary means are adequate for its investigation. It would seem likely that the methods by which the spatial structure of the crystal, the molecule, and the atom are being exposed by recent physical investigation will be required before the mechanism of the life processes can be followed in detail. The most fundamental advance, therefore, in biochemistry probably waits on further discoveries in physics.

A third line on which great developments in physiology have been made in recent years is in the study of colloid chemistry, and of the physical and electrical properties of solutions. The medium in which occur the processes constituting life has certain characteristic physico-chemical properties, and many of the manifestations of life depend upon the colloidal nature of the protoplasm, and of the fluids

surrounding the living cell. The permeability of colloidal membranes and the effects of salts and electric forces upon them; the electric charges on colloidal particles, and the aggregation, precipitation, or dispersion of such particles under the influence of physical or chemical agencies; the phenomena of surface tension, of adsorption, of katalysis in colloidal systems—all these are of surpassing interest to the physiologist in his study of the fundamental nature of the physical accompaniments of vital activity, but of a complexity still unrelieved by any really satisfactory generalisation, enabling him to understand their behaviour. Why is a precise balance of various salts so essential to the living creature? What processes, chemical or physical, underlie the phenomena of immunity, of hæmolysis, of agglutination? What is the nature of the electric change which is the only known accompaniment of the transmitted nervous impulse? What—even in the most general way—is the mechanism by which chlorophyll stores up the energy of sunlight? Why do two bodies, almost identical in chemical and physical properties, have so completely different effects upon the living cell? All such questions—questions of the most general interest and of the most fundamental application—are at present unanswerable. They will be answered only by a more searching analysis of the geometrical, as well as of the chemical and physical, properties of the living cell, and of the colloidal and chemical bodies occurring in its structure. Colloidal chemistry is an attempt to deal with the individual, as well as with the collective and average properties of particles of matter, and of the molecules, atoms, and electrons which compose them; such individual treatment is an essential step in the study of the mechanism of the living cell.

Up to recent times it was customary to adopt a philosophical and non-committal attitude towards the molecule, the atom, and the formulæ of stereo-chemistry. Although one might describe the behaviour of the molecule by means of a structural formula, this formula might be nothing more than a working hypothesis very far removed from the real facts. The evidence for the structural formula was indirect—it involved no direct use of spatial measurements. To-day the position has changed, and convincing evidence for the real spatial existence of the molecule, and of the atom and its parts, has been produced. The laws of aggregates, such as the Gas Laws, the Second Law of Thermodynamics or the chemical Laws of Mass Action, might enable one to evade the mechanism, and to obtain results true on the average and in the mass; but the mechanism was still there—in space as well as in time—and such laws had no application to it. If, then, the atom, the molecule, the colloidal particle, have a spatial existence, we may expect their properties—their

individual chemical and physical properties—to depend largely —if not entirely—upon the number and distribution of the electrons making them up. The increased intellectual respectability of the structural formulæ of chemistry has reacted upon physiology, and most physiologists to-day would be prepared to believe in a spatial distribution of the parts or processes of the living cell ; the days of a homogeneous, undifferentiated protoplasm with certain mysterious labels called " properties " are gone. Clearly, therefore, the spatial characteristics of the processes of a living cell are related to the spatial characteristics of the atom, molecule, and particles concerned, and will be understood only when the latter are understood, and when the methods by which the latter were discovered are applied to the living cell. The living cell has still many secrets to yield to the older mass-methods of physics and chemistry, but the most fundamental advance waits for means which can take account of the distribution in time and space of the parts and events concerned in vital processes. At present one is forced to infer the mechanism by analogy from mechanical systems, showing the same general physical features.

The laws of physics come under two main categories, those which are true—within limits—of all systems, large or small, and those which are statistical in nature and true only of systems large enough to allow the laws of aggregates to apply. Many of the most fertile generalisations of physics, such as the Second Law of Thermodynamics, the Gas Laws, the Laws of Osmotic Pressure, and the Laws of Mass Action, are of the latter restricted kind, applicable only to systems where the number of parts is so large that the laws of averages work out correctly. A particle showing Brownian movement is already beginning to be too small to obey these statistical laws, and the whole system of co-ordinated mechanisms of many living cells is confined within a space no larger than that of a particle showing vigorous Brownian movements ; we may well doubt whether the laws of averages will always work out correctly in such a system. It is possible therefore, indeed probable, that, in the organised system of minute mechanisms making up the living cell, such statistical laws will not, in the last resort, be found to hold. The laws of mass action apply certainly to the case of a ferment action occurring in a large homogeneous system : the number of working parts is so large that the statistical rule works out correctly. But in the living cell, where a single ferment molecule or particle may be an essential link in the mechanism devoted to some specific function, there is no *à priori* reason why such a statistical rule should hold at all. Indeed, it would seem likely that an explanation of the behaviour of the living cell will finally have to rest on

an individual treatment of the particles, molecules, and atoms, and on those physical generalisations which are true not only of the mass, but of the units composing it. If this line of reasoning be correct, all kinds of interesting speculations arise as to the behaviour of a system in which the statistical rules do not apply, and it may be possible some day, by direct experiment on living creatures, to prove or disprove the applicability of such rules. If, for example, a living cell were shown to provide at constant temperature more free electrical, or mechanical, energy than was accounted for by the free energy of the food-stuff used, it would be evading a statistical rule, the Second Law of Thermodynamics. This possibility may seem far-fetched, and it is probably better—as a matter of expediency, until the assumption is disproved—to assume that the statistical rules of the exact sciences do apply to the living cell with the same rigidity as do the laws of a more general nature. But it will be wise to bear in mind that possibly the fundamental difference between the living and the non-living world—a difference obvious enough, though difficult to fit into a mechanistic philosophy—may be simply that the statistical rules governing the non-living, *i.e.* the non-organised, do not necessarily, and under all conditions, apply to the minute organised mechanisms employed by the living cell. We are reduced, therefore, by these considerations also to a study of the individual properties of the particle or molecule, as the means whereby physiology is most likely to advance along physical lines.

The living tissues of whose physical and chemical properties —in relation to function—we know most are the striated muscle cell and the medullated nerve fibre. There is no peculiar virtue in the investigation of these tissues, except that—more than others—they allow one to obey the foremost of all working rules and to "isolate one's variables." Generations of physiologists have applied themselves to the investigation of the properties of muscle and nerve, following a sure scientific instinct, for with these highly differentiated cells, showing sharply defined physical characters, it *is* possible to isolate one's variables and to investigate them separately in a manner quite impossible in what some people regard as the simpler "general utility" cell. In many respects all living cells behave according to the same general rules, and the relations established for muscle and nerve may often be applied, *mutatis mutandis,* to other cells and organs. For example, it has been shown that a muscle, when excited, liberates energy from a "ready store" of some kind, using oxygen only in the slower and secondary processes of "recovery." This general statement probably applies to the conduct of all living cells. Or, again, the excita-

bility and the conductivity of nerve may be shown to be two distinct and separable processes, the former due to a local change at the seat of excitation, the latter to a wave propagated with a finite and measurable speed; the continued normal behaviour of a muscle or nerve has been shown to depend upon a precise adjustment of the hydrogen-ion concentration and an accurate balance between various metallic ions in the fluids bathing it; the change of form produced by contraction has been demonstrated to be associated with the liberation of lactic acid from glucose in the interior of the muscle cell: all these phenomena probably have their counterparts in other tissues. Thus, apart from their intrinsic interest, the investigation of these two very important tissues is likely to lead us to reliable conclusions as to the behaviour of living cells in general. Nearly all living cells show the phenomena of excitability—these cells show it in a highly specific form. We know as yet very little about the mechanism by which excitation occurs, but it would appear that in some sense all forms of excitation are really electrical, or ionic, in nature, and set up a propagated disturbance, whose only known accompaniment is an alteration in ionic permeability associated with the electrical change, and due possibly to some kind of transmitted upset of the orientation of the protein molecules of the nerve sheath. Various characteristics of the excitatory process and of the propagated disturbance are known—characteristics established by careful and often very beautiful physical experiments—but there is little doubt that much still remains to be done, simply in giving a clear description—apart from any theory—of the actual physical facts accompanying them. The welding, co-ordinating, and modifying influences of the central nervous system on such impulses must obviously remain a mystery so long as the actual physical accompaniments of the impulses are not known. The physical theory of the nature of the impulse will probably require considerable improvements in knowledge, both of molecular physics and of colloidal chemistry, for its description.

More is known about the physical accompaniments of the activity of a muscle cell. It shows the same phenomena of excitability as a nerve, but in addition to these certain chemical, physical, and mechanical changes occur whose investigation has given us, partly by analogy, an extensive insight into the nature of the internal mechanism of the cell. The muscle twitch, the fundamental unit on which all muscular action is based, consists of a temporary change in the elastic properties of the fibre, accompanied (if the fibre be allowed to shorten) by the production of mechanical energy, and by the evolution of heat, the using up of oxygen, and the production

of carbon-dioxide and lactic acid. The twitch of a muscle fibre cannot be varied in strength by varying the stimulus—its action is "all or none." The heat is liberated, partly in the phase of contraction, partly in the phase of relaxation, and in large measure—in the presence of oxygen—in the "recovery" phase. It appears that the energy exchanges of a muscle are like those of an accumulator employed in driving an electric motor: on applying the stimulus, stored chemical energy is discharged, heat and work are produced, and in the phase of recovery the system is recharged at the expense of some oxidative process. The elastic changes are due to chemical reactions, being largely affected by temperature or by chemical substances in contact with the muscle. It would seem that the stimulus causes a momentary alteration in the permeability of some surface in the fibre, an alteration accompanied by the electric change. This momentary change of permeability allows lactic acid (prepared from sugar or glucose under the influence of a katalyst) to pass out, and by some physico-chemical action to increase the tension of certain longitudinal surfaces in the fibre. In relaxation the acid is removed from these sensitive surfaces by some reaction unknown; in recovery part of this lactic acid is oxidised, and the remainder is restored to its previous position as the sugar or glycogen from which it came. The ordinary voluntary contraction is the summed effect of a series of such individual "twitches," graded and co-ordinated by the nervous system.

The application of physical methods to the heart is another field which has proved very fertile of recent years. Our present knowledge of this organ is due largely to the use of the string galvanometer, an instrument designed specifically for recording the very rapidly changing electric currents produced by the beating heart, and since employed for such diverse purposes as locating enemy guns or recording the movements of the hand. By connecting the galvanometer either directly on to an exposed and beating heart, or indirectly to the same tissue through the intact conducting body of a human subject, the passage of the wave of contraction over the heart has been recorded, and its characteristics in health and disease described. The spontaneous activity also of the isolated heart has been the chief means by which a preliminary account has been given of the effect of various salts upon the living cell, and the same organ has contributed considerably to our knowledge of the oxidative and chemical processes occurring in activity and of their relation to the mechanical changes produced.

In dealing by physical methods with the intact animal, as distinguished from the isolated tissue, perhaps the most striking

advance of recent years has occurred in connection with respiration and the respiratory functions of the blood. The reactions of hæmoglobin with oxygen in the presence of salts, acids, and carbon-dioxide have exhibited the most striking adaptation to the needs of the animal under a wide variety of conditions ; in fact, seeing that a chemical compound is a fixed and not an adaptable thing, one might almost say that the evolution of the animal in its present form has hinged upon the physical properties of hæmoglobin, as that of the plant has depended upon those of chlorophyll. The physical properties and reactions of these two pigments are still only imperfectly understood, and progress in the studies associated with either depends to-day more on progress in knowledge of the physico-chemical basis of their reactions than on any biological factor. The hæmoglobin and the chlorophyll reactions are good examples of the way in which physiology is dependent on physics and chemistry for all possibility of further advance. The CO_2-carrying power of the blood, and the exact regulation of the respiration and of the hydrogen-ion concentration of the blood by the respiratory centre and kidneys, are also examples of the way in which exact methods are exposing the exquisite co-ordination of the body, a co-ordination physical in its precision and mechanism, biological in its complete adaptation to the needs of the animal.

The growth of biochemistry in the last twenty years has opened up a field of work whose limits are seen to widen yearly, both from the point of view of fundamental knowledge and more especially from that of practical importance. The achievements of biochemistry however, great as they have been, have only served to emphasise our extreme ignorance of the actual mechanism by which the chemical activities of the animal are conducted, and to show the vast importance of the things still remaining to be done. The synthesis, by chemical means, of the organic compounds occurring as a result of the activity of the living cell, and the discovery of the chemical nature of other such substances not yet synthesised, have proceeded side by side with the realisation of the importance to the animal of various chemical bodies—the vitamines—still unidentified. The "chemical correlation" also of the functions of the body, though undoubtedly an essential part of the animal's economy, depends mainly upon chemical substances still unknown, and upon reactions with them of unknown elements in the cells of the various organs. Pharmacology, the science of the action of drugs upon the normal and the abnormal tissues, remains an empirical science, no clue practically being available as to the mode of working of the active drug, nor as to why small changes in its chemical or

physical nature may completely alter its effect. The study of immunity also—a matter of the most profound practical importance and of the greatest theoretical interest and significance—continues to lack any really satisfactory chemical or physical principles to explain, or to correlate, the very complex series of reactions undoubtedly occurring in the chemical interplay between the invading organism, the body fluids, and the body cell. The facts of pharmacology, of biochemistry, and of immunology all suggest the existence of substances with a highly specific chemical structure, fitting into some portion of the chemical machinery of the living cell in the same way as a key fits into a lock or a bolt fits into a nut; but our knowledge of the chemical structure of the living cell, as distinguished from that of the products of its activity, disintegration, or destruction, is still so imperfect that we have at present no sure evidence as to the nature of the reactions in which chemical "messengers," drugs, vitamines, or the various factors in immunity, play their diverse parts. The most hopeful method, at present available, of investigating the chemical structure of the living cell itself lies perhaps in employing the highly specific nature of the reactions associated with anaphylactic shock, on the lines of the suggestive and beautiful work of Dale and his colleagues. The attempt, in any case, to advance by chemical methods, even though only partially successful, has at any rate made us realise the necessity of further knowledge of the internal chemical machinery of the cell, and of the organisation and distribution of that machinery, and, since to realise the existence of a problem is to go half-way to solving it, while to state it in precise terms is to go still further, we may look forward hopefully to the day when the problems of biochemistry, of pharmacology, and of immunity will be susceptible of at least as exact a treatment as is being applied now to the elucidation of the structure of the crystal or the atom. That day may be distant, but when it dawns it will give mankind a tool surpassing all the dreams of the alchemist.

POPULAR SCIENCE

PHYSICS IN WARFARE

FINDING GUNS BY PHOTOGRAPHING SOUND

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Sound-ranging was invented during the Great War for the purpose of locating hostile batteries. It will be shown in the sequel how remarkable was the degree of accuracy attained.

Other methods had previously been used for finding the enemy's guns. These were often liable to a fairly large error, but they continued to be employed right up to the end of the war, often as a check on the positions given by the sound-rangers.

Of these methods, two call for special mention, viz., "Flash Spotting" and Aeroplane Photography. The "Flash Spotting" was carried out by observation groups, special branches of the Royal Engineers formed for this purpose, numbered consecutively from one upwards. The principle underlying their work is that if a flash be observed from two or more stations, and the corresponding bearings be noted, then, when these bearings are plotted on the map, they intersect in a point which gives the location of the gun.

In practice, however, it was found that these lines intersected at an extremely acute angle, so that the position of the gun could be determined with but a fair degree of certainty. The reason for this is that the stations from which observations were taken were always fairly close together when compared with the distance of any of them from the gun, which was situated well behind the enemy's lines.

In theory, aeroplane photography should give the position of a gun with the utmost degree of certainty. In practice, however, the objects appeared so small, even when viewed through a strong magnifying-glass, and were so well camouflaged, that it was often impossible to say positively that the given object was a battery. But if it was known by the sound-

ranging results, or otherwise, that a battery existed in a given small area, then it was often possible to say definitely that it was situated at a particular point.

To ensure a much greater degree of accuracy in these locations, sound-ranging was invented. The French commenced experiments in October 1914. The British took it up, and at the end of 1915 established an experimental section at Kemmel Hill, just south of the Ypres salient. This proved so successful that other sections were formed, and in 1916 two of them were attached to each of the British Armies on the Western Front. Their value exceeded all expectations; other sections were formed, and in 1917 and 1918 there were more than thirty on the Western Front and about half a dozen in Palestine and Salonika.

The whole system of sound-ranging is built up on the simple fact that sound travels comparatively slowly. Its speed is really very great compared with the ordinary standards of daily life, and is about 1,120 feet per second at ordinary temperatures; but when compared with the speed of light, which is 186,330 miles per second, it is almost inappreciable. The time taken by sound to travel some distance from the source can be measured fairly accurately. That it does take some measurable time is proved by many common daily observations, such as, for instance, hearing an engine whistle after the escape of steam is seen. If, therefore, the time taken by sound to travel from its source to some observing stations be noted, the position of the source can be calculated.

In warfare, it is not possible to make such observations accurately, but accurate observations can be made of the differences in the times taken by sound to reach the various stations, and on these differences the system is based.

To appreciate the principle fully, let us consider for the moment the beautiful curve known as the ellipse. If E and F be the foci and P any point whatever on the curve, then it is known that the sum of the distances EP and FP is a constant. This, indeed, is the property made use of in drawing the ellipse by the common method of two pins and a piece of string.

The hyperbola, on the other hand, has the property that the difference of the distances EP and FP is a constant. It follows, therefore, that if the constant difference be known, a series of points P can be found, and so the hyperbola constructed. This fact is fundamental in sound-ranging. A gun is fired from some unknown point behind the enemy's lines. The times at which its report reaches two observing stations, E and F, are noted, and so the difference is calculated. This enables us to say with certainty that the gun must lie at some

point on the hyperbola which has these stations, E and F, for foci and is such that $EP—FP$ = the observed time difference.

If, now, a third station be set up and its time reading be taken in conjunction with one of those already used, a second hyperbola can be constructed upon which the gun must also lie. The weapon must, therefore, be at the intersection of the two curves, and so its position is determined.

In practice six such stations were employed, and were arranged in general on the arc of a circle just behind the front lines. They were situated about a mile apart, so that sound would take something like four and a half seconds to travel directly from one to another.

The accuracy of the results does not depend on having a very large number of observing stations close together, but on having a few stations separated as widely as possible, so as to give a large time difference. The accuracy of the whole system depends, indeed, on the correct measurement of these time differences. Mechanical means have to be employed, for human agency cannot be relied upon. It is found in practice that from one-twentieth to one-fifth of a second elapses between the time a man hears a sound and the time he presses a button to record it, and, moreover, that this quantity varies from man to man, so that no satisfactory record can thus be made when the time differences are required accurately to the order of about one-hundredth of a second. This difficulty was overcome in the British Army by the invention of the Tucker microphone, an instrument that records the arrival of the sound instantaneously. The details of the constructing of this instrument cannot be published, the officers and men of the sound-ranging sections having been pledged to secrecy.

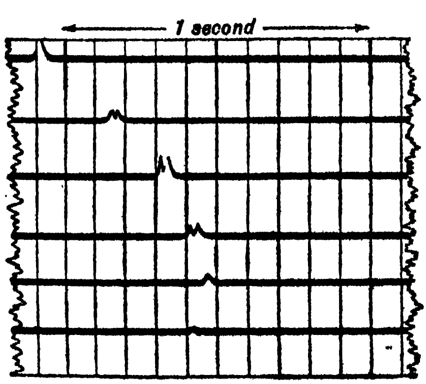

Fig. 1.

The record made by this instrument took the permanent form of a photograph of the shadows of the strings of a very sensitive galvanometer, the arrival of the sound being indicated by a "kick" on the cinema film which was used for this purpose.

The film is graduated in hundredths of a second of time (tenths are shown in Fig. 1), and therefore can be read to one-thousandth part of a second, the graduation marks being made by the flickering of a light caused by the rotation

in front of it of a specially constructed cog-wheel, controlled by a tuning-fork. This piece of apparatus will be considered in greater detail later.

The horizontal lines correspond to the six recording stations in the field, each of the strings thus photographed being connected to a microphone. The time differences, therefore, in the sound reaching the stations are simply read off from the film. In Fig. 1 this difference for the first two stations is about twenty-four hundredths of a second, and for the second and third about sixteen.

These differences are then corrected to allow for the velocity and direction of the wind, and for the temperature at the time, both of which affect the actual velocity of sound. They are then transferred to a map of the district, around the edge of which a specially constructed scale is drawn. In practice, the guns that had to be located were so far away, compared with the distance between the microphones, that it was often found sufficiently accurate to plot the asymptote of the hyperbola instead of the curve itself, and even when this was not the case, a suitable correction from previously calculated tables could be applied. This made it possible to work with a straight line instead of with a curve, which would have been troublesome to construct. The line was actually laid down on the map by means of a fine thread which was pivoted at a point midway between those marking the positions of the two microphones in question and which was placed on a suitable reading on the scale round the edge. The four other strings were similarly placed, and the point at which they intersected gave the location of the gun.

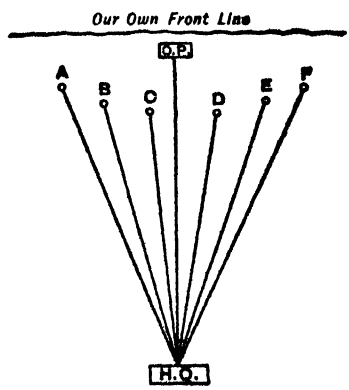

FIG. 2.

We must now consider the mechanical means adopted to make this photographic record of time differences that we have been discussing—means that illustrate the resources of modern physics.

Let *A*, *B*, *C*, *D*, *E*, and *F* (Fig. 2) be the six observing stations to which reference has already been made, at each of which a microphone is placed. These stations were situated just behind the front line, and were frequently arranged on the arc of a circle for convenience, although this condition is not essential. Each station was connected electrically to the headquarters of the sound-ranging section, marked *H.Q.* in the figure, and situated about 3,000 to 4,000

yards behind the most advanced trenches. Close to the line and in front of the microphone base was a dug-out, known as the observation post; this was also connected electrically to *H.Q.* Two trained observers were stationed there, and they controlled the recording apparatus at *H.Q.* They were sufficiently in advance of the microphone base to enable them to hear the report before it actually reached the microphones, and to start the film running by the time the microphones were affected.

To understand the method of working we must consider a most beautiful device in electricity known as the Wheatstone Bridge. This is, in effect, simply an electrical balance; it consists essentially of four arms containing resistances, the slightest variation in which can be detected by a sensitive galvanometer, which takes the place, in this electrical arrangement, of the pointer in the ordinary balances used for weighing. If the resistances in the four arms be P, Q, R, and S, respectively, then it can be shown that when they are adjusted in such a way that $P : Q = R : S$, then no current at all will flow through the galvanometer, since perfect balance is attained.

If, however, the resistances are altered so that this relation is no longer true, then a current will flow and the needle of the galvanometer will be deflected.

In sound-ranging there were six distinct Wheatstone Bridge arrangements, one for each microphone. The microphone in question, together with the wire connecting it to *H.Q.*, often some miles in length, was made to correspond to one resistance, P, whilst the other three, Q, R, and S, were at *H.Q.*, and were adjusted to complete the required balance.

In practice it was found that the microphone took an appreciable time to heat when the current was switched on, so that the balance of the bridge was upset. This difficulty was met by putting a second microphone in the second arm of the Bridge, so that its fluctuations should correspond more or less to those of the microphone in the field.

The galvanometer then remained appreciably steady, and gave a continuous line as its record on the film.

But when the gun report reached the recording microphones it completely altered their resistance, thus causing a current suddenly to flow through the galvanometer, the string of which gave a sharp kick, which in turn was photographed as a break in the otherwise continuous line. (See Fig. 1.)

We have now to consider the apparatus. A small electric lamp was so arranged that it sent a beam of light through a case containing the time-wheel on to a galvanometer, the recording part of which consisted of six vertical strings. The shadows of these strings cast by the beam of light were re-

flected each from the inside edge of a right-angled prism, into a cinematograph camera, the positions of the shadows on the film being controlled by small adjustments of the prisms. This camera was worked by a small motor, which was in turn connected to a relay arrangement under the control of the forward observers in the observation post just behind the front line, who could therefore start or stop the film at will. In practice they started it as soon as they heard the report of a hostile gun, so that the first indication of artillery activity that the people at Headquarters had was the hum of the motor as it was switched on. A few seconds later the strings were seen to kick, and then the report was heard.

The time-wheel consisted of a disc, around the edge of which a number of little cogs were placed. It was controlled by an electro-magnet, being pivoted excentrally to it. The electro-magnet was switched on and off automatically by a tuning-fork of frequency 50, which therefore made a hundred half-vibrations per second. Matters were so arranged that the beam of light from the lamp was intercepted by the cogs one hundred times per second, so that, at these small intervals, the shadows of the cogs were photographed and gave the vertical time-markings on the film as shown in Fig. 1, which were therefore put on as the film was run through. Had they been marked on the film before it was required for use, it would have been almost impossible to have adjusted the rate of the motor sufficiently accurately to have passed the film through exactly at the necessary rate.

Sound-ranging was able to give not only the actual position of the hostile battery in the field, but was able to determine its nature, *i.e.* whether gun or howitzer, and also its calibre, *i.e.* the size of the shell it fired. We shall consider these separately.

The essential distinction between howitzers and guns is that the former throws a shell very high but to no great distance, whilst the latter fires at long range, but comparatively to no great height.

Moreover, the howitzer shell travels more slowly than sound, whilst the gun shell travels far more quickly. It is this latter distinction between the two that is utilised in sound-ranging and is actually recorded on the film.

Consider the case of a howitzer shell. Suppose the shell to be fired from the point *O*, Fig. 3, and let us consider the state of things at the end of, say, five seconds. The sound-wave of the discharge will have travelled to the extreme right position indicated, the numbers under the horizontal line giving the position of the sound-wave at the end of successive seconds, whilst the numbers above give the positions of the shell at the same times.

The shell, in passing through the air, is continuously setting up pressure waves which travel onward with the velocity of sound, and thus faster than the shell itself. It is impossible to draw all these pressure-waves, so we will content ourselves by indicating only those that are set up at the end of successive seconds. At the end of the first second the pressure-wave will originate from the point *I*, and will have four seconds left in which to travel, so that its position at the end of the fifth second will be as shown by the arc *b*, the pressure-wave set up as the shell leaves the gun being coincident with the sound-wave of the discharge report, and so represented by the arc *a*.

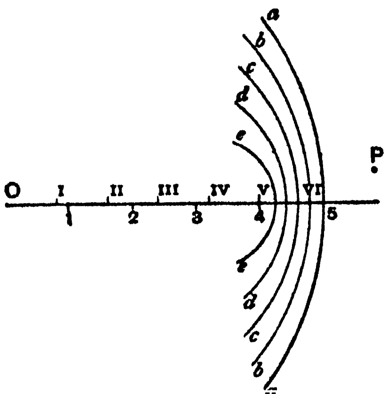

FIG. 3.

At the end of the second second the pressure-wave will originate from the point *II*, and will have three seconds left in which to travel, so that its position will be indicated by the arc *c*.

Similarly, the pressure-waves at the end of the third and fourth seconds will originate from points *III* and *IV*, and will have two and one seconds respectively in which to travel, and so at the time we are considering will be represented by the arcs *d* and *e*.

An observer at the point *P* would hear the report of the discharge, and then a continuous sound as the pressure-waves, *b*, *c*, *d*, *e*, pass him ; then would come the shell and the sound of its explosion as it burst. This continuous, long drawn-out sound was the ordinary well-known " whine " of the howitzer shell.

On the sound-ranging film this was recorded as a sharp break followed by a series of more or less wavy fluctuations, as seen clearly in Fig. 1.

The case of the gun, however, is altogether different, for the shell travels faster than sound, and so the pressure-waves caused by it arrive at any given point before the sound of the discharge, *i.e.* the report of the firing. This pressure-wave is itself received by the ear as a sound-wave, so that in the case of the gun two reports are heard. Fig. 4 will make this clear.

Suppose, for the sake of argument, that a high-velocity shell loses one-tenth of its velocity per second and leaves the gun with an initial speed twice that of sound ; its position

at the end of successive seconds will be denoted by the Roman figures above the horizontal line.

Let the equally spaced numbers below the horizontal line represent the position of the sound-wave of the discharge at the end of successive seconds.

Let us consider the state of things at the end of six seconds. The pressure-wave set up by the shell as it leaves the gun will have reached the position indicated by the arc *p*, drawn with centre *O* and radius 6. The sound-wave of the report will also have reached the same position. As in the case of

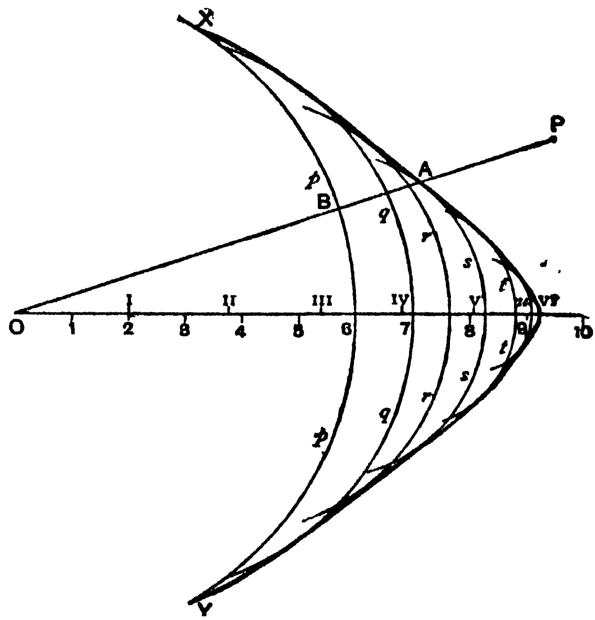

FIG. 4.

the howitzer shell, the gun shell is continuously setting up pressure-waves which travel onward with the velocity of sound, and so slower than the shell itself. It is again impossible to draw all these pressure-waves, and so, as before, we will content ourselves by indicating those set up at the end of successive seconds.

At the end of the first second the shell will be at the position *I*, its pressure-wave will have five seconds left in which to travel until the time we are considering. It will, therefore, have come into the position represented by the arc *q*, whose centre is *I*, and whose radius is 5.

In the same way the pressure-wave set up at the end of the second second will be in the position *r*, centre *II*, radius 4.

Similarly, succeeding pressure-waves will be in the positions *s*, *t*, *u*, the respective centres being *III*, *IV*, and *V*, and respective radii 3, 2, 1.

The waves we have drawn overlap at certain points. If we had drawn all the waves set up at every instant we should find a continuous overlapping, so that the true front of the whole pressure-wave is evidently the envelope of the waves we have indicated.

The apparent effect observed by a person standing at some such place as *P* is that he hears first of all a sharp crack due to this envelope, then, after an interval, he hears the sound

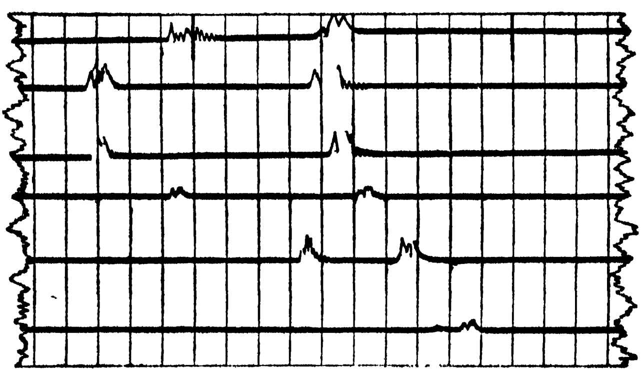

FIG. 5.

of the discharge. The sharp crack is known technically as the Onde-de-choc, and is followed by the true sound-wave at an interval represented by *AB* expressed in time. Thus, in the case of a gun, the observer hears two distinct reports, provided he is standing in a suitable position.

On the film the double break shows up distinctly, somewhat as indicated in Fig. 5.

In locating the gun, therefore, the second set of breaks, due to the true sound-wave, had to be used.

It is clear that the onde-de-choc will be heard only by an observer if he is standing somewhere between *X* and *Y*, *i.e.* in what is known technically as the "Onde-de-choc cone," a definite cone determined by the angle *XOY*.

In general, most or all of the microphones were situated in this cone, so that a double break was recorded by most or all of the strings.

The intervals between these breaks varied from string to string according to the situation of the microphone. The interval in each case is represented by the length AB in Fig. 4. In the case of a gun, therefore, the film looked something like that shown in Fig. 5.

The interval between the onde-de-choc and the gun report is an indication of the calibre of the gun, for the bigger the gun the faster does the shell travel, and hence the greater this interval.

The calibre of the gun may, however, be determined in a different way.

Let us suppose that the positions of the gun and of the burst have been determined accurately, and are located at G and B respectively on the map of the district used by the computers in the field. Let M be one of the microphones. The calibre of the gun can be determined if the range and time of flight are known. The range is determined merely by measuring the distance GB on the map. The determination of the time of flight is somewhat more difficult.

Suppose the gun is fired at zero time, which is not observed. Let the sound reach the microphone, M, x seconds later, this being found by measuring the distance GM by a ruler graduated to read the results in seconds of time. From the film we find that the sound of the burst reaches the microphone later still, say y seconds afterwards, so that the sound of the burst reaches the microphone at a time x plus y seconds. But the burst occurred before this time, say, z seconds previously, a time found by measuring the distance MB in seconds as before, so that the burst occurred at a time $x + y - z$ seconds.

Since the gun was fired at zero time, the time of flight must be $x + y - z$ seconds.

The calibre of the weapon is then found from tables.

It remains to be added that, when the hostile artillery had been located, our own batteries often fired on it. In such cases the Sound-rangers would frequently pick up the bursts of our own shells, and so indicate where they had fallen with respect to the target.

NOTES

Prof. A. G. Nathorst (1850—1921) (Marie Carmichael Stopes, D.Sc.)

PROF. A. G. NATHORST, the famous Swedish professor whose recent death we mourn, was, while he lived, the world's most distinguished Palæobotanist. He was more than a Palæobotanist ; he was also a great geographer and geologist, who received much honour in his own country. His lamented death thins the already decimated ranks of Palæobotanists, and leaves the younger workers in that science without the guidance of one of the most sincere and generous of colleagues.

Nathorst came of a family originally of English descent, which took root in Sweden in the eighteenth century, and A. G. Nathorst's father and grandfather were both cultivated men, his father being a Professor of Agriculture.

Nathorst, who was born in 1850, joined the Geological Survey of Sweden in 1873. In 1884, Sweden (already a pioneer land in Palæobotany through Swedenborg's interest in the subject) created an honorary professorship in Palæobotany in appreciation of Nathorst's work.

He was also Honorary Fellow of more than thirty foreign learned societies, and received, among many other medals, the Lyell Medal of the Geological Society of London. He took part in several famous Polar expeditions to Spitzbergen, Greenland, Königkarsland and elsewhere.

By the early age of nineteen he had already published the first of his scientific papers on Geology, and his wonderfully accurate and beautiful work on a variety of subjects (but pre-eminently palæobotanical) total altogether close on 400 original memoirs.

In addition to originating, securing the foundation of and seeing the completion of the first adequate Palæobotanical Institute in Europe, Nathorst's help and inspiration permeates work by almost every other palæobotanical researcher in every geological horizon. No one can teach Palæobotany adequately without constantly referring to him, and in particular to the magnificently illustrated series of memoirs dealing with the early Mesozoic floras of the Hörsandstone, from which he reconstructed fascinating specimens with previously unknown morphology.

I well remember the keen pleasure which his visits to the British Museum brought me while I was working there some years ago, and I have still the cover-slip which I happened, by accident, to have in my handbag, and the lid of a glass box, which was all we could muster in that department in the way of equipment for him to show me the microscopic structure of some beautiful *Williamsonias* he had with him. Although that was not many years ago, Palæobotany in the British Museum has since been more adequately equipped, and this achievement was not a little due to Nathorst's indirect and genial influence.

Though seventy-one, Nathorst really died prematurely, the doctors say, owing to overwork in earlier years which led to an acute form of anæmia. Although he was, ever since I knew him, stone deaf, and one had either to speak with the hands or write one's own remarks, he was a most entertaining and charming companion. One of the red-letter days of my life was spent with him and Sir Cecil Spring-Rice (then British Minister at Stockholm), who was also a delighted listener to Nathorst's fund of stories.

He leaves a place at the very top of his science quite unfilled, and those who knew him feel that no one can ever fill it.

The Oxford University Expedition to Spitzbergen (J. B. G.)

We have received from a press agent some account of the expedition from Oxford University to Spitzbergen. As at present arranged the members of the expedition consist of an Ornithologist—the Rev. F. C. R. Jourdain, Mr. Julian S. Huxley—well known to the readers of the daily press for his brilliant researches on the thyroid gland, Mr. Carr-Saunders, the editor of the *Eugenics Review*, Mr. Paget-Wilkes, a medical student, Mr. F. G. Binney, who edits the *Isis*, a student-journal at Oxford, and several others, among whom Mr. J. Slater of the Royal School of Mines is to be mentioned. Mr. Huxley, Mr. Carr-Saunders, and Mr. Paget-Wilkes served during the war.

On looking at the names of the members of the expedition, we see no zoologist with a knowledge of marine fauna, qualified to give a reliable account of such natural zoological phenomena as may be met with. We specially regret the non-inclusion of a marine biologist, some man with several years' experience of the life of both plants and animals of our shores.

Mr. Roger Pocock, a writer on nature subjects, goes in advance to Tromso, Norway, from which the expedition will start. It is to be hoped that the expedition will succeed in bringing back to this country some interesting new facts, as well as collections of the interesting fauna which exist there.

The financial status of the expedition is not quite so secure as one could wish, and it is hoped that further financial assistance will be forthcoming before the start is made next month.

American Biologists

On Tuesday, March 15, a meeting was held by the National Union of Scientific Workers in the Royal School of Mines, South Kensington, when Sir Daniel Hall, K.C.B., F.R.S., took the chair, and a lecture was given by

Mr. W. B. Brierley (head of the Department of Mycology at Rothamsted) on "Personal Impressions of American Biological Research."

Mr. Brierley opened by explaining that his visit to America was made primarily to attend the Phyto-pathological Conference, which was peripatetic, ending at Lancaster, Ohio. By means of a sketch-map Mr. Brierley showed a complicated personal itinerary, from Quebec as a point of arrival, reaching to the southern limits of the United States, and including all the principal universities and biological stations.

He then indicated the most striking and individual feature of American agriculture, which he described as the main source of wealth of the country. This was the almost complete concentration in wide areas of a single crop, so that there were 500 miles together of maize, of cotton, or of rice, and not much smaller areas of fruit or vegetables for preserving. One consequence of this was that a plant disease ran riot through a whole area, and the field problems confronting the Americal agricultural biologist were so vast and menacing as almost to destroy the possibility of academic research, except in the eastern industrial regions, and to force the whole available scientific personnel into the field to stem a tide of disaster. In the industrial area, containing the older universities, the biological work approximated closely to that done in this country in subject and mode of attack, but in the State Universities in the newer agricultural regions—each with its own single crop presenting urgent problems for solution—certain features were noticeable :

(i) An early and extreme specialisation, subjects which were here studied after a degree course in botany (such as plant pathology), being themselves degree courses, and the graduates, almost all of whom, from economic pressure on individuals and the crying need in the field, were unable to take post-graduate training, immediately devoting themselves exclusively to the study of a single type of disease.

(ii) There was almost no gradation between the academic biologist of real eminence and national or international reputation and the ordinary worker dealing with a limited field of applied science. For this reason the science on which their specialised practice was founded was apt to be too much in the background. Of these giants of American biology, Mr. Brierley gave arresting word-pictures.

Coming back, he felt Europe and England to be somewhat old, sophisticated, and contemptuous of youth. America is young, and has all the boundless energy of adolescence and its unique fervour.

Sir D. Hall, before opening the discussion, pointed out that America was not a country of farmers, but of industrialists working upon the land. Consequently they were less tied by tradition, and more ready to look to science for help. On the other hand, the State legislatures, which supported the biological work, were very apt to demand immediate results, and some promising work was spoiled by premature publication. We, in this country, should take warning of the danger of allowing the legislature to get direct control of scientific research. He welcomed such a visit as Mr. Brierley's as a help towards counteracting the tendency in all civilised countries to erect quarantine walls against the entry of plants from abroad, for fear of disease. This fear was easily exploited by commercial firms for their own ends. The only way to get over the difficulty was to establish such mutual confidence between biologists in different countries as to render a guarantee of health given by the experts in any country absolutely trustworthy.

Dr. R. R. Gates said that the tendency in the past had been for American advanced biology students to go to Germany to study. It was beginning to be realised that this was not necessary, and their own universities could and should provide such training. He hoped to see far more interchanges of visits between England and America.

Mr. S. J. Duly spoke of the very careful grading of cereals in America

by scientific experts, and of how a Grade I certificate from the Bureau of Plant Industry was a good security with a bank.

Replying to a question, Mr. Brierley mentioned that the same bureau kept a record of all plant research planned or carried out in the United States, and published a programme.

He hoped to see a quinquennial international conference of biologists in this country.

Mr. F. T. Brooks, proposing a vote of thanks to Mr. Brierley, said that an American visitor to the British Association at Cardiff had commented on the wide knowledge of their subject generally shown by British biologists.

Dr. J. W. Evans, F.R.S., proposing a vote of thanks to Sir Daniel Hall, added a further plea for international fellowship and understanding in science.

English Rhythms

On March 5 Sir Ronald Ross delivered a lecture at Oxford to the Oxford Branch of the English Association on the subject of Poetry. Mr. John Masefield was in the chair, and the President of the Royal Society (Professor Sherrington) and the President of Magdalen College (Sir Herbert Warren) were present. The lecturer dealt particularly with the subject of English Rhythms, of which he had long made a special study, and illustrated his remarks by reading examples written by himself. He said that the recent amazing development of the art of music had done much, not exactly to eclipse the elder Muse, Poetry, but to place her upon a more remote pedestal; but he honoured poetry more because of the intellectual appeal contained in it, and admitted that he was very much out of the fashion as he preferred man's intellect to all his other faculties. "Evidently both the arts have or can have rhythm almost exactly in common; and while the one adds tone-music to the rhythm the other adds phone-music to it, besides the intellectual appeal. It is the combination of the rhythm and the tone-music which bestows upon the younger art her intense power of immediate penetration; and I do not, therefore, agree with those who would divest Poetry both of her rhythm and her phone-music and leave her shivering on her intellectual height with nothing to clothe her except perhaps only some thin sentimentalism of the day. To take first what I mean by phone-music—the balance and interplay of the vowels and consonants—I think that our language loses much by the horrible spelling in which it is disguised. England is a country of poets largely because our language is a harsh one. This seems a paradox; but it is not one, because English can be so rendered as to give a most rich and varied phone-music by *selection* of the proper phones, and it is largely the contrast between the harsh progress of our ordinary speech and the melody of the selected speech in our best poetry which moves us. The contrast is much greater than in the more mellifluous tongues, such as Greek, the Romance languages, and Hindustani or Burmese, let us say—and therefore the more (subconsciously) gratifying. Now our spelling almost ignores most of our softest breathed phones—the rich *dh* is turned into the sharper *th*, the *z* into the hissing *s*, the *zh* into *sh*; our long, rolling vowels are almost lost in our hieroglyphics; and there is little relation between the printed and the uttered word. I am glad to see that some of our highest poets, especially the Poet Laureate and Mr. Doughty, sometimes rebel against the tyranny; and I have for years put my own humble verses into a form of phonetic or rather homographic script, which is carefully arranged to give what I consider to be beautiful spelling, as distinct from ordinary ugly phonetic spelling, and which I call *Musaic* spelling, as it is especially designed for verse. I have indeed printed some pieces (in my *Lyra Modulata*) in a style of this kind (to the horror of my friends and the confusion of my enemies) and intend to bring out a book in it shortly! . . . Regarding rhythm, I have always

considered it to be an axiom that the rhythm of music and of poetry are essentially the same, being fundamentally based upon the dance, the march, and the beat of the drum. True, the matter has been recently much discussed, not always with complete assent to this proposition; but I consider that the dissentients have too often confused rhythm with the tone-music or with the phone-music so closely interwoven with it. As I have certainly recognised since 1881, the metrical foot is really the same as the smaller units of musical time, such as the quaver or crotchet, while the metrical line is the equivalent of one or two bars—" rests " being taken due account of. Much has been written on the matter, as, for instance, by S. Lanier, William Thompson, and Theodore Reinach (*Greek Rhythm*)—though I think these writers make the mistake of identifying the foot with the musical bar. As I have argued elsewhere, I think that we are tending to lose our sense of rhythm-melody in consequence of our love of tone-melody and tone-harmony. Many races deal chiefly with rhythm-melody—as anyone who has been in " barbarous " countries will recognise. I remember that once in the Egyptian town of Ismailia a beggar passed our house every morning at the same time playing the same and a most exquisite rhythm on his timbrel. The so-called howling dervishes at Constantinople really perform a wonderful oratorio of rhythm-melody to the words of the Koran; and it is we—we supermen—who think that they are howling. If one educates one's ear this fading music will be restored."

In giving his examples he said that, just as in music we may have only one note to the crotchet, or two quavers, or four semiquavers, and so on, while two consecutive notes may be replaced by three notes (called triplets), so in verse each crotchet-foot may contain one syllable, two syllables, three, or four, or more syllables. If crotchet-feet containing different numbers of syllables are scattered arbitrarily anyhow throughout a line-bar, we have what he called an arbitrary rhythm. In a true rhythm a crotchet-foot of a given number of syllables should occupy the same position in the line-bar, or at least in the stanza, which corresponds to the interval in music contained between two sets of double bars. He recognised, however, that, apart from the rhythm-music, the phone-music requires a recognition of long and short syllables in English, much as in Latin and Greek verse. But this long-and-short is in English connected with euphony rather than with rhythm. If a foot contains only one or two syllables, these must be long; if it contains more, these must be short. " Of course, feet of one, two, three, and four syllables (interspersed with corresponding rests) are found everywhere in English verse. In ' regular ' verse all the feet tend to contain the same number of syllables; but mixed feet are common, especially as the result of elisions used in quite an arbitrary manner. Many of our finest lyrics, however, such as the great choruses of Swinburne and of Mr. Bridges' *Demeter*, have the feet mixed in true rhythm, but this true rhythm is usually broken, or at least changed, after a short stanza or two. I wish (by the way) that in such cases more indication could be given of the rhythm, because different rhythmic readings of the same lines are frequently possible. It is as absurd to print verses written in any out-of-the-way rhythm without any indication as to what that rhythm should be, as it would be to print music without separating the bars or indicating the time. English blank verse, usually described as a pentameter, is really seen to consist, if we count the rest at the end of the line, either of two bars of three crotchets each, or of three bars of two crotchets each—the final result being much the same owing to the feeble stress given to the first note of the bar in both cases." He said that he had made many trials of several schemes of blank rhythm, especially with a view to finding the one which should be most suitable for the translation of the *Iliad*, and finally decided that the blank verse metrical scheme would suffice if the dissyllabic feet were to be more frequently sup-

planted by really effective trisyllabic feet. He protested against the habit of ignoring the rhythm when reading verse, indulged in so much by our alleged elocutionists of to-day. In conclusion he said that—

The personal note, the wandering strain, the song of the bird in the bush, naturalism, tricks of style, attempts at originality, and other shibboleths of literary criticism are insignificant. To us poetry is not a wandering strain, but a record, to be engraved on the adamant of human memory—because it is a record. To us, poetry has no fashion, no tricks, no style, for it is and must be written to-day as it was written thousands of years ago when the writer of Job heard the morning stars singing together and Homer created for the guidance of men those two great parables of Strength and of Wisdom. Things that have fashion, tricks, and style are really only journalism. Poetry has no new words, no new devices, not even new images: the only new thing about her is the new experience which she has to record in the archives of the human race. Science is the Differential Calculus of the mind—that which divides, subdivides, and analyses: poetry is the Integral Calculus of the mind—that which sums up. Without this summary science is an almanack, philosophy an opinion, literature a bulletin, history a vast acreage of tombstones, the human race a race of ants in the earth. But in order to engrave her great integration for ever upon the memory of man, poetry employs one great device—call it what you will, music, art, design. Its real name is Beauty—that is, Perfection. Poetry is the perfected utterance of the human spirit. Plato defined it as the utterance of the gods; but the aim of our philosophy is the godhood of man.

Two Opinions on Prohibition of Alcohol

At the Station of Experimental Evolution of the Carnegie Institute of Washington, several workers on the problem of the effects of alcoholisation on certain mammals have concluded that there is a deleterious influence of alcoholism on even remote progeny. This matter has been debated the world over for several decades, and the American observers have finally decided that alcohol is injurious both to the individual and to his future offspring. We agree fully that over-indulgence in all alcoholic stimulants may be injurious, but we cannot believe that such measures as have been taken in America with regard to alcoholic drinks are either to be welcomed or necessary in this country.

Easter

On April 27 Lord Desborough moved the second reading of his Bill for fixing Easter. He is to be warmly congratulated on the efforts which he has made in this direction, as the absence of a fixed date for the Easter holidays is an unmitigated nuisance to the whole public (see our last number on Our Holiday System). The Bill seems to have been received with very general approval in the House of Lords, and what opposition there was to it was based simply upon points which the public think are of very small importance, such as the sacerdotal difficulties. The Government expressed a wish to shelve the matter until a number of commercial, business, and religious bodies signified their unanimous approval of it. This argument is always extremely popular with Government Departments, because they know well enough that no unanimity can ever be obtained on any subject from a large number of different institutions, and moreover, because no individual is ever able to collect so many opinions on any new proposal. Human beings are much like cows in a field, when one tries to drive them through a gate: while some are going out, others return, and, while some return, others go out! But there is this difference: that, whereas all the cows are in one field, all the religious and

other institutions are scattered over the whole world. We were glad, therefore, to see that the Marquess of Crewe expressed his regret that Government was shelving the question, and averred that he thought Government should now take steps to further the reform. The debate was adjourned in order to give the Government an opportunity to review the question further, and we hope that in the interests of the people the measure will ultimately pass.

Physiological Reviews[1] **(W. L. Symes, M.R.C.S.)**

In *Physiological Reviews* the American Physiological Society has launched an exceedingly valuable publication.

It is to appear in four quarterly parts, constituting one volume (*ca*. pp. 500) comprising about twenty articles of which the subjects and authors are selected by the Editorial Board.

The first part (pp. 176), issued in January of this year, contains articles on: (i) "The Origin and Conduction of the Heart-beat" (J. A. E. Eyster and W. J. Meek); (ii) "The Present Status of the Problems of Anaphylaxis" (H. Gideon Wells); (iii) "Photo-electric Currents in the Eye" (Charles Sheard); (iv) "Evidence of Functional Activity on the Part of the Capillaries and Venules" (D. R. Hooker); (v) "The Carbon-dioxide Carriers of the Blood" (Donald D. Van Slyke), which fulfil admirably the editor's aim "to provide concise but comprehensive reviews of the recent literature and present status of various subjects in physiology."

Such reviews will appeal to all concerned with biological science, and are to be welcomed and recommended cordially as interesting and helpful summaries of the matters with which they deal.

Notes and News

The first triennial award of the Kelvin gold medal has been made to Dr. W. C. Unwin by a committee of the presidents of the chief engineering institutions of Great Britain.

The John Fritz gold medal for notable scientific and industrial achievement has been awarded to Sir Robert Hadfield by the unanimous vote of a committee representing the national organisations of civil, mechanical, mining, metallurgical, and electrical engineers of the U.S.A.

The gold medal of the Royal Astronomical Society has been awarded to Dr. T. Norris Russell, of Princeton University.

The Founder's medal of the Royal Geographical Society has been given to Vilhjalmur Stefansson for his services to the Dominion of Canada in the exploration of the Arctic Ocean. The Patron's gold medal goes to Gen. Bourgeois for his services to geography and geodesy as Director of the Service géographique de l'Armée and as President of the Conférence Internationale de la Carte du Monde au Millionième.

The Boyle medal of the Royal Dublin Society has been given to Dr. F. H. Pethybridge, of the Department of Agriculture, Dublin.

Dr. G. E. Hale, of the Mount Wilson Solar Observatory, has been awarded the Actonian prize of the Royal Institution.

Prof. Sydney Young has been elected President of the Royal Irish Academy, Sir James Walker President of the Chemical Society, Mr. A. Chaston Chapman President of the Institute of Chemistry (in succession to

[1] *Physiological Reviews*, edited for the American Physiological Society by W. H. Howell, Baltimore (Reid Hunt, Boston); F. S. Lee, New York; J. J. R. MacLeod, Toronto; Lafayette B. Mendel, New Haven; H. Gideon Wells, Chicago; D. R. Hooker, Baltimore. (Williams & Wilkins, Baltimore, U.S.A. $6.50 per volume.)

Sir Herbert Jackson), and Dr. T. Lyman President of the Physical Society of America.

Col.Howard Bury has been chosen to lead the expedition to Mount Everest, and Mr. Harold Raeburn to take charge of the reconnaissance of the mountain.

We have noted with great regret the announcement of the death of the following well-known scientific men during the past quarter: Mr. Bertram Blount, chemist; Dr. W. Ironside Bruce, radiologist; Prof. R. B. Clifton, F.R.S., Professor of Experimental Philosophy in the University of Oxford 1865–1915; Frédéric Houssay, Professor of Zoology at the Sorbonne and Dean of the Faculty of Science; Georges Humbert, mathematician; Prof. Isao Iijima, head of the Department of Zoology at the Imperial University of Tokyo; Prof. L. C. Miall, Professor of Botany in the University of Leeds; Dr. E. J. Mills, F.R.S., Emeritus Professor of Technical Chemistry in the Royal Technical College, Glasgow; Lord Moulton; Prof. A. G. Nathorst, late Director of the Palæobotanical Museum of the Swedish Academy; Prof. W. Odling, F.R.S., President of the Institute of Chemistry, 1885; Prof. A. W. Reinold, F.R.S., late Professor of Physics at the Royal Naval College, Greenwich; R. A. Rolfe, Botanist of the Royal Botanical Gardens, Kew; W. T. Sedgwick, Professor of Biology in the Massachusetts Institute of Technology; Franz Steindachner, zoologist, Intendant of the Hofmuseum, Vienna.

The Dean and Chapter of Westminster Abbey have decided to place a bronze medallion in the Abbey as a memorial of Sir William Ramsay.

Dr. E. H. Hall, Rumford, Professor of Physics in Columbia University, retires at the end of the session with the title of Professor Emeritus.

Dr. E. F. Nichols, who for the past year has been Director of the Nela Park Laboratory, Cleveland, U.S.A., succeeds the late Robert S. Maclaurin as President of the Massachusetts Institute of Technology.

The Faculties of Law and Medicine and the Polytechnic Institute of Rio de Janeiro have been combined to form the University of Rio de Janeiro, the first university in Brazil.

We are indebted to *Nature* (Mar. 24) for part of the following information concerning the meeting of the British Association at Edinburgh from Sept. 7 to Sept. 14 this year. The President, Sir Edward Thorpe, will deliver the inaugural address on Wednesday evening, Sept. 7, and the popular evening lectures will be given by Prof. C. E. Inglis (on the Evolution of Cantilever Bridge Construction, involving a comparison of the Forth and Quebec bridges) and by Prof. W. A. Herdman (on Edinburgh and Oceanography). Joint sectional meetings have been arranged for the discussion of the Age of the Earth, Biochemistry, Vocational Training and Tests (Economics, Education, and Psychology), Langmuir's Atomic Theory (Chemistry and Physics), the Relation of Genetics to Agriculture, the Proposed Mid-Scotland Canal, and the Origin of the Scottish People.

The only legacy or donation to pure science in this country which we have noted during the past three months is the £3,000 left to the Royal Society by the late Dr. Muirhead. Sir Ernest Cassel has given £225,000 for founding and endowing a hospital or sanatorium for the treatment of functional nervous disorders, and has purchased an estate at Penshurst, Kent, for the purpose.

The Edinburgh University appeal for £500,000 has been met only to the extent of £200,000; but the Worcester Polytechnic Institute two-million-dollar endowment fund had reached 1.9 million dollars on April 1, and the contributions of private persons and manufacturers in the district have no doubt already supplied the small remainder. Among the legacies and other contributions to Universities and Colleges in the U.S. are $250,000 to Dartmouth College under the will of Sanford H. Steele, to erect, as a memorial to his brother (a graduate of 1857), a building for teaching and research in chemistry; $150,000 to the Jefferson Medical College, Philadelphia, from the late Daniel Baugh, a trustee of the college; $1,000,000 to the School of Medicine and

Dentistry of the University of Rochester, N.Y., from the daughters of the late Henry A. Strong of that city.

A rather remarkable gift is that of $500,000 to the dermatological research laboratories of the University of Pennsylvania for medical research. This represents the profits from the sale, during the war, of the drug arsphenamine made by three members of the staff of the laboratory, Dr. F. Schamberg, Dr. A. Kolner, and Prof. G. M. Raiziss.

The Government of Panama has assigned $10,000,000 for the erection of an Institute of Tropical Diseases as a memorial to the late Surgeon-General Gorgas.

The Rockefeller Foundation, which is now assisting in an anti-tuberculosis campaign in 38 of the 87 departments of France, has given up to that country the complete control of the elaborate anti-tuberculosis organisation established in Eure-et-Loir at a cost of 4,000,000 francs. It includes 24 dispensaries, 4 complete isolation services, a departmental sanatorium, and a laboratory. It is to serve as a model for similar organisations which the French Government proposes to establish throughout the country.

Science (Feb. 4, 1921) states that M. Painlevé, Professor of Mathematics at Paris and former Prime Minister, has returned from China, where he had been on a mission concerning Chinese universities and railways. He has obtained a promise from the Chinese Government of an annual grant of 100,000 francs for an Institute of Higher Studies in Chinese at Paris. The Chinese Government has also agreed to the creation in one of the Chinese universities of an affiliated branch of the University of Paris, and it will devote to this purpose annually a sum of 500,000 francs on condition that the French Government gives the same amount. The Chinese President has also promised to have reproduced the collection of four great classics which contain the essence of Chinese civilisation and to present three copies to France. These volumes contain altogether not less than 5,000,000 pages.

The trustees of the Captain Scott Memorial Fund have decided to establish a Polar Research Institute in connection with the new department of Geography in the University of Cambridge. The Institute is to serve as a depository for the manuscripts and log-books of polar expeditions and as a place where the results of such expeditions can be worked out. It is hoped that it may be possible later on to provide a library, map-room, and museum of polar gear and equipment; but the funds which the trustees have allotted will suffice only to found the institute.

The New Anatomy Department at University College is well under way towards the beginning of construction. Plans of the buildings are complete, and, from what we have seen of them, seem to be splendidly arranged. The old hotels on the future site at Gower Street have already been pulled down and the ground cleared. Alumni of University College all the world over will feel glad to know that their college is to possess the most complete and largest Department of Anatomy in the British Empire.

Prof. J. P. Hill, F.R.S., has been elected to the Chair of Embryology, which will be tenable in the new department. This step will ensure the adequate teaching of the subject, as well as a head who will be able to direct efficiently many researches in mammalian embryology.

We find ourselves in complete agreement with the strictures passed by *Nature* (April 21) concerning the very inadequate provision made by the Government in the Civil Service Estimates for university education. The annual grant is increased by only £500,000, of which £106,030 goes to institutions which have hitherto received no grant from this source. There is, of course, the plea of an impoverished nation and of the economy imposed by the "anti-waste" campaign in the daily press; but the influence of these circumstances is not very conspicuous in the very generous salaries paid to civil servants, the utter waste of the ten-million-pound bribe to the miners,

or the attempt to grant free first-class fares to all members of Parliament irrespective of their ability to pay for themselves.

From leaflets circulated by the Department of Scientific and Industrial Research we note that twenty-three Research Associations have now been licensed by the Board of Trade. The Jute Industry Research Association and the Cast Iron Research Association have been approved by the Department but have not yet been licensed, while the Aircraft Association and the Research Association of Liquid Fuels for Oil Engines are still under consideration. The Lord President of the Council has established an Inter-Departmental Committee on Patents with the following terms of reference : " (1) To consider the methods of dealing with inventions made by workers aided or maintained from public funds, whether such workers be engaged (*a*) as research workers, or (*b*) in some other technical capacity, so as to give a fair reward to the inventor and thus encourage further effort, to secure the utilisation in industry of suitable inventions and to protect the national interest. (2) To outline a course of procedure in respect of inventions arising out of State-aided or supported work, which shall further these aims and be suitable for adoption by all Government Departments concerned."

It is also announced that the first B.A. report on Colloid Chemistry and its Applications will be reprinted by H.M. Stationery Office, to which, therefore, applications for copies (price 2*s*. 6*d*.) should be made either direct or through any bookseller.

In a paper read before the Historical Section of the American Association for the Advancement of Science entitled, " Sir William Osler's Last Historical Discovery," Mr. J. Christian Bay dealt with Osler's research on the work of Nicholas of Cusa (1401–64). It appears that Cusa possessed some knowledge of static electricity, that he performed experiments, and in general was far in advance of his time in his ideas on magnetism. He may be regarded as having anticipated Gilbert by some 150 years ; but the fact that it has taken over 500 years to bring his discoveries to light removes a good deal of their value !

A. T. Dempster, of the Ryerson Laboratory, Chicago, announces that he has been able to confirm Aston's discovery that lithium consists of two isotopes of atomic weights 6 and 7 (with a relative accuracy of 2 parts in 700). He found also that the proportions of the lighter to the heavier atoms varies from 1 : 4 to 1 : 12 according to the experimental conditions. To give the accepted atomic weight for lithium (6·94) the portion of the lighter constituent should be, however, only $\frac{1}{16}$ that of the stronger.

During the war it became necessary to apply a Brinell hardness test to materials whose softness or thinness made it impossible to apply this test in its usual form. A machine for use in these cases was devised by Messrs. H. Moore and R. Mather at the Research Department, Woolwich, and described by the former to the Institution of Mechanical Engineers in January 1921. The machine, modified for general use, is now manufactured by Alfred Herbert, Ltd., of Coventry, to whom we are indebted for the following particulars : The hardness of the specimen is estimated from the size of the dent made when a very small steel ball (e.g. of 1 mm. diameter) is pressed on to its surface by a dead load not greater than 50 kilograms weight. This depression is so small that the thickness of the specimen need not exceed one-hundredth of an inch. The test has been applied to cutlery blades, to phosphor-bronze hair-springs, and to brass cartridge cases in their finished state (i.e. when ready for firing). It is also being used to explore strain hardening. The degree of hardening by plastic deformation of metals generally increases with increasing amounts of permanent strain. Thus if a metal object, originally of uniform hardness, is unequally strained, the distribution of strain will usually be indicated by differences in hardness from point to point. It is evident that this small ball testing machine provides a most convenient method of

investigating such strains in finished articles. The measurement of the dents needs a travelling microscope magnifying about 125 times and reading to ·001 mm.

Dr. C. W. Metz has lately studied the chromosomes in two species of robber flies, *Asilus*, and he has found an occurrence which seems to be of theoretical importance. In the ordinary zygote, as is well known, each kind of chromosome is paired, one of each pair coming from the egg and one from the sperm. In a cell generation before the ripe gametes are formed the homologous members of the pairs come together in what is called synapsis. Metz has found that *Asilus* is an exception to the general rule, for the homologous chromosomes remain closely associated throughout the entire growth period of the first spermatocyte, and there is an elimination of the leptotene and zygotene stages. But true synapsis occurs in the telophase of the last spermatogonial division.

Professor C. B. Davenport, of the Eugenics Record Office, Coldspring Harbour, has been collecting statistics with regard to the heredity of twin births. It appears that about 1 per cent. of human births are twin births, but there are certain families in which the proportion rises to 5, 10, or even 15 per cent. There can be little doubt that in both man and such animals as the sheep there are strains showing a special tendency towards the production of twins. The study of twins is complicated by the fact that there are two types, namely, twins derived from a double ovulation and twins derived from a single ovulation, in the latter case there being a subsequent fission or budding of the fertilised egg. Such single-egg twins are easily distinguished clinically by being both enveloped in the same chorion, and they are always of the same sex. Professor Davenport has come to the conclusion that the influence of the male in twin production is determined by the circumstance that twin production does not depend merely upon double ovulation, but upon such a quality of the male element as shall result in a high proportion of fertilisation of eggs ovulated and a small proportion of fertilised eggs containing lethal factors.

The *New Zealand Journal of Science and Technology* contains an interesting article by Mr. C. A. Cotton on Earthquakes in their relationship to the city of Wellington. Wellington is built partly in the hollow and partly on the hills surrounding this hollow. In some parts the hollow or lower ground is very little above the sea-level, and it would need perilously little subsidence to enable the sea to sweep over the land which was raised in the earthquake of 1855. Moreover, as anyone will agree who has visited Wellington, there are numbers of houses built upon the steep hills and cliffs around the city which would be bound to suffer if and when a severe earthquake occurred. Nevertheless, much of the evidence goes to show that the severe earthquakes which have occurred within the memory of man have caused the land levels to be raised rather than subside, and we trust that any further earthquakes which may occur at Wellington will be in this direction.

The Australian opossum has become successfully acclimatised in New Zealand, and it is surprising to learn that the present annual value of opossum-skins exported from New Zealand is not less than £15,000. Professor H. B. Kirk, who has written a report on the subject, believes that the great alpine range could well be stocked with a new brood of Tasmanian brown opossums. It is said that the animals cause very little damage in the bush.

Prof. R. Ruggles Gates has brought out in the *New Physiologist* a most interesting and valuable paper on Mutations and Evolution. This article is undoubtedly one of the most important contributions to the subject which has been printed in recent years. The paper will be reviewed in the forthcoming edition of Science Progress.

The principles of racial subdivision for Europe, to-day, are the Nordic, Mediterranean, and Alpine. It has been claimed that of these races the Nordic

type was the superior. The head of the Nordic race is dolicho-cephalic, the eyes are light, and the hair is fair or red, the nose is straight, and the body generally tall. While it is very difficult in such a mixed nation as we are to define which race is or is not responsible for our successes militant or intellectual, there does seem to be some ground for believing that the dominant race in the British Empire is the Mediterranean. Still, in approaching a subject of this kind we must remember that generalisations are quite often unjust. Sir Charles Walston believes that such ethnological generalisation is both unfruitful and injurious, and that the aim of ethnographers and historians should be in concentrating the efforts of the citizens of their countries on the maintenance of the spirit of their constitution and on the realisation of the highest ideals of mankind.

The Observatory on Salcombe Hill, above Sidmouth, is henceforth to be called "The Norman Lockyer Observatory," as a memorial to the scientific pioneer who has been described by Dr. Deslandres, President of the Paris Academy of Sciences, as "one of the greatest astronomers of all time."

The appropriateness of the choice of memorial will be generally recognised. The Observatory was founded by Sir Norman Lockyer and Lieut-Colonel F. K. McClean in 1913, after the closing of the Solar Physics Observatory at South Kensington. The founders provided funds for erecting the necessary buildings on land given by Lady Lockyer, as well as for valuable instruments, laboratory apparatus, and astronomical publications for the library—Lady Lockyer and other donors also contributing funds and apparatus. The greater part of the Observatory equipment presented by Lieut.-Colonel McClean consisted of instruments that had belonged to his father, the late Mr. Frank McClean, F.R.S., who did such notable astronomical work both at Tunbridge Wells and at the Cape Observatory. The institution is unique in this country in being the property of a corporation consisting of members interested in the promotion of astronomical research. Sir Norman Lockyer's astronomical work in the Observatory adds to the historical interest of the buildings.

It is proposed to render the memorial more complete by placing in the Observatory a portrait of Sir Norman Lockyer, in the shape of a Medallion, to be executed by Sir Hamo Thornycroft, R.A.

As there are, no doubt, many persons who will value the opportunity of joining in this tribute, it has been decided not to restrict to a few friends participation in defraying the cost of the Medallion, but to invite contributions of any amount from all who may wish to express appreciation of Sir Norman's astronomical work. Names of donors will be recorded in a suitable manner in the Observatory.

Contributions towards the cost of the Medallion should be sent to the Hon. Secretary of the Observatory Corporation, Captain W. N. McClean, 1, Onslow Gardens, London, S.W.7.

We have received a new publication called *Man in India*, a quarterly record of anthropological science with special reference to India, edited by Rai Bahadur Sarat Chandra Roy, M.A., B.L., M.L.C., and published by the editor at the *Man in India* office, Church Road, Ranchi. The first number issued in March of this year contains the following articles: "A Suggested Programme for Anthropological Investigations in India," by W. Crooke; "Anthropological Research in India," by the editor; "Human Sacrifice in Central India," by Rai Bahadar Hira Lal; "Kinship and Marriage in India," by W. H. R. Rivers, and various notes.

The report on Post-graduate Teaching in the University of Calcutta during the session 1919–20 shows that, while the aggregate number of post-graduate students in the University was slightly greater than in the previous session, the numbers in the departments of Science and Mathematics had decreased considerably, while the department of English showed a correspondingly large increase. The University has set on foot a scheme for investigating

the sources of information on early Indian history contained in manuscripts written in Tibetan, Chinese, and Japanese. To this end it has arranged to have certain members of its staff instructed in Chinese and Japanese, and has imported a learned doctor from the Monastery of Gaden, Tibet, to give instruction in that language. The outstanding feature of the report is the list of papers published by the Department of Physics in the University. Prof. C. V. Raman and his research works have published 16 papers since July 1919 and have 14 "under publication," while other members of the staff have another 20 papers either published or in various stages of preparation. Sixteen of these have appeared in the standard English and American journals.

The report of the Fuel Research Board on *The Winning, Preparation, and Use of Peat in Ireland* (H.M. Stationery Office, 3*s*. net.) contains a very complete account of the whole peat question so far as it affects Ireland. This matter has, however, recently been dealt with in these "Notes" (Oct. 1920) in connection with Prof. Purcell's lecture on *The Peat Resources of Ireland*. It will therefore suffice if we summarise very briefly the recommendations of the committee. They conclude that peat fuel may be won in Ireland on a scale which would warrant the establishment of electric power stations at one or more of the most favourably situated bogs. The peat winning should be associated with the reclamation of land for agricultural purposes, and, to be economically possible, must be carried out by machinery. This would lessen the labour required, but the outstanding difficulty remains—namely, that peat-winning is a seasonal occupation, making most of its demand on labour from February to September. The committee, recognising this, and pointing out that casual labour is a perpetual source of restlessness and discontent, recommended that the peat industry should be coupled with an agricultural colony in the hope that the two might be worked so as to supplement each other. This suggestion, together with an estimate of the cost of running an experimental colony at the Bog of Allen, was submitted to the Department of Agriculture and Technical Instruction for Ireland. A sub-committee of this department appointed to examine it very properly pointed out that, since both agriculture and the peat industry require men during the same period of the year, these industries could hardly supplement each other. Further, it was shown that the cost of the experimental station had been most seriously under-estimated. Here the matter appears to rest, except that the Fuel Board has set up a Swedish peat machine at Turraun, and is experimenting at Greenwich with the peat cut by it.

We have received from the Fuel Research Board *Technical Paper No.* 1 (A new laboratory method for the assay of coal for carbonisation purposes) and *No.* 2 (Report on the Simmance Total Heat Recording Calorimeter), and from the Building Research Board its first *Special Report* (Sand-lime and other concrete bricks). These papers contain detailed tests, descriptions, and recommendations concerning the subjects with which they deal, and may all be obtained from H.M. Stationery Office at a cost of a few pence.

The inaugural meeting of the Institute of Physics was held on Wednesday, April 27, in the rooms of the Institution of Civil Engineers. The chair was taken by Sir R. T. Glazebrook, first President of the Institute, and the chief speakers were Sir J. J. Thomson (the Honorary Fellow of the Institute) and Mr. A. J. Balfour.

Sir J. J. Thomson opened his remarks by referring to the very satisfactory support the Institute had received from those qualified to join it. Its membership roll contains 300 names, and, even including school teachers, it is estimated that there are not more than 1,000 eligible persons in the country altogether. He then went on, in a most amusing and interesting speech, to contrast the condition of physical science as it exists to-day with its state when he commenced his scientific career some fifty years ago. In those days the only

physical laboratories in the kingdom were those at the Royal Institution, at the Scottish Universities, at University College, London, and at Oxford. The Cavendish laboratory had been planned and cost about £8,500, exclusive of fittings. The number of physicists in the country was extremely small, and limited to those in whom enthusiasm for research overcame the disadvantages attendant on the smallness of its pecuniary rewards. It was, in fact, the period when physics was just commencing to form a definite and separate profession in this country.

Those who worked in the first half of the century and whose names remain well known to-day were, with but few exceptions, men who gained their livelihood in another field or from private means. Their physical work was their hobby and its own reward; it might enhance their reputation, bring them pleasant companionship, but was not expected to line their pockets. It was a period in which there were but few workers, but those men of outstanding merit. There was, indeed, no scope and no incentive for men of smaller talent. Gradually, in the course of years the field of research has widened, it requires its labourers as well as its pioneers, its benefits to mankind have become apparent to all but those who will not see, and there is work both for genius and those of lesser gift. Modern educational methods have provided workers, but the old tradition dies hard, and too often the work has still to be its own reward. We are quite sure that many of the older and more distinguished physicists take a pride in supplying information and giving the benefit of their wisdom, knowledge, and experience, without material recompense; would, indeed, be offended at the offer of a monetary reward, and certainly far too proud to ask for it. The result of this attitude in the past is to be seen in the general contempt for science in bureaucratic and political circles. A lawyer is worthy of his hire, a commission agent must be paid a great price; but a scientist is too worthless even to be given control of the Department of Scientific Research! It is to endeavour to correct these notions that the Institute of Physics exists, to try to secure a more widespread recognition of Physics as a profession, and, we fear in the far distant future, to serve as an organisation for securing and controlling a building wherein the activities of all the physical societies may be concentrated—a club, a meeting-place, and a library for the physicists throughout the country.

Mr. Balfour emphasised the fundamental character and importance of physical work, and expressed surprise that the formation of the Institute had been so long delayed. He confessed that when he saw great industrial disputes going on about the distribution of the results of industry he could not help thinking, "Why do not you devote half the energy and half of the amount of money involved to increasing the power of man over nature, which would increase the share and increase the total result to be divided among members of the community, instead of devoting your energies to saying how the relatively insignificant amount we now produce is to be divided among the producers?" We cannot help feeling how unfortunate it is that Mr. Balfour has been converted to this view so late. Now "the work that the Advisory Council has done in providing opportunities for research deserved all the praise which Sir J. J. Thomson had given to it. *Unfortunately, the present impoverished state of the country has compelled a reluctant Treasury to cut down the sum at their disposal.*" It can only afford £10,000,000 for the miners, huge bonuses to civil servants, free first-class fares for M.P.s, their relief from income tax—alas! alas!! alas!!!

CORRESPONDENCE

To the Editor of "Science Progress"

SYMBIOSIS

From H. Reinheimer

Dear Sir,—Kindly allow me to reply briefly to the review of my book on *Symbiosis, a Socio-physiological Study of Evolution*, in your issue for April.

(*a*) I have *not* stated that "all plants whose pollen causes hay-fever are useless weeds," but merely (on medical authority) that the majority are.

(*b*) I have *not* overlooked self-fertilisation, but merely considered it as inferior in progressive evolution to cross-fertilisation.

Perhaps your reviewer would have been better pleased if (in my primitiveness) I had fastened upon exceptions rather than upon rules, because this would have rendered it easier for him to brand me as one whose twenty years of scientific work is "based upon half-truths with their pernicious verisimilitude."

(*c*) I have *not* declared, as is imputed, that wild races are exempt from losses of "factors." On the contrary, I have asserted it. Domestic races I regard as deficient not *qua* domestic, but because they are over-exploited. Wild species are not immune *qua* wild, but in so far as theirs is normal metabolism.

(*d*) My hypothesis does *not* resolve itself into a question of the superiority, if any, of *Cystococcus* symbiotic over *Cystococcus* growing freely. Your reviewer admits I am giving an extended meaning to symbiosis. Why, then, should he pin me down to the narrow morphological view, which I consider inadequate and obsolete? My view is that a symbiotic is generally superior in avail towards life to a parasitic relation.

(*e*) According to your reviewer, my view that predatory animals or plants are more subject to parasitism than others "will scarcely bear a moment's investigation."

With regard to this I will merely say that, whilst he has, of course, a right to his opinion, I do not feel in the least shaken in my own by his *ipse dixit*,

Yours faithfully,

H. Reinheimer.

May 2, 1921.

To the Editor of "Science Progress"

OPTICAL PROJECTION

From R. S. Wright, M.I.E.E.

Dear Sir,—I hardly know how far it may be within the bounds of publishing etiquette to review your reviewer, but I really must protest against one or two of the statements made regarding my book on the above subject.

In the first place, I read that "There are minor inaccuracies, such, for instance, as where the passing of luminous carbon particles from one carbon

to the other is referred to as if it were the actual source of light. This, of course, is not the case, the source being in the case of continuous current, the crater on the positive carbon. The length of arc is also arbitrarily laid down as being one-eighth of an inch; but this again would vary with the amount of current and the size of the carbons."

With regard to the statements made, I cannot find any reason for the first, and if your reviewer will turn to the bottom of page 45 he will read: "The light from the continuous current Arc Lamps comes chiefly from this upper or positive carbon, which 'craters' as it is used."

It is true that I state later on that the current is conducted across the Arc by a bridge of white-hot carbon particles, but there is no statement whatever that this is the main source of light.

With regard to the length of the Arc being arbitrarily laid down as one-eighth of an inch, this is distinctly stated as being approximate only, and as a guide for the amateur in lantern work it is sufficiently near.

Your reviewer also expresses his regret that Optical Bench Lanterns are not referred to, but he seems to have overlooked the fact, although he refers to it, that the book is being published in two parts. The one now under review deals with Optical Lanterns for ordinary slide demonstration only, and the Optical Bench type, belonging to the class known as Science Lanterns, will be dealt with in their right place.

I am sorry to have to make these comments, but I think it is a great pity that a reviewer should contribute criticisms which a little more careful reading of the book would have avoided.

Yours faithfully,

R. S. Wright.

May 17, 1921.

ESSAYS

THE OPERATIVE ROOTS OF THE CIRCLE-FUNCTION (Sir Ronald Ross).

1·0. NEARLY two hundred years ago, in the first pages of his *Miscellanea Analytica* (London, 1730), A. De Moivre published the famous theorem with which his name is associated, and which is now commonly written

$$(\cos\theta + \sin\theta\,.\sqrt{-1})^n = \cos n\theta + \sin n\theta\,.\sqrt{-1}. \qquad (1)$$

In SCIENCE PROGRESS, vol. xv., No. 50, p. 628, April 1921,[1] I showed that this theorem is only a particular case of a wider operative theorem which may be written, in the notation used by me,

$$[\cos\theta\,.\,\mathrm{O} + \sin\theta\,.\sqrt{r^2 - \mathrm{O}^2}]^n = \cos n\theta\,.\,\mathrm{O} + \sin n\theta\,.\sqrt{r^2 - \mathrm{O}^2}, \qquad (2)$$

whatever real value n may have; and I now proceed to indicate some of the immediate consequences of the rule.

This equation means that the operation contained within square brackets on the left, when raised to the nth operative power (that is, when it is iterated on itself $n - 1$ times), becomes the operation on the right. Now, $\sqrt{r^2 - \mathrm{O}^2}$ is the operation which constructs a circle of r radius about the origin of rectangular axes. Denote it by K_r: then if we use the proper measure for angles—what I call Quadrantal Measure—we have

$$K_r^m = [\sqrt{r^2 - \mathrm{O}^2}]^m = \cos m\,.\,\mathrm{O} + \sin m\,.\,K_r. \qquad (3)$$

That is, if m be a fraction, the expression on the right is an mth *operative root* of the circle-function. If we now put $r = 0$ and substitute θ for m—as the Quadrantal Measure enables us to do—we have

$$K_0^\theta = [\sqrt{-\mathrm{O}^2}]^\theta = \cos\theta\,.\,\mathrm{O} + \sin\theta\,.\,K_0 \qquad (4)$$

where $K_0 = \iota\mathrm{O}$. This was previously shown to be De Moivre's Theorem; since

$$[(\cos\theta + \iota\sin\theta)\mathrm{O}]^n = (\cos\theta + \iota\sin\theta)^n\mathrm{O}. \qquad (5)$$

1·1. It is easy to see that K_r^θ is the *operative ratio* of two chords drawn from a point on the circumference of a circle of r diameter (not radius) and separated by the angle θ. That is, if a and b be the two chords,

$$a = K_r^\theta b \qquad \text{or} \qquad \frac{a}{b} = K_r^\theta, \qquad (1)$$

the double fraction-line being used to distinguish an operative ratio or fraction

[1] In that note I asked for information as to whether the proposition here given is already known, but I have had no affirmative reply. Mr. Laugharne-Thornton, however, points out that De Moivre gave his theorem in a different form to the one used here; and the Rev. J. Cullen, S.J., Stonyhurst College, kindly informs me that, as I said, my proposition is not given in the works of Hamilton, Tait, or Joly on Quaternions.

from an algebraic one. Thus K_r^θ is a *versor* which converts b into a—that is, which turns b through the angle θ and also changes its length to equal that of a, another chord of the circle of r diameter. If $r = 0$, then K_0^θ, that is,

$$(\cos\theta + \iota \sin\theta)\mathbf{O}, \qquad (2)$$

is a "complex" versor which affects the infinitesimal chord of a circle of zero-diameter (point-circle) in the same manner.

We may infer that most of the important theorems which have been based on De Moivre's can be similarly generalised in terms of K. For example, we can resolve $K_r^n \pm \mathbf{O}$ into *operative factors* and thus solve the equation $K_r^n \pm \mathbf{O} = o$; and in place of the usual trigonometrical *exponential values* we obtain

$$K^\theta = \theta^\circ\mathbf{O} + \theta K + \frac{1}{2}\theta^2K^2 + \frac{1}{6}\theta^3K^3 + \ldots, \qquad (3)$$

the subscript r being understood. The *operative logarithm* is another important entity because it expresses the angle of rotation; and it may be denoted by the abbreviation *opl*. Thus as $K^\theta = a//b$, we write

$$\theta = opl_K\frac{a}{b}; \qquad \text{or} = l_K\frac{a}{b} \text{ for short.} \qquad (4)$$

Lastly, the similarity of K_r^θ to a *quaternion* is obvious, and we may surmise that it will be useful in most branches of geometry.

2·0. The *Quadrantal Measure* for angles is always very convenient—not only here, but for general use. We denote an angle merely by the positive or negative integer or fraction which indicates its ratio to a right angle. Thus the angles, π, $\frac{1}{2}\pi$, 60°, 45°, 30° . . . are expressed respectively by 2, 1, $\frac{2}{3}$, $\frac{1}{2}$, $\frac{1}{3}$ . . . ; and $\cos 1 = o$, $\sin 1 = 1$, $\cos(1-\theta) = \sin\theta$, etc. Using Circular Measure, we should write

$$K_r^m = \cos m\,\tfrac{1}{2}\pi\,.\,\mathbf{O} + \sin m\,\tfrac{1}{2}\pi\,.\,K_r; \qquad (1)$$

but the Quadrantal Measure enables us to omit the $\frac{1}{2}\pi$.

2·1. The *Explicit Operative Notation* employed here is easily explained as follows. Let y be any function of x whatever—suppose for example that

$$y = \phi x = a + bx + cx^2 + \ldots + qx^n. \qquad (1)$$

Now, it is most necessary in operative analysis for us to be able to denote any operation ϕ explicitly and in detail without involving x or any other argument, and in such a manner that we may indicate its inversion, its iteration, and its algebraic and operative relations with numbers or with other operations without circumlocution. How can we do so? There is certainly no method in general employment now; but there *is* one, and only one, method available—we can substitute ϕ° for x in the expression on the right. Then

$$\phi = a + b\phi^\circ + c(\phi^\circ)^2 + \ldots + q(\phi^\circ)^n. \qquad (2)$$

If both sides of this equation be applied to the argument x, the original equation is restored. But this notation has never been used (so far as I can find) in consequence of one of the most singular (but unnoticed) fallacies[1] in mathematics, that $\phi^\circ = 1$. If we substitute this value in the above equation we obtain

$$\phi = a + b + c + \ldots + q; \qquad (3)$$

which is both meaningless and absurd; so that the algebraic machinery breaks down—clear evidence that there has been an error somewhere. Six-

[1] It is true that $\phi^\circ x = x$, but not that $\phi^\circ x = 1 \times x$.

teen years ago I tried to expose the fallacy and to suggest the proper solution in a lengthy paper (2). The truth is that the "identical operation" ϕ^0 cannot possibly be equated to numerical unity or to any number, but is itself the *unit of operation*. Denote this by O for brevity, and set aside a special *operative bracket* (the square bracket is the best), and we possess at once a powerful addition to our mathematical algorithm, which enables us to apply either algebraic or operative processes to the same equations, at will and with equal rigidity. It is not symbolism, but merely the logical completion of the ordinary algebraic and functional notation now in general use.[1]

3·0. It is advisable to begin with the simplest algebraic proof of the proposition 1·02. Perhaps the most useful one is the following. Let

$$y = \cos\gamma \,.\, x + \sin\gamma \,.\, \sqrt{r^2 - x^2}, \qquad x = \cos\theta \,.\, z + \sin\theta \,.\, \sqrt{r^2 - z^2}\,;$$

that is, $y = [\cos\gamma \,.\, \mathrm{O} + \sin\gamma \,.\, \sqrt{r^2 - \mathrm{O}^2}]x, \qquad x = [\cos\theta . \mathrm{O} + \sin\theta . \sqrt{r^2 - \mathrm{O}^2}]z\,;$

and therefore $y = [\cos\gamma . \mathrm{O} + \sin\gamma . \sqrt{r^2 - \mathrm{O}^2}]\,[\cos\theta \,.\, \mathrm{O} + \sin\theta \,.\, \sqrt{r^2 - \mathrm{O}^2}]z.$ (1)

Now, it can easily be shown (see 3·61) that

$$r^2 - (\cos\theta \,.\, \mathrm{O} + \sin\theta \,.\, \sqrt{r^2 - \mathrm{O}^2})^2 = (\cos\theta \,.\, \sqrt{r^2 - \mathrm{O}^2} - \sin\theta \,.\, \mathrm{O})^2. \quad (2)$$

Hence $[\cos\gamma \,.\, \mathrm{O} + \sin\gamma \,.\, \sqrt{r^2 - \mathrm{O}^2}]\,[\cos\theta \,.\, \mathrm{O} + \sin\theta \,.\, \sqrt{r^2 - \mathrm{O}^2}] =$

$$= \cos\gamma\,(\cos\theta \,.\, \mathrm{O} + \sin\theta \,.\, \sqrt{r^2 - \mathrm{O}^2}) \pm \sin\gamma\,(\cos\theta \,.\, \sqrt{r^2 - \mathrm{O}^2} - \sin\theta . \mathrm{O})$$
$$= \cos\,(\theta \pm \gamma) \,.\, \mathrm{O} + \sin\,(\theta \pm \gamma) \,.\, \sqrt{r^2 - \mathrm{O}^2}. \quad (3)$$

This process can be continued for any number of angles, and the reason for the ambiguity of sign will be explained later (4·26). Now write

$$K_r = + \sqrt{r^2 - \mathrm{O}^2}, \quad (4)$$

and suppose that θ is $\frac{1}{n}$th of a right angle, so that, in Quadrantal Measure, $\theta = \frac{1}{n}$ and $n\theta = 1$. Then

$$\left[\cos\frac{1}{n} . \mathrm{O} + \sin\frac{1}{n} . K_r\right]^n = \cos 1 \,.\, \mathrm{O} + \sin 1 \,.\, K_r = K_r\,;$$

and $K_r^{\frac{1}{n}} = \cos\frac{1}{n} . \mathrm{O} + \sin\frac{1}{n} . K_r\,;$ or $K_r^\theta = \cos\theta \,.\, \mathrm{O} + \sin\theta \,.\, K_r.$ (5)

Hence if we use only the positive value of the radical,

$$K_r^\gamma\, K_r^\theta = K_r^{\gamma+\theta} = K_r^\theta\, K_r^\gamma\,; \quad (6)$$

that is, these operations are Abelian, or permutable.

It can easily be shown by the methods employed for De Moivre's Theorem that n, and therefore θ, may be positive or negative, integral or fractional.

3·1. If we use *either* value of the radical for K_r, we obtain ambiguous results which are inconvenient at present for deducing its properties. This can most easily be done from the known properties of the trigonometrical

[1] Thus when we wish to render an operation explicitly but apart from its subject, we replace that subject by ϕ^0, or O; and when the expression so obtained is to operate on the following matter (as ϕ operates on x in ϕx), we enclose it in square brackets; and when it operates on itself $n - 1$ times (as ϕ does in ϕ^n), we affix the exponent n to the square brackets. On the other hand any number or expression in other than square brackets is merely multiplied into the following matter (as in $(\phi)x$); or is raised to its nth algebraic power by an exponent (as in $(\phi)^n$). The powers of O, such as O^n, are algebraic; and $\phi \,.\, \psi$ denotes the algebraic product of ϕ and ψ.

ratios contained in the formulæ just given. Thus we find the following *operative* properties of K:

$$K_r^0 = \cos 0 \,.\, \mathbf{O} + \sin 0 \,.\, K_r = \mathbf{O}$$
$$K_r^{-1} = \cos(-1) \,.\, \mathbf{O} + \sin(-1) \,.\, K_r = -K_r$$
$$K_r^2 = \cos 2 \,.\, \mathbf{O} + \sin 2 \,.\, K_r = -\mathbf{O}$$
$$K_r^3 = \cos 3 \,.\, \mathbf{O} + \sin 3 \,.\, K_r = -K_r$$
$$K_r^4 = \cos 4 \,.\, \mathbf{O} + \sin 4 \,.\, K_r = \mathbf{O} \qquad (1)$$
$$K_r^\theta = \cos\theta \,.\, \mathbf{O} + \sin\theta \,.\, K_r \qquad K_r^{-\theta} = \cos\theta \,.\, \mathbf{O} - \sin\theta \,.\, K_r$$
$$K_r^{1+\theta} = \cos\theta \,.\, K_r - \sin\theta \,.\, \mathbf{O} \qquad K^{1-\theta} = \cos\theta \,.\, K_r + \sin\theta \,.\, \mathbf{O} \qquad (2)$$
$$K^{\theta\pm 2} = -K^\theta \qquad K^{\theta\pm 4} = K^\theta \qquad (3)$$
$$K_r^\theta + K_r^{-\theta} = 2\cos\theta \,.\, \mathbf{O} \qquad K_r^\theta - K_r^{-\theta} = 2\sin\theta \,.\, K_r$$
$$K^{\theta-1} + K^{1-\theta} = 2\sin\theta \,.\, \mathbf{O} \qquad K_r^{\theta-1} - K^{1-\theta} = 2\cos\theta \,.\, K_r \qquad (4)$$

(See however 4·21).

Here the value of $K_r^{1+\theta}$ has been obtained from trigonometrical properties of the sine and cosine; for we have assumed that

$$K_r^{1+\theta} = \cos(1+\theta) \,.\, \mathbf{O} + \sin(1+\theta) \,.\, K_r = -\sin\theta \,.\, \mathbf{O} + \cos\theta \,.\, K_r.$$

But we may obtain the same result by direct operation; for

$$K_r^{\theta+1} = K_r^\theta K_r = [\cos\theta \,.\, \mathbf{O} + \sin\theta \,.\, K_r]K_r = \cos\theta \,.\, K_r + \sin\theta \,.\, K_r^2$$
$$= \cos\theta \,.\, K_r - \sin\theta \,.\, \mathbf{O}; \qquad (5)$$

for, as shown above, $K_r^2 = -\mathbf{O}$. For another example,

$$K_r^{\theta\pm 2} = K_r^{\pm 2} K_r^\theta = [-\mathbf{O}][K_r^\theta] = -K_r^\theta;$$

but also $K_r^{\theta\pm 2} = [\cos\theta \,.\, \mathbf{O} + \sin\theta \,.\, K_r]K_r^{\pm 2} = -\cos\theta \,.\, \mathbf{O} + \sin\theta \,.\, K_r^{1\pm 2}$, (6)

and $K_r^{1\pm 2} = K^3$ or $= K^{-1}$, both of which $= -K_r$. Of course

$$\mathbf{O}\phi = \phi \text{ and } \phi\mathbf{O} = \phi.$$

8·2. By operating with it either on zero or on r, we can break up a K-operation into two parts which resemble the scalar and vector parts of a quaternion. For

$$K_r^\theta 0 = [\cos\theta \,.\, \mathbf{O} + \sin\theta \,.\, K_r]0 = \cos\theta \,.\, 0 + \sin\theta \,.\, \sqrt{r^2 - 0^2} = r\sin\theta;$$
$$K_r^\theta r = [\cos\theta \,.\, \mathbf{O} + \sin\theta \,.\, K_r]r = \cos\theta \,.\, r + \sin\theta \,.\, \sqrt{r^2 - r^2} = r\cos\theta. \qquad (1)$$

Hence we may write

$$r \,.\, K_r^\theta = K_r^\theta r \,.\, \mathbf{O} + K_r^\theta 0 \,.\, K_r; \qquad (2)$$

and also
$$rK_r^{\theta+\gamma} = K_r^\theta r \,.\, K_r^\gamma + K_r^\theta 0 \,.\, K_r^{1+\gamma}$$
$$= K_r^\gamma r \,.\, K_r^\theta + K_r^\gamma 0 \,.\, K^{1+\theta} = rK_r^{\gamma+\theta}; \qquad (3)$$

and
$$rK_r^{\theta+\gamma}0 = K_r^\theta r \,.\, K_r^\gamma 0 + K_r^\theta 0 \,.\, K_r^\gamma r = r^2\sin(\theta+\gamma),$$

and
$$rK_r^{\theta+\gamma}r = K_r^\theta r \,.\, K_r^\gamma r - K_r^\theta 0 \,.\, K_r^\gamma 0 = r^2\cos(\theta+\gamma): \qquad (4)$$

for
$$K_r^{1+\gamma}0 = K_r^\gamma r, \text{ and } K_r^{1+\gamma}r = K_r^{2+\gamma}0.$$

Put slightly differently,

$$K_r^\gamma(r\sin\theta) = K_r^\gamma K_r^\theta 0 = K_r^\theta(r\sin\gamma) = r\sin(\theta+\gamma).$$
$$K_r^\gamma(r\cos\theta) = K_r^\gamma K_r^\theta r = K_r^\theta(r\cos\gamma) = r\cos(\theta+\gamma). \qquad (5)$$

By supposing that $r = 1$ and by writing K for K_1, we have

$$K^{\gamma} \sin\theta = \sin(\gamma+\theta) = K^{\theta} \sin\gamma,$$
$$K^{\gamma} \cos\theta = \cos(\gamma+\theta) = K^{\theta} \cos\gamma; \qquad (6)$$

so that
$$K \sin\theta = \sin(1+\theta) = \cos\theta,$$
$$K \cos\theta = \cos(1+\theta) = -\sin\theta,$$

and
$$\cos\theta = K \sin\theta \text{ and } \sin\theta = K^{-1} \cos\theta. \qquad (7)$$

Thus if we had deduced the properties of K^{θ} independently of trigonometry, we could easily have proved the formulæ for the trigonometrical ratios of sums of angles by the method used in 3·0.

3·3. The reader must be warned to be careful regarding the signs. Obviously $\sqrt{1-\sin^2\theta} = \pm\cos\theta$; but this ambiguity does not exist when we use K. For $K^{\gamma} \sin\theta$ always equals $\sin(\gamma+\theta)$, the sign of which depends upon the magnitude of $\gamma+\theta$; so that always

$$K \sin\theta = \sin(1+\theta); \quad K \sin(-\theta) = \sin(1-\theta);$$
$$K \cos\theta = \cos(1+\theta); \quad K \cos(-\theta) = \cos(1-\theta).$$

As $\cos(-\theta) = \cos\theta$ and $\sin(2-\theta) = \sin\theta$, we may think erroneously that $K\cos(-\theta) = K\cos\theta$ and $K\sin(2-\theta) = K\sin\theta$; but

$$K\cos\theta = -\sin\theta, \qquad K\cos(-\theta) = \sin\theta; \qquad (1)$$

and while $K\sin\theta = \cos\theta$, $\quad K\sin(2-\theta) = K^{1+2-\theta}\mathrm{O} = K^2K^{1-\theta}\mathrm{O} = -\cos\theta$.
Similarly $K(-\sin\theta) = KK^2\sin\theta = K^2K\sin\theta = -\cos\theta. \qquad (2)$

Before putting such trigonometrical expressions as $\cos\theta$ and $\sin\theta$ under K-operators we must know the sign of θ and distinguish between the sine of an angle and of its supplement. In this respect the K-operators are more exact than the trigonometrical ratios. See also 4·21.

3·4. The expression for K_r^{θ} consists (1) of the two *operations* O and K, and (2) of their *numerical coefficients* $\cos\theta$ and $\sin\theta$. If we operate on it with K_r^{γ} we can suppose that this affects *either* O and K, *or* their coefficients; so

that $\quad K_r^{\gamma}K_r^{\theta} = \cos\theta \,.\, K_r^{\gamma} + \sin\theta \,.\, K_r^{1+\gamma}, \qquad (1)$

or $\quad = \cos(\gamma+\theta)\,.\,\mathrm{O} + \sin(\gamma+\theta)\,.\,K_r; \qquad (2)$

or $\quad rK_r^{\gamma}K_r^{\theta} = K_r^{\theta}r\,.\,K_r^{\gamma} + K_r^{\theta}\mathrm{O}\,.\,K_r^{1+\gamma} = K_r^{\theta+\gamma}r\,.\,\mathrm{O} + K_r^{\theta+\gamma}\mathrm{O}\,.\,K_r$, *etc.* $\qquad (3)$

3·5. As an example of all this consider the expression for *multiple angles*. By 3·14 (omitting r for convenience)

$$[K^{\theta} + K^{-\theta}]K^{\theta} = [2\cos\theta\,.\,\mathrm{O}]K^{\theta} = 2\cos\theta\,.\,K^{\theta}.$$

But
$$[K^{\theta} + K^{-\theta}]K^{\theta} = \mathrm{O} + K^{2\theta};$$

therefore
$$K^{2\theta} = 2\cos\theta\,.\,K^{\theta} - \mathrm{O}. \qquad (1)$$

Now put $s = 2\cos\theta$ for short, and suppose n to be any integer.

Then $\quad K^{2n\theta} = [sK^{\theta} - \mathrm{O}]^n$ and $K^{2n\theta+\theta} = [sK^{\theta} - \mathrm{O}]^nK^{\theta} \qquad (2)$

are the general expressions. To find the expansions of these we must iterate the expression in square brackets, and for this we shall require to know the value of $[K^{\theta}][sK^{\theta} - \mathrm{O}]$—— or, as we can write it without ambiguity, $K^{\theta}(sK^{\theta} - \mathrm{O})$. As $sK^{\theta} - \mathrm{O} = K^{2\theta}$, and we have $K^{\theta}K^{2\theta} = K^{2\theta}K^{\theta}$, the two operations are permutable. Hence

$$K^{\theta}(sK^{\theta} - \mathrm{O}) = sK^{2\theta} - K^{\theta},$$

and
$$K^{4\theta} = [sK^{\theta} - \mathrm{O}]^2 = s^2K^{2\theta} - 2sK^{\theta} + \mathrm{O},$$
$$K^{6\theta} = [sK^{\theta} - \mathrm{O}]^3 = s^3K^{3\theta} - 3s^2K^{2\theta} + 3sK^{\theta} - \mathrm{O}, \qquad (3)$$

and so on; the expansion being like that of a binomial raised to an algebraic power except that powers of $\mathbf{O}$ do not appear.[1] The series can be further reduced; so that, for instance,

$$K^{7\theta} = s^6K^\theta - s^5\mathbf{O} - 5s^4K^\theta + 4s^3\mathbf{O} + 6s^2K^\theta - 3s\mathbf{O} - K^\theta. \qquad (4)$$

Operating alternately on zero and on r (or 1), we get the values of $r\ sin\ m\theta$ and $r\ cos\ m\theta$ which are the well-known ones. Hence we find the exponential values of $sin\ \theta$ and $cos\ \theta$ and the series of 1·13. This can be done more elegantly by using operators throughout, but the demonstration (involving limits) is out of place here.

8·6. The *Pythagorean Proposition* is given by an important *algebraic* property of the K-operations; that is to say

$$[\mathbf{O}^2 + (K_r)^2]K_r^\theta = [\mathbf{O}^2 + (r^2 - \mathbf{O}^2)]K_r^\theta = [r^2]K_r^\theta = r^2;$$

hence $$(K_r^\theta)^2 + (K_r^{1+\theta})^2 = r^2. \qquad (1)$$

For, any number by itself operating on any subject merely reproduces itself—because $[r^2]S = [r^2\mathbf{O}^\circ]S = r^2(S)^\circ = r^2$, whether S be a number or an operation. The only operation which can be equated to a number is $\mathbf{O}^\circ = 1$. This proposition was really used in equation 3·02.

The following algebraic results are also frequently required:

$$K^\theta . K^\theta + K^{1+\theta} . K^{1+\theta} = \mathbf{O}^2 + (K)^2,$$
$$K^\theta . K^{-\theta} + K^{1+\theta} . K^{-1-\theta} = \mathbf{O}^2 - (K)^2; \qquad (2)$$
$$(K^\theta)^2 + (K^{-\theta})^2 = 2r^2\ sin^2\ \theta + 2\ cos\ 2\theta . \mathbf{O}^2,$$
$$(K^\theta)^2 - (K^{-\theta})^2 = 2\ sin\ 2\theta . \mathbf{O} . K; \qquad (3)$$
$$(K^\theta - cos\ \theta . \mathbf{O})^2 = sin^2\ \theta . (K)^2 = (r^2 - \mathbf{O}^2) . sin^2\ \theta,$$
$$(K^\theta)^2 - 2\ cos\ \theta . \mathbf{O} . K^\theta + \mathbf{O}^2 = r^2\ sin^2\ \theta; \qquad (4)$$

the subscript r being understood.

8·7. The *trigonometrical derivation* of K_r^γ is obviously as follows. Let a, b, c be the *lengths* of the sides of any triangle; let α, β, γ be the opposite angles, and r be the diameter of the circumscribed circle. Then

$$a = b\ cos\ \gamma + c\ cos\ \beta = b\ cos\ \gamma + \sqrt{c^2 - c^2\ sin^2\ \beta}$$
$$= b\ cos\ \gamma + \sqrt{r^2\ sin^2\ \gamma - b^2\ sin^2\ \gamma} = K_r^\gamma b;$$

that is $$a^2 - 2ab\ cos\ \gamma + b^2 = c^2 = r^2\ sin^2\ \gamma.$$

Of course the two terms of $K^\gamma b$ are respectively the orthogonal projections of b on a, and of Kb on a, the latter being also equal to the orthogonal projection of c on a.

8·8. What is the relation between K_r^γ and K_p^γ? Let $a = K_r^\gamma b$ and $p = mr$. Then

$$ma = mK_r^\gamma b = cos\ \gamma . mb + sin\ \gamma . \sqrt{m^2r^2 - m^2b^2}$$
$$= K_{mr}^\gamma mb = K_p^\gamma mb;$$

that is $$K_p^\gamma b = \frac{p}{r}K_r^\gamma\left(\frac{r}{p}b\right)$$

or $$K_p^\gamma = \left[\frac{p}{r}\mathbf{O}\right][K_p^\gamma]\left[\frac{p}{r}\mathbf{O}\right]^{-1}.$$

For example, from 3·7, $a = K_r^\gamma b = \frac{r}{c}K_{ey}^\gamma\frac{c}{r}b$, where $c = r\ sin\ \gamma$.

[1] The reason for this is that $[\mathbf{O}]^n = \mathbf{O}$ when n is an integer.

3·9. I conclude this section with two important propositions. *First* it will be seen from the above that these operations are in many ways freely associative and commutative with sines and cosines and with each other. Let A, B, C, . . . denote K_r^α, K_r^β, K_r^γ, . . . for short, so that $A^2 = K_r^{2\alpha}$, $B^2 = K_r^{2\beta}$, etc. Then, for example,

$$A = AB^{-1}B = \cos(\alpha-\beta) . B + \sin(\alpha-\beta) . K_rB. \qquad (1)$$

Therefore
$$\begin{aligned} A^2 &= \cos 2(\alpha-\beta) . B^2 + \sin 2(\alpha-\beta) . K_rB^2 \\ &= \{\cos^2(\alpha-\beta) - \sin^2(\alpha-\beta)\} . B^2 + 2\cos(\alpha-\beta)\sin(\alpha-\beta) . K_rB^2 \\ &= \cos^2(\alpha-\beta) . B^2 + 2\cos(\alpha-\beta)\sin(\alpha-\beta) . K_rB^2 \\ &\quad + \sin^2(\alpha-\beta) . K_r^2B^2 ; \qquad (2) \end{aligned}$$

while
$$\begin{aligned} (A)^2 &= \cos^2(\alpha-\beta) . (B)^2 + 2\cos(\alpha-\beta)\sin(\alpha-\beta) . B . K_rB \\ &\quad + \sin^2(\alpha-\beta) . (K_rB)^2 ; \qquad (3) \end{aligned}$$

so that the operative square (2) is similar in form to the algebraic square (3). For another example

$$A^2 + B^2 = [AB^{-1} + A^{-1}B]\,AB = 2\cos(\alpha-\beta) . AB \quad \text{(by 3.14)};$$

or
$$\begin{aligned} A^2 + B^2 &= (\cos 2\alpha + \cos 2\beta) . \mathbf{O} + (\sin 2\alpha \pm \sin 2\beta) . K_r \\ &= 2\cos(\alpha-\beta) . \{\cos(\alpha+\beta) . \mathbf{O} + \sin(\alpha+\beta) . K_r\} ; \end{aligned}$$

which is the same. Hence

$$A^2 - 2\cos(\alpha-\beta) . AB + B^2 = 0 ; \qquad (4)$$

while
$$(A)^2 - 2\cos(\alpha-\beta) . A . B + (B)^2 = r^2 \sin(\alpha-\beta) \quad \text{(by 3.64)}. \qquad (5)$$

From these we have

$$[A+B]\,[C+D] = AC + AD + BC + BD,$$
$$(A+B)\,(C+D) = A . C + A . D + B . C + B . D ;$$
$$[A+B]^2 = A^2 + 2AB + B^2, \qquad (A+B)^2 = (A)^2 + 2A . B + (B)^2 ; \quad (6)$$

and so on for any exponent. Equation (4) is interesting, because, though real, it is of the unreal algebraic form $y^2 - 2\cos\theta\, yx + x^2 = 0$, or $y = \cos\theta . x + \iota \sin\theta . x$. In fact, when $r = 0$, (4) degenerates to this algebraic statement, and the operative and algebraic equations 4 and 5 become the same. Now, as indicated in 5·2, A, B, C, . . . are the operator-chords of a circle, and when $r = 0$ that circle is a point-circle—which explains the periodicity of "complex" numbers. They are indeed merely special cases of the theorems here outlined.

Applications to the Theory of Equations are evidently possible, but they cannot be examined here.

The *second* proposition is that by an elementary theorem regarding surds, an equation between A, B, C, . . . must break up into two equations between the rational parts and the irrational parts respectively—which is by no means a property of complex numbers only. For example, by 6·2, the equation $ABC = K_r^2$ denotes any triangle in "right sequence," so that

$$\cos(\alpha+\beta+\gamma) . \mathbf{O} + \sin(\alpha+\beta+\gamma) . K_r = -\mathbf{O} ; \qquad (7)$$

hence $\cos(\alpha+\beta+\gamma) = -1$ and $\sin(\alpha+\beta+\gamma) = 0$; that is, $\alpha+\beta+\gamma = 2$.

4.0. For the *geometrical interpretation* of the K-operations, draw two circles, both of r diameter, touching each other ; and let one circle be called the *positive* circle and the other the *negative* circle. Let their point of contact be the origin ; and from it draw positive and negative *tangents* t and $-t$, and the two diameters r and $-r$. Then it is obvious that we can draw *from* the origin *outwards four chords* of the same length, one on each side of the diameter of the positive circle (a and a' in Figure 1) and one on each side of the diameter of the negative circle ($-a$ and $-a'$).

We may call a and a' *counter-chords* (for a reason which will appear); and $-a$ and $-a'$, the same.

Let the angles made by a, a' with the positive tangent be called the *chord-angles* of these chords; and the same for any chords; and denote the corresponding chord-angles by Greek letters. By Euclid III, 32, all angles subtended by a chord in the alternate arc (to its left) are equal to the chord-angle; but they may also be called *arc-angles*. The same will apply to $-a$ and $-a'$ if we use the generalised idea of an angle. All angles are positive when taken counter-clockwise.

Chords drawn with the centre of the circle on the left may be called *right* chords (a); and those drawn with the centre on the right, *left* chords (a'). In the positive circle, the chord-angles of right chords are acute, those of left chords obtuse; and the sum of the chord-angles of two counter-chords is two right angles—that is, in the case of a and a' the angles $\alpha + \alpha' = 2$ in Quadrantal Measure.

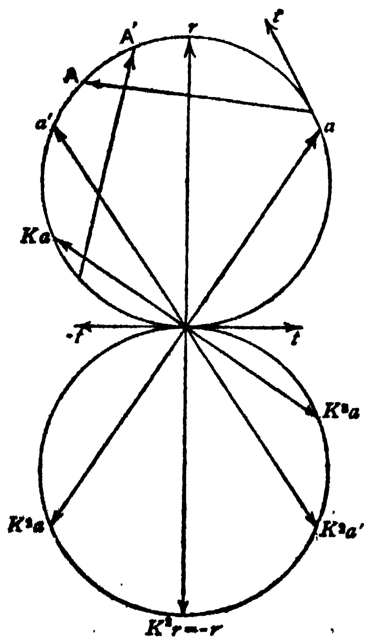

FIG. 1.

From any point in the positive circle a *third chord* may be drawn such that its length is equal to the lengths both of a and of a'. Its chord-angle (namely, the angle which it makes with a positive tangent drawn from its own point of origin) must equal either α or α', but *not both*. By rotating the circle containing it, it can be brought to coincide either with a or with a', but *not* with *both*. We shall here assume therefore that all chords of equal positive circles which have equal chord-angles are themselves equal. For example, if a chord be drawn from the end of a equal in length to a but in a reversed direction backwards to the origin, then it equals, not a nor $-a$, but a'.

Any two chords which begin or end at the same point may be *coinitial*, *or coterminous*, or *sequent* at that point; and by rotating the circle containing one of the chords it can be brought into either of these three relations with the other chord.

We assume (for brevity), regarding any chord a, that $a = r \sin \alpha$.

The tangents t and $-t$ may be considered to be counter-chords of zero length, with chord-angles 0 and 2 respectively, and may be denoted by 0 and -0, or $0'$ respectively.

With these definitions the K-operations can now be employed for any geometrical problem; but I have space only to indicate the first steps.

4.1. (1) *To prove* that r is its own counter-chord. For the chord-angle of r is 1, and of r' is $2-1$. Therefore it is immaterial in which direction r be drawn, whether from the origin or toward it. So also for $-r$.

(2) *To prove* that $r = K_r 0$ and that $0 = K_r^{-1} r$. For

$$K_r 0 = \cos 1 \,.\, 0 + \sin 1 \,.\, \sqrt{r^2 - 0^2} = r;$$

$$K_r^{-1} r = \cos(-1) \,.\, r + \sin(-1) \,.\, \sqrt{r^2 - r^2} = 0;$$

if we take the positive value of the radical (see 3·04)

(3) *To prove* that $a = K_r^a o = K_r^{a-1} r$. For

$$K_r^a o = \cos a \,.\, o + \sin a \,.\, \sqrt{r^2 - o^2} = r \sin a = a;$$
$$K_r^{a-1} r = \cos (a - 1) \,.\, r + \sin (a - 1) \,.\, \sqrt{r^2 - r^2} = r \sin a = a.$$

If a be a right chord the angle of rotation from r to a is a negative angle, namely $1 - a$ reversed. If a be a left chord, denote it by a' and its chord angle by a'. Then $a' - 1$ is a positive angle.

We can obtain either of the equations in the enunciation from the other. For if $a = K_r^{a-1} r$, then, since $r = K_r o$, $a = K_r^{a-1} K_r^1 o = K_r^a o$; and if $a = K_r^a o$ it also equals $K_r^a K_r^{-1} r$.

(4) *To prove* that if $a, b, c, \ldots$ be any chords of the same or of equal circles and $\alpha, \beta, \gamma, \ldots$ be their respective chord-angles, then $K_r^{-a} a = K_r^{-\beta} b = K_r^{-\gamma} c$, etc. For

$$a = K_r^a o, \qquad b = K_r^\beta o, \qquad c = K_r^\gamma o, \textit{ etc.}$$

therefore $\qquad o = K_r^{-a} a = K_r^{-\beta} b = K_r^{-\gamma} c = \textit{etc.},$

and $\qquad r = K_r^{1-a} a = K_r^{1-\beta} b = K_r^{1-\gamma} c = \textit{etc.}$

(5) *To prove* that, if a and b be any two chords of the same or of equal circles and θ be the angle between them, then $a = K_r^\theta b$.

For $\qquad K_r^{-a} a = K_r^{-\beta} b$, and $a = K_r^{a-\beta} b$;

and $\theta = a - \beta$.

(6) *To prove* certain relations between counter-chords. Let a and a' be counter-chords and a and a' their respective angles. Then by 4·0, $a + a' = 2$, and

$$a' = K_r^{a'-a} a = K^{2-2a} a = - K^{-2a} a,$$
$$a = K_r^{a-a'} a' = K_r^{2-2a'} a' = - K^{-2a'} a';$$
$$a' = K_r^{1-a} r, \qquad r = K_r^{1-a} a;$$
$$K_r^a a' = K_r^{a'} a;$$
$$a' = K^{2-a} o, \qquad a = K_r^a o.$$

From this last equation we have it that the *length* of a' is $r \sin (2 - a)$, and of a is $r \sin a$. These *lengths* are equal, but this does not mean that the chords themselves are equal (see 4·0).

4·2. *Remarks.* (1) There *seems* to be a discrepancy here; for if $a = r \sin a$ and $a' = r \sin (2 - a)$, then $a = a'$. We might have used Hamilton's tensor-symbol and have written

$$Ta = TK_r^a o = r \sin a;$$

but really we should not employ the trigonometrical ratios at all until we finally come to put our operative analysis into scalars; and I have employed them here merely for the purpose of giving a simple exposition. We can save unnecessary symbols by remembering that $\sin \theta = \sin (2 - \theta)$ and $\cos \theta = \cos (- \theta)$ *only for scalar values.*

(2) In place of o and − o for the zero-counter-chords we can write t and t' or even dt and dt'. I prefer the first.

(3) It seems curious that a reversed chord is not a negative chord; but the supposition that it is so leads to immediate discrepancies. The reversed chord is really the counter-chord.

(4) The effects of repeated operations with K_r will be easily gathered from Figure 1. If a be a right chord, $K_r a$ will be a left chord drawn from the

origin normal to a; but it is not of the same length unless $\alpha = \frac{1}{2}$. By 3·11, $K_r^2 a = -a$, and is a chord in the negative circle equal in length to a but drawn from the origin in the opposite direction; and $K_r^3 a$ is the negative of $K_r a$ and equals $K_r^{-1} a$, both being in the negative circle. Similarly K_r^{-2}, K_r^{-3} turn a chord through two, or three, negative right angles.

(5) Of course any chord may be considered to be the radius-vector (capable of negative values) of the double circle of Figure 1 referred to polar co-ordinates with the pole at the origin and the initial line along the positive tangent t.

(6) In 3·0 it was shown analytically that $K^\gamma K^\theta$ equals either $K^{\gamma+\theta}$ or $K^{\theta-\gamma}$, the ambiguity being due to that of the sign of $\sqrt{r^2 - o^2}$ (using K here in a purely algebraic sense and not as subsequently defined). The explanation of this is now easy; for as *two* counter-chords of the same length may be

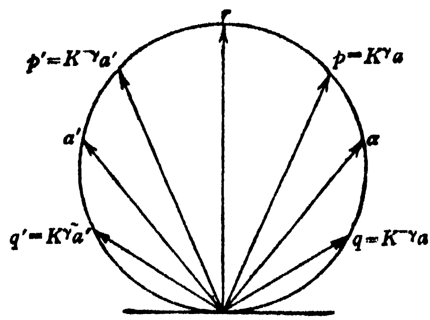

FIG. 2.

drawn from any point of a circle it follows that operation on both of them by the same operator K^γ will have different results. Let a be a right chord and a' its left counter-chord; let γ be positive; and let $p = K^\gamma a$ and $q' = K^\gamma a'$. Then $p = K^{\gamma+\alpha} o$, and $q' = K^{\gamma+2-\alpha} o$. That is $q' = K^{2-(\alpha-\gamma)} o$, and is a left chord and the counter-chord of q where $q = K^{\alpha-\gamma} o$; while the counter-chord of p is $p' = K^{2-\alpha-\gamma} o$. Hence the analytical equations of 3·0, being concerned only with lengths, are, so to speak, forced to give ambiguous results (Figure 2).

(7) With this explanation we see that K_r^θ *always turns a subject chord in the same direction*—namely counter-clockwise if θ be positive.

5·0. We may now touch upon the *operative ratios* and *operative logarithms* of chords suggested in 1·11 and 1·14.

If a and b be two fixed numbers, such as the co-ordinates of a point, then $\frac{a}{b} (= \phi)$ denotes *any* curve ϕ passing through that point; but if y and x be variables susceptible of all values, then $\frac{y}{x}$ denotes a *specific* curve ϕ passing through an infinite series of points (see 7·1).

If $a//b = \phi$ and $c//d = \psi$ then

$$\frac{a}{b}\frac{c}{d} = \frac{c}{d}\frac{a}{b} = \phi\psi = \psi\phi \qquad (1)$$

only if ϕ and ψ be permutable. This is the case if $\phi = \chi^n$ and if $\psi = \chi^m$,

say, since $\chi^n\chi^m = \chi^m\chi^n$. It will also be the case with all the operative ratios dealt with here, which will always represent some operative power of K_r.

For all numbers we assume that

$$\frac{a}{b}\frac{b}{c} = \frac{a}{c} \text{ and } \frac{b}{a} = \left[\frac{a}{b}\right]^{-1}; \qquad (2)$$

but for all chords we also have by 4·15

$$\frac{a}{b} = K^{\alpha-\beta}, \qquad \frac{c}{d} = K^{\gamma-\delta}; \qquad (3)$$

therefore $$\frac{a}{b}\frac{c}{d} = \frac{c}{d}\frac{a}{b} = \frac{a}{d}\frac{c}{b} = \frac{a//b}{d//c} = \frac{a//d}{b//c} = \frac{a}{e}\frac{c}{e}\frac{e}{b}\frac{e}{d}, \text{ etc.}, \qquad (4)$$

where e is any fifth chord. But I shall not here use such expressions as $[a]\ [c]$ because these require considerable discussion.

5·1. We may also use operative logarithms as suggested in 1·14, so that

$$l_K\frac{a}{b}\frac{c}{d}\frac{e}{f} \ldots = l_K\frac{a}{b} + l_K\frac{c}{d} + l_K\frac{e}{f} + \ldots$$
$$= \alpha - \beta + \gamma - \delta + \epsilon - \zeta + \ldots \qquad (1)$$

5·2. Since we can always interpolate any third chord between the two chords of an operative ratio, that third chord may be the positive zero-chord if we wish; so that

$$\frac{a}{b} = \frac{a}{o}\frac{o}{b},$$

and $$\frac{a}{b}\frac{c}{d}\frac{e}{f} \ldots = \frac{a}{o}\frac{o}{b}\frac{c}{o}\frac{o}{d}\frac{e}{o}\frac{o}{f} \ldots \qquad (2)$$

By 4·13, $a = K_r^\alpha o$, $b = K_r^\beta o$, $c = K_r^\gamma o$, etc. Hence

$$\frac{a}{o} = K_r^\alpha, \qquad \frac{b}{o} = K_r^\beta, \qquad \frac{c}{o} = K_r^\gamma; \qquad (3)$$

and $$\alpha = l_K\frac{a}{o}, \qquad \beta = l_K\frac{b}{o}, \qquad \gamma = l_K\frac{c}{o}. \qquad (4)$$

The meaning of this is that we can reduce ratios between any two chords to ratios between each separate chord and the zero-chord; and that the operative logarithms of these latter ratios are always the corresponding chord-angles. I therefore denote the ratio of any chord to the zero-chord by the capital of the letter which denotes the former chord, so that for instance

$$A = \frac{a}{o} = K_r^\alpha, \qquad l_K A = \alpha;$$

and $$\frac{a}{b}\frac{c}{d}\frac{e}{f} \ldots = AB^{-1}CD^{-1}EF^{-1} \ldots \qquad (5)$$

Hence, while a, b, c . . . are *directed lengths* measured by numbers, A, B, C, . . . are the corresponding *K-operations*, possessing each a geometrical relation with its respective chord; and we may usefully call them *operator-chords.*

Of course juxtaposition and indices affect A, B, C, . . . as they affect all operations—i.e. operatively and not algebraically.

Two important operator-chords are those of the *diameter* r and of the *zero-chord* itself. The former may be denoted by R—so that $R = \frac{r}{o} = K_r^\pi$;

and as the chord-angle of r is unity, we have $l_K R = 1$. The operative ratio of the zero-chord to itself must be the same as that of any quantity or operation to itself—that is $\frac{a}{a} = \mathbf{O}$. Hence also $\frac{o}{o} = \mathbf{O}$; that is, *the unit of operation* $\mathbf{O}$ *is the operator-chord corresponding to the zero-chord.* Its chord-angle is zero.

Just as we denote two counter-chords by a and a' so we can denote two counter-operator-chords by A and A'.

(These ideas can be extended beyond the present subject.)

5·3. The following formulæ can be immediately verified and are frequently required :

$$\frac{a'}{r} = \frac{r}{a}, \qquad \frac{a'}{b} = \frac{b'}{a}, \qquad \frac{a}{b'} = \frac{b}{a'}, \qquad \frac{a'}{b'} = \frac{b}{a};$$

or

$$AA' = BB' = RR' = R^2 = -\mathbf{O}, \text{ or } A' = -A^{-1}. \quad Rr = -o. \tag{1}$$

$$AB = BA,\ Ab = Ba. \quad \text{If } A = BC,\ a = Bc = Cb. \tag{2}$$

$$Ao = r \sin a = \mathbf{O}a = a; \qquad Ar = r \cos a = Ra = +\sqrt{r^2 - a^2}. \tag{3}$$

$$A = R^a = \cos a . \mathbf{O} + \sin a . R; \qquad \mathbf{O}r . A = Ar . \mathbf{O} + Ao . R. \tag{4}$$

$$\mathbf{O}r . AB = Ar . \mathbf{O}B + Ao . RB = Br . \mathbf{O}A + Bo . RA = etc. \tag{5}$$

$$r . ABo = Ar . Bo + Ao . Br = Ar . \mathbf{O}b + Ao . Rb = etc. \tag{6}$$

$$r . ABr = Ar . Br - Ao . Bo = Ar . Rb - Ao . \mathbf{O}b = etc. \tag{7}$$

$$A . B = Ao . Bo + AB . \mathbf{O} = Ar . Br + RAB . R. \tag{8}$$

$$A . BC - B . AC = Ao . BCo - Bo . ACo = Ar . BCr - Br . ACr = Co . AB^{-1}o. \tag{9}$$

$$[A^2 + B^2]\ [C^2 + D^2] = A^2C^2 + A^2D^2 + B^2C^2 + B^2D^2 = 2 \cos (\alpha - \beta) \cos (\gamma - \delta) . ABCD. \tag{10}$$

(See also 3·2, 3·4, 3·5, 3·6, and 3·9.)

6·0. The geometrical application of the K-operations naturally commences with the subject of rectilinear figures in circles—a definition which includes all triangles. I have space only for a few notes.

However any chords may be placed in any circle the identity of 4·14 always holds good, namely that

$$a = K_r^{\alpha - \beta} b \qquad \text{or} \qquad Ba = Ab.$$

Any series of *sequent* chords (4·0) in a circle may be in *right*, or *positive*, rotation, as in Figure 3 ; or in *left*, or *negative*, rotation ; or in *changed* rotations. This does not mean that all chords in right rotation are right chords ; or in left rotation are left chords.

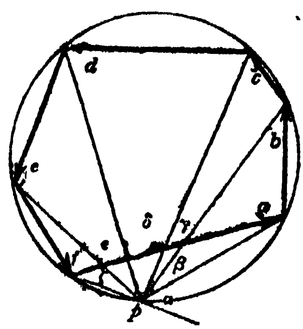

FIG. 3.

6·1. If the n chords $a, b, c, d, \ldots$ form a convex closed figure in right rotation (Figure 3), then, by drawing chords to all the angles of the figure from any point on the circumference of the containing circle, we see that the sum of the arc-angles (that is, of the chord-angles) of the chords of the figure is equal to two right angles ; so that

$$\alpha + \beta + \gamma + \delta + \ldots = 2; \tag{1}$$

therefore

$$l_K \frac{a}{o} \frac{b}{o} \frac{c}{o} \frac{d}{o} \cdots = 2; \tag{2}$$

and

$$ABCD \ldots = R^2. \tag{3}$$

Hence $A' = BCD \ldots$, $\quad B' = ACD \ldots$, $\quad C' = BAD \ldots$, etc. $\quad$ (4)

If we take the same figure in left rotation we must replace all the chords in the above equations by their counter-chords; so that

$$A'B'C'D' \ldots = R^{2n-2}. \tag{5}$$

Symmetrical formulæ in terms of the chords are

$$\frac{a}{r}\frac{b}{r}\frac{c}{r}\ldots = \left[\frac{o}{r}\right]^{n-2}, \qquad \frac{a'}{r}\frac{b'}{r}\frac{c'}{r}\ldots = \left[\frac{o}{r}\right]^{2-n}. \tag{6}$$

The reader can easily deal with re-entrant polygons. Suppose that we already have the right-rotation polygon $ABCD = R^2$ and wish to add an outside triangle in place of the side D, then this triangle must be (by the following section) $D'EF = R^2$, or $D = EF$. Hence the enlarged polygon is $ABCEF = R^2$. If the added triangle is to be re-entrant, we shall have $DEF = R^2$; whence $ABC = EF$, or $ABCE'F' = R^2$.

6·2. The equation for *any triangle* in right rotation is

$$ABC = R^2; \tag{1}$$

whence $A' = BC$, $B' = CA$, $C' = AB$; (2)

$R^2A = B'C'$, $R^2B = C'A'$, $R^2C = A'B'$; (3)

or $a' = Bc = Cb$, $b' = Ca = Ac$, $c' = Ab = Ba$, *etc.*, (4)

give each side in turn, a' and b, b' and c, etc., being co-initial. The various scalar equations for the solution of triangles are supplied by these, by the variations noted in 5·2, and by others which I have no space for. The left-rotation triangle is $A'B'C' = R^4$.

For the *right-angled triangle*, since one side, say c, must be a diameter, we have

$AB = R$, $A' = RB$, $B' = RA$, (5)

$\alpha + \beta = 1$, $A'B' = R^3$. (6)

For *the isosceles triangle*, if the sides a and b are equal (they cannot be counter-chords)

$A^2 = C' = B^2$, $2\alpha = \gamma' = 2\beta$; (7)

and for the *equilateral triangle*

$A^3 = R^2$, $A' = A^2$, $3\alpha = 2$. (8)

Of course in all these figures r is the diameter of the circumscribed circle. The scalars of $a = Ao$, $b = Bo$, ... give the equations $a = r \sin \alpha$, $b = r \sin \beta$, ... which are the properties of all chords of circles, not merely of triangles.

All the above equations must apply equally to the point-circle and to its infinitesimal polygons (1.12); and I denote its operator-chords by A_o, B_o, C_o, ...

All the equations may also be presented in terms of R only, since $A = R^\alpha$, $B = R^\beta$, etc. The notation to be employed is suggested by the problem under consideration. It is often easiest to form first the equation between the angles, as in 6·11; or at other times to commence with chord-ratios. When a and b are co-initial the relation $\frac{a}{b} = \frac{c}{o} = C$, where c is the third side of the triangle, is especially useful.

6·3. Hitherto we have dealt only with chords of the same or of equal circles. Those of *different circles* are easily connected by the relation shown in 3·8, between K^γ_r and K^γ_p. Operator-chords of different circles are denoted by A_r, A_p, B_r, B_p, etc.; and A_a, B_b, C_c, ... are diameters of circles drawn round a, b, c, ... and are therefore self-reversible.

It is geometrically obvious that any proposition regarding chords of one

circle holds regarding the similar chords of any other circle. If $a = K_r^{\gamma} b$, then $ma = K_{mr}^{\gamma} mb$; so that

$$mK^{\alpha}K^{\beta}K^{\gamma} \ldots = K_m^{\alpha} mK^{\beta}K^{\gamma} \ldots = K_m^{\alpha}K_m^{\beta}K_m^{\gamma} m\mathrm{o} \ldots;$$

or
$$mABC \ldots = A_m B_m C_m m \ldots; \qquad (1)$$

and conversely
$$A_m m\mathrm{o} = mA.$$

Or if $\frac{a}{\mathrm{o}}\frac{b}{\mathrm{o}}\frac{c}{\mathrm{o}} = \frac{r}{\mathrm{o}}\frac{r}{\mathrm{o}}$, then $m^3\frac{a}{\mathrm{o}}\frac{b}{\mathrm{o}}\frac{c}{\mathrm{o}} = m^2\frac{r}{\mathrm{o}}\frac{r}{\mathrm{o}} = \frac{ma}{\mathrm{o}}\frac{mb}{\mathrm{o}}\frac{mc}{\mathrm{o}} = \frac{m\mathrm{o}}{\mathrm{o}}\frac{mr}{\mathrm{o}}\frac{mr}{\mathrm{o}}$,

and $\frac{m\mathrm{o}}{\mathrm{o}} = \frac{\mathrm{o}}{\mathrm{o}} = \mathrm{o}$. Also $A_p = \frac{p}{r}A_r\frac{r}{p}\mathrm{o}$. $\qquad$ (2)

6·4. I have only sufficient space to indicate the procedure by a simple example. Draw any triangle a, b, c in the r-circle and a perpendicular p between one of the angles α and the opposite side a. Draw circles round the

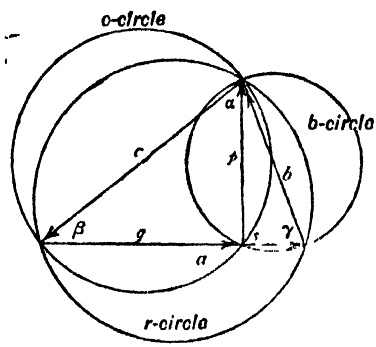

FIG. 4.

other two sides b and c as diameters: then the perpendicular p is the radical-axis of these two latter circles. Let the lines be directed any way we please—as, for example, in Figure 4. Let p divide a into two parts q and s; and denote reversed chords (not necessarily *left* chords) by accents. Then we have in Figure 4 three right-rotation triangles which are, by the useful formula of 6·04,

$$\frac{a}{r}\frac{b}{r}\frac{c}{r} = \frac{\mathrm{o}}{r}, \qquad \frac{q}{c}\frac{p}{c} = \frac{\mathrm{o}}{c}, \qquad \frac{p'}{b}\frac{s}{b} = \frac{\mathrm{o}}{b}\left(\text{or } \frac{p}{b} = \frac{s}{\mathrm{o}}\right).$$

What are the relations between these six lines? The angles β and γ are common to two versors (chord-ratios) each.

Hence $\qquad p = \frac{c}{q}\mathrm{o} = \frac{c'}{q}\mathrm{o} = K_c^{\beta}\mathrm{o} = \frac{c}{r}K_r^{\beta}\frac{r}{c}\mathrm{o} = \frac{c}{r}K_r^{\beta}\mathrm{o} = r\sin\gamma\sin\beta.$

Also $\qquad p = \frac{s}{o}b = K_b^{1-\gamma}b = bK_1^{1-\gamma}1 = b\cos(1-\gamma) = r\sin\beta\sin\gamma.$

And $\qquad q = \frac{o}{p}c = K_o^{-\beta}c = cK_1^{-\beta}1 = c\cos(-\beta),$

and $\qquad s = \frac{p}{b}\mathrm{o} = \frac{b'}{p'}\mathrm{o}$ (by 5·31) $= K_b^{1-\gamma}\mathrm{o} = K_b^{-\gamma}b = b\cos(-\gamma).$

Therefore $\qquad a = q + s = \frac{c}{p}\mathrm{o} + \frac{p}{b}\mathrm{o} = K_o^{-\beta}c + K_b^{-\gamma}b = etc.$

To verify the last result, we have

$$K_c^{-\beta}c + K_b^{-\gamma}b = K_c^{\beta}c + K_b^{\gamma}b \text{ (by 3·21)} = \frac{c}{r}K_r^{\beta}r + \frac{b}{r}K_r^{\gamma}r$$

$$= \frac{1}{r}K_r^{\gamma}o \,.\, K_r^{\beta}r + \frac{1}{r}K_r^{\beta}o \,.\, K_r^{\gamma}r = K_r^{\gamma+\beta}o \text{ (by 3·24)}$$

$$= K_r^{2-a}o = K_r^{a}o \text{ (scalars)} = a.$$

The assumption that $K_c^{-\beta}c = K_c^{\beta}c$ is often useful, but it is allowable only when we are finally reducing to scalars. But as the *length* c equals either $\frac{1}{r}K_r^{\gamma}o$ or $\frac{1}{r}K^{2-\gamma}o$, we also have

$$a = -K_c^{2-\beta}c - K_b^{2-\gamma}b = -\frac{1}{r}K_r^{2-\gamma}o \,.\, K_r^{2-\beta}r - \frac{1}{r}K_r^{2-\beta}o \,.\, K_r^{2-\gamma}r = -K_r^{2+a}o = a;$$

or $a = -\frac{1}{r}C'o \,.\, B'r - \frac{1}{r}C'r \,.\, B'o = -B'C'o$ (by 3·24 and 5·36);

therefore (see 6·23) $\qquad R^2A = B'C'.$

6·5. These equations are put in various ways as exercises; and each should be compared with the figure. The lines may be given other directions than those shown. The symbols are freely commutative, but the zero-chords are not the same as their scalar, zero. The reader may compare the perpendiculars from all the angles and should remember that

$$[A, B, C, \ldots]S = [AS, BS, CS, \ldots]; \text{ e.g. } [A \,.\, B \,.\, C \,.]S = AS \,.\, BS \,.\, CS.$$

By putting $S = o$, or $= r$, we get the corresponding scalars. Observe the method used for denoting a polybasic function.

Extension to three dimensions and some important formulæ are required to complete this geometry. As regards both it has a rough resemblance to quaternions; but, nevertheless, it is really only ordinary trigonometry put in another way.

7·0. I have no space to pursue details and practical applications any further, but must conclude with some necessary general notes.

Trigonometry is dominated by two great Secondary Operations, namely,

$$H \equiv [\mathbf{o}^2]^{-1}[\mathbf{o} - 1][\mathbf{o}^2] = [\mathbf{o}^2]^{-1}[1 + \mathbf{o}]^{-1}[\mathbf{o}^2]; \qquad (1)$$

$$K \equiv [\mathbf{o}^2]^{-1}[1 - \mathbf{o}][\mathbf{o}^2]$$

$$= [\mathbf{o}^2]^{-1}[\mathbf{o}^{-1}][1 + \mathbf{o}][\mathbf{o}^{-1}][1 + \mathbf{o}]^{-1}[\mathbf{o}^{-1}]^{-1}[\mathbf{o}^2]. \qquad (2)$$

Both of them are of the $\zeta\xi\zeta^{-1}$ type ("conjugates," etc. in Group Theory), of which ξ may be called the *nucleus* and ζ and ζ^{-1} the *alae*, and the general property of which is that $[\zeta\xi\zeta^{-1}]^n = \zeta\xi^n\zeta^{-1}$. Thus, though apparently very similar, H (hyperbola) and K (Circle) can now be seen to possess quite distinct molecular structures, the nucleus of H being merely the invert of the First Primary Operation,[1] while that of K is the Third Primary—so that the iteration of it gives periodic results.

By attaching $r\mathbf{o}$ and its invert $r^{-1}\mathbf{o}$ as additional alae we have

$$H_r = [r\mathbf{o}][H][r\mathbf{o}]^{-1} = \sqrt{\mathbf{o}^2 - r^2},$$

$$K_r = [r\mathbf{o}][K][r\mathbf{o}]^{-1} = \sqrt{r^2 - \mathbf{o}^2}. \qquad (3)$$

The *iteration* of H is obtained immediately, since $[\mathbf{o} - 1]^n = \mathbf{o} - n$; so that $H_r^n = \sqrt{\mathbf{o}^2 - nr^2}$. That of K_r was given in 3·0. The following relation holds between H and K, namely

$$K_r = H_q^n K_r H_q^n, \qquad (4)$$

[1] The Primary Operations are derived successively from $1 + \mathbf{o}$, each by one *iteration* (including inversion) and one *change of base*. The higher ones appear to have been scarcely studied as yet.

showing that K is invariant between H^n and H^n. Also

$$\mathbf{O}^2 - (H_r)^2 = r^2 = \mathbf{O}^2 + (K_r)^2; \qquad (5)$$

and, as $K_r H_q = K_q H_r$ generally, $K_q = K_0 H_q$, where $K_o = \sqrt{-1} \, . \, \mathbf{O}$. (6)
The trigonometrical ratios are compared as follows:

$$sin\, \mathbf{O} = \mathbf{O}^{-1}\, cosec\, \mathbf{O} = [1 - \mathbf{O}]\, covers\, \mathbf{O} = K^{-1} cos\, \mathbf{O} = K^{-1}\mathbf{O}^{-1}\, sec\, \mathbf{O}$$
$$= \mathbf{O}^{-1}H^{-1}\, cot\, \mathbf{O} = \mathbf{O}^{-1}H^{-1}\mathbf{O}^{-1}\, tan\, \mathbf{O} \ \left(= \frac{\mathbf{O}}{K}\, tan\, \mathbf{O}\right). \qquad ($$

Thus if we require $\dfrac{sec\, \mathbf{O}}{cosec\, \mathbf{O}}$ we have $\dfrac{sec\, \mathbf{O}}{cosec\, \mathbf{O}} = \dfrac{sec\, \mathbf{O}}{sin\, \mathbf{O}} \dfrac{sin\, \mathbf{O}}{cosec\, \mathbf{O}} = \mathbf{O}^{-1}K\mathbf{O}^{-1} = \dfrac{\mathbf{O}}{H}$.

Similarly $H = \dfrac{tan\, \mathbf{O}}{sec\, \mathbf{O}}$; and $\mathbf{O}^{-1} = \mathbf{O}^2\mathbf{O}^{-1}\mathbf{O}^{\frac{1}{2}}$; $1 - \mathbf{O} = \mathbf{O}^{-1}[1 + \mathbf{O}]\mathbf{O}^{-1}[\mathbf{O} - 1]\,\mathbf{O}^{-1}$;
and $K = \mathbf{O}^{-1}H^{-1}\mathbf{O}^{-1}H\mathbf{O}^{-1}$. (8)

Trigonometrical solutions (including inversions) are generally most easily dealt with in this way; but I also employ another symbol $T = -\mathbf{O}^{-1}\left(= -\dfrac{1}{\mathbf{O}}\right)$, which has operative roots resembling those of K; for, if $\theta = n$ in Quadrantal Measure, then

$$T^\theta = [-\mathbf{O}^{-1}]^\theta = \frac{sin\, \theta \, . \, T - cos\, \theta}{cos\, \theta \, . \, T + sin\, \theta}. \qquad (9)$$

7·1. In applying operative methods to Cartesian Geometry, I denote a curve, not by a *function* $y = \phi x$, but by the *operation* ϕ alone. Thus $\mathbf{O}$ is the mid-axis ($y = x$); $0\mathbf{O}$ and $\infty\mathbf{O}$ are the axes of x and y; $a + b\mathbf{O}$ is a straight line; and K_1 is the circle of unit *radius* described round the origin as centre. To change a unit-circle into an r-circle we attach the alae $r\mathbf{O}$ and $r^{-1}\mathbf{O}$. To transfer the circle so that its centre shall be at the point (X, Y) we attach the additional alae $Y + \mathbf{O}$ and $\mathbf{O} - X$; so that now

$$y = [Y + \mathbf{O}]\,[r\mathbf{O}]\,[K]\,[r\mathbf{O}]^{-1}[X + \mathbf{O}]^{-1}x, \qquad (1)$$

and

$$x = [X + \mathbf{O}]\,[r\mathbf{O}]\,[K]^{-1}\,[r\mathbf{O}]^{-1}\,[Y + \mathbf{O}]^{-1}y. \qquad (2)$$

The ellipse with centre at origin, namely mK, can be diminished or enlarged or translated in precisely the same way; and so can H or any other curve ϕ. To rotate ϕ about the origin through the angle θ, we have, if ϕ_θ is the curve when rotated,

$$[cos\, \theta \, . \, \mathbf{O} + sin\, \theta \, . \, \phi_\theta][cos\, \theta \, . \, \mathbf{O} - sin\, \theta \, . \, \phi] = \mathbf{O}. \qquad (3)$$

For if $y = \phi x$ and $y' = \phi_\theta x$, then by the familiar rules

$$y = K_R x = K_R^\theta y' = K_R^{\theta+1} x',$$
$$y' = K_R x' = K_R^{-\theta} y = K_R^{1-\theta} x; \qquad (4)$$

where $R^2 = [\mathbf{O}^2 + (\phi)^2]x = [\mathbf{O}^2 + (\phi_\theta)^2]x'$, and R is the radius-vector of a point on ϕ and of the similar point on ϕ_θ. To find ϕ_θ we must solve the equation (3). To rotate the axes, put $-\theta$ for θ in the above; and changes of co-ordinates are effected in much the same way. Easy geometrical constructions exist for illustrating ϕ^{-1}, ϕ^n, $\phi\psi$, $\phi \, . \, \psi$, etc.; and equations are solved when necessary, by iteration or operative division as shown in my papers 2 and 3.

7·2. Only the *principal* nth root of K has been considered here; but it is clear from 3·03 and from 4·26 that other values exist; and they seem to be obtainable in just the same way as the nth algebraic roots of $K_0 1$—so that, for instance, K has the two operative square roots $K^{\frac{1}{2}}$ and $K^{\frac{5}{2}}$ (or $-K^{\frac{1}{2}}$), and

the three operative cube roots $K^{\frac{1}{3}}$, $K^{\frac{4}{3}}$, and $K^{\frac{7}{3}}$, all of which are different. But these operative roots are real and do not involve the imaginary unit. In the sense of 1·05, however, the algebraic roots of $K_0 1$ are given by the operative roots of K_0.

7·3. This paper has been the briefest possible introduction to a geometry, suggested by the K-operations, which is almost as simple and elegant as Hamiltonian geometry, but which has the advantage of not being based upon any preliminary assumption—indeed, I repeat, the whole matter here set forth is nothing but ordinary trigonometry put in the form of operator and operand. I hope to show in a subsequent paper its relation both with Hamilton's and with Grassmann's systems, as well as with "complex numbers." Years ago I attempted to prove (1) that these systems can be combined and consolidated by the introduction of a single scalar unit which is common to both but the existence of which was overlooked by the authors and, apparently, by subsequent writers. The present results obviously suggest further developments in the same direction; but the exposition requires another paper.

7·4. The theorem 1·02 may be proved in several ways. It is the property of four numbers which possess, two and two, the same modulus. Geometrically, it can be derived at once from Ptolemy's Theorem—that the product of the diagonals of a quadrilateral in a circle equals the products of opposite sides, two and two (M. Halma's *Almagest*, 1813, vol. i, page 29, or H. M. Taylor's *Euclid*, III, 37B). See also my paper 4—*examples*.

7·5. As K_r^θ occurs frequently in geometry, many geometrical interpretations of it may be given. Perhaps that of 4·0 is the most fertile; but it may also be presented as the operative ratio of the co-ordinates of rotation, as indicated in 7·14. This interpretation is connected with that of 4·0 by the method of 6·4.

7·6. Operative analysis seems to reduce everything to the two fundamental processes, *iteration* (including inversion) and *change of base*, and I venture to think that it will eventually replace our present methods of working. Functions are not mere jumbles of quantities put together anyhow, but possess, so to speak, molecular as well as atomic structure. Operative procedure enables us to apply group-theory immediately to every problem.

7·7. The fact that ϕ° does not equal numerical unity applies to all cancelled operations (except $\mathbf{O}^\circ$), such as Δ°, Σ°, D°. I hope to show later how recognition of this fact consolidates Boole's differential symbolism, and how it can be applied in many directions, as for determinants, invariants, series, and the Calculus.

7·8. The names *Real Quaternion* or *Operative Quaternion* may be suggested for K_r^θ; but it is better to reserve decision on this for the present.

May 13, 1921.

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1. The Algebra of Space (*G. Philip & Sons, London*, 1901).
2. "Verb-Functions" (*Proc. R. Irish Acad.*, XXV, A, 1905).
3. "The Solution of Equations by Operative Division" (*Science Progress*, Vol. X, Nos. 38, 39, 40, 1915–16).
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MENTAL PROCEDURE AND THE CONCEPT OF NATURE

(Joshua C. Gregory, B.Sc.)

It may be misleading to say, with Sir Martin Conway, that "Any fact may be chosen as the centre of all knowledge," and suggestive of error to add, with him, "Any study, if pursued to the end, leads on to all other studies."[1] The biologist wanders naturally into psychology, because animals seem to be conscious, and the psychologist can hardly avoid metaphysics: such sequences of studies are obvious and inevitable. Geographical analogy, however, may induce error if we think that knowledge is like a territory which can be traversed, with finally similar results, from any starting-point. A traveller will visit the same places and see the same scenes whether he begins his circuit of the globe at Shanghai or New York. In a survey of the earth, or of a territory, the particular physical point of departure is, in essence, indifferent. It partly decides the order of visitation; but, in principle, a geographical survey, when made, is the same whatever its starting-point. Mental points of departure may be less, perhaps much less, indifferent. Unlike geographical territory, which presents the same features to travellers from all starting-points, knowledge may vary, and vary all through, with the mental beginning of the knower.

Common sense will probably accept this suggestion. Geographical points of departure seem to be simply entries into established routes; mental routes seem to be determined, at any rate in part, by their beginnings. Psychology is usually one thing to a former biologist and another thing to a devotee with mystical experiences. Physicists incline to different views of atomic structure from chemists. Different social environments, different educations, and different mental types produce different versions or views within the same field of knowledge. Knowledge seems to be very relative to the point of departure of the knower: his original conceptual outfit seems largely to decide what he will subsequently believe and think.

Two little girls gave their father, who was a vicar, a birthday present of a Bible. They knew that many people gave books to their father, and wrote in them "with the author's compliments," so they put the same inscription in their gift. In this story, *Punch* genially indicates a mental habit which Descartes described more sedately: "Whenever men notice some similarity between two things, they are wont to ascribe to each, even in those respects in which the two differ, what they have found to be true of the other."[2] The little maids assumed that a method appropriate to one situation would be equally appropriate to another; their elders make less obvious mistakes, but they have the same habit. Descartes perceived the analogical method that largely dominates the human mind. When Hobbes remarked, ". . . men measure, not only other men, but all other things, by themselves,"[3] he singled out from this analogical method of working its aspect of working on mental models. Animism uses the conscious being as its mental model, and compares everything else with it. Our most familiar experiences, our most deeply or clearly realised conceptions, our habitual methods of explanation, serve as mental models when we are faced with new facts or situations and invited to interpret them. Knowledge can, of course, prosper by analogies. But it seems clear that mental routes must be greatly influenced by their points of departure, for these points of departure are, in part at any rate, the mental models with which the mind starts. The movements of thought demonstrate changes in the general stock of mental models from age to age: it is possible that the human mind's original

[1] *The Domain of Art.*

[2] *Rules for the Direction of our Intelligence*, trans. by Haldane and Ross, Rule 1.

[3] *Leviathan*, ch. ii.

outfit may have only decided the route of thought without affecting its final goal. But it is also possible that its position in the general organisation of reality may permanently limit or qualify its knowledge. It is conceivable that the human mind is so placed in the cosmos and so dominated from experience derived from that placing that it is confined to knowledge which is analogically derivable from its primary models of reality.

Our general physical position in reality, represented by our body, and distinguished from our particular locality on the earth's surface, is intermediate between the vastness of great objects like Jupiter and the minuteness of small objects like motes and bacteria. There is a similar intermediateness in our psychical position. We grasp most easily, according to Bertrand Russell, those conceptions which are logically neither very complex nor very simple.[1] According to Lossky, our knowledge mainly hovers between minute detail and wide generality. No single science, he remarks, has yet completed the ascent of knowledge to the highest generality or the descent to the lowest. The "medium universal" is most clearly discriminated by the human mind; the universal is not clearly connected in consciousness; unique individual events are apprehended through their aspects which are similar to those of other events, and detailed, minutely particular knowledge of individual things is very rare.[2]

We are impelled by our general psycho-physical position in the universe to search for more microscopic aspects than are contained in our primary, more macroscopic vision of things. We see objects which, roughly speaking, are big enough to clutch, and we touch or hear objects which compel us to speculate on their minuter parts. If our physical size placed us as directly in mental touch with the microscopic details of matter as it actually places us in direct mental contact with more macroscopic aspects, if we sensed molecules and atoms or particles approaching molecular magnitudes instead of sensing larger objects, if we lived and moved among corpuscles as we now live and move among trees and stones, our primary version of this world might, by providing a new point of mental departure, transform, for us, the whole field of knowledge. All our attempts to conceive the microscopic structure of matter may be too rigidly controlled by the assumption that gross bodies are composed of parts which, in essence, are miniatures of themselves. The microscopic aspects of matter may not be like the grosser aspects. Our psycho-physical position in the universe imposes upon us, as a fundamental model of reality, the physical thing as we sense or apprehend it. If we are analogically tied to these macroscopic models our version of the microscopic world may always remain relative to our point of mental departure, and always be a more or less successful method of thinking without any actual grasp of reality.

Professor Whitehead is anxious to keep "nature" for the physicist and the physical philosopher out of the clutches of the epistemologist and the metaphysician. They are to have their turn, and they must be consulted for the final interpretation of reality, but in the "philosophy of science" they are interlopers who distract the inquirer from his proper task. It seems doubtful, however, whether his success corresponds to his anxiety either in his ability to achieve a "Concept of Nature" purified from epistemological items or in his attempt to vindicate the principle that we should and can discuss nature and our knowledge of it without reference to our own rôle as knowers.

It is pleasant to simplify and very often temporarily effective. It is simpler, and much less distracting, to forget that the nature "which we observe in perception through the senses" is perceived by percipients and known by knowers. It avoids troublesome suggestions and possibilities that

[1] *Introd. to Math. Phil.*, p. 2.

[2] *The Intuitive Basis of Knowledge* (Duddington's trans.), pp. 291–302.

habits apparently in nature may really belong to its observers. It permits an exclusive devotion to nature by simply regarding it as something known and releases thought from the extra effort of widening its field to include the knower. Science must constantly ignore the knower, and the "philosophy of science" must constantly ignore him too: temporary restrictions on the field of inquiry, temporary isolations of parts from the whole, are required for the concentration of attention. Such simplificatory release from the impossible feat of comprehending everything at once is essential for the development of the "Concept of Nature"; but, if persisted in as a permanent principle of inquiry, the custom of ignoring the knowers may hopelessly distort conceptions of the things they know.

Thought probably began with a naïve failure to understand the protest that "none of our perplexities as to nature will be solved by having recourse to the consideration that there is a mind knowing it." [1] Even when the realisation that we are minds knowing does force its way into our absorption in the things we know it requires a constant reflective effort to remember the possibilities it may involve. It is now permanently obvious that we can think either "homogeneously" or "heterogeneously" about nature, as Whitehead expresses it: think about nature without thinking about thought, or both think of nature and think of such thinking.[2] When he adds, "Natural science is exclusively concerned with homogeneous thoughts about nature," [3] and affirms the "property of being self-contained for thought" to lie "at the base of natural science," [4] he apparently reaffirms his protest against consulting our mental procedures when we endeavour to resolve the perplexities of nature. Yet, when he proposes to substitute for the traditional concepts of "persistent ultimate material distributed among the persistent ultimate points in successive configurations at successive ultimate instants of time," [5] EVENTS as the ultimate facts in nature,[6] he explains the ascendancy of the concept of nature as bodies or substances adventuring through space and in time by affirming that "what is a MERE PROCEDURE OF MIND in the translation of sense-awareness into discursive knowledge has been TRANSMUTED INTO A FUNDAMENTAL CHARACTER OF THE UNIVERSE." [7] If we have been seduced into thinking of material, space, and time as ultimates, and of events as their derivatives by a mental habit, and if we are invited to note how we can escape deflection by this mental habit from the true view of the event as primary, we seem to be explicitly invited to consider the implications of remembering the knowing mind.

We are certainly accustomed to a "macro-corpuscular" estimate of nature: to thinking of the physical world in terms of separate bodies, "macro-corpuscles," as it were, which are scattered through space, and able to move with some degree of freedom both in space and time. This mental habit, enforced upon us from the first, pervading our every notion and action, is obviously reflected in our efforts to understand the microscopic structure of physical reality.

"The general principle of relativity now proposed by Einstein . . . ," according to Prof. Wildon Carr, "shows that it is impossible to abstract from the mind of the observer and treat his observations as themselves absolute and independent of their objectivity." [8] He opposes to Whitehead's

[1] *An Inquiry Concerning the Principles of Natural Knowledge*, Preface.
[2] *The Concept of Nature*, p. 3.
[3] *Ibid.*
[4] *Ibid.*
[5] *An Inquiry Concerning the Principles of Natural Knowledge*, pp. 5–6.
[6] *The Concept of Nature*, p. 15.
[7] *Loc. cit.*, p. 16.
[8] *The General Principle of Relativity*, pp. 21–2.

refusal of any success in resolving the perplexities of nature by referring to the habits of the knowing mind the affirmation that "there is no absolute physical reality which a mind may contemplate in its pure independence of the contemplator and the conditions of its contemplation."[1] The rôle of corpuscular theories of matter divides opinion concerning the function of the knowing mind in science. Physical science has prospered in explaining macroscopic aspects of nature by microscopic interpretations through particles moving in void spaces, and, generally speaking, it has never prospered by rejecting or ignoring corpuscular theories. Democritus established the dominancy which the corpuscular concept was to secure and maintain in interpreting physical phenomena. Descartes' protest against his conceptions[2] was rapidly doomed to oblivion, and supplies one significant, historical instance of the potency of corpuscular theories of matter and of the impotency of rival conceptions in replacing them. The corpuscle, a speculatively attenuated solid body, moving in interspaces, is the primary model of the microscopic structure of matter which dominates the present-day molecular and atomic theories. The extraordinary success of the atomic theory since Dalton, its power of collecting numerous and varied data under systematising principles, its adaptability to new requirements as research discloses fresh facts, its fertility both in explaining the old and in stimulating discovery of the new, suggests that it is a real version of reality, essentially independent of the mind or of its analogical route of interpretation from the macroscopic to the microscopic. Gomperz suggests that, though the atomic theory is still a fertile hypothesis, it may not even be the ultimate truth at our disposal.[3] The human mind may cling pertinaciously to the corpuscle because it is so dominated by its primary models of reality. It may be so habituated, by its fixed, relatively macroscopic position in reality, to bodies separated by space and with some freedom to move in it, to "macro-corpuscles," that it can only conceive more microscopic nature by looking, as it were, as its daily vision of the world through the wrong end of a telescope. It may be compelled to compare molecular or atomic systems to solar or stellar complexes because its daily perceptions tie it firmly to its primary analogical models. The success, the persistence, the apparent incorrigibility of the corpuscular conception may imply a determined, resourceful ingenuity in applying an analogy to which it is psycho-physically restricted. If this alternative estimate be true we cannot naïvely include corpuscular conceptions in our "concept of nature" without noting that they represent a mental procedure as well as an experience of nature.

Descartes thought that matter is a plenum consisting "of closely packed figures with plane surfaces, let us say cubes." When movement proceeded in this plenum it "would cause the cubes to become spherical by gradually wearing off the angles." But "the movement of the cubes cannot alter their relative position without creating void," and "before the movement alters the shape of the cubes by fracturing the angles it must cause their displacement, and the slightest displacement destroys the plenum."[4] In this Cartesian dilemma the rubbing away of angles represents an incipient atomisation—the beginnings of a division of matter into separated corpuscles. This incipient atomisation indicates the natural tendency of the human mind to atomise matter—a tendency which can never be long or successfully repressed. It seems to be a tendency imposed upon it by its familiar "macro-corpuscular" vision of the world with which it begins its explora-

[1] *Loc. cit.*, p. 23.

[2] Vide *The Principles of Philosophy*, trans. by Haldane and Ross, Princ. 202.

[3] *Greek Thinkers*, Bk. III, ch. ii.

[4] Carr, *The General Principle of Relativity*, p. 98.

tions, because it appears so persistently and pervasively wherever these explorations lead.

Child remarks on the connection between neo-vitalistic hypotheses and CORPUSCULAR theories of heredity and organic constitution which explain the natures of living things by composing them of "invisible hypothetical organisms."[1] Gross organisms, "macro-organisms," are speculatively reduced to collections or organisations of "micro-organisms"; the constituents of animals and plants are, as it were, gross organisms, which appeal directly to sense and are dispersed through space and time, seen through the wrong end of a telescope. If it is, as Child asserts, always possible to make the hypothetical units behave as the facts demand,[2] and if human ingenuity can deal resourcefully with its primary atomic conception of matter, corpuscular theories may appear in and dominate both physics and biology because separated bodies, animate and inanimate, moving through space and in time, make an initial impress on the human mind and supply it with fundamental models of reality.

The mind itself has been, with considerable persistence, atomistically conceived. For associationist psychology the mind is composed of discrete "ideas," connected by "association," in a manner parallel to the ultimacy in matter of discrete corpuscles. Herbart, comments Merz, was impressed by the inner life as a continual movement of ideas. Merz's further comparison of the Herbartian psychology to a conceived plan of psychical mechanics[3] irresistibly suggests a transference of the kinetic theory of matter to the mind. If the "chemistry" of ideas as appropriately denotes associationists in general as a "mechanical" theory of ideas appropriately describes Herbart,[4] conceptions concerning the structure of the mind have been as rigorously determined as those concerning the constitution of matter or organisms by that fundamental model—separated bodies adventuring in voids.

Psychological atomism has been more eclipsed or overshadowed (mainly by the opposing notion of the continuum) than biological or physical atomism, but it discloses the inveterate tendency of humans to insert corpuscularity in the heart of reality. This atomising tendency is constantly manifested: it has been suggested, for instance, at various odd moments, that consciousness itself is a rapid series of gushes; space and time are inveterately analysed into points and instants. Psychological atomism, biological atomism, and physical atomism may be essentially one mental procedure determined by a fundamental analogical habit in the mind and by the provision of a fundamental mental model in the enduring, individual body which appears to be lodged in space and time. Recent thought parcels out energy into separated units, just as matter has been conceptually broken up into infra particles. Perhaps an inveterate mental habit, supported by resourceful ingenuity, has again had its way. Perhaps it is so difficult to-day to establish consistent theories of atomic structure because the same mental habit which modelled the atom on the gross body insists on the same model for the constituents of the atom.

Reality may be, as it were, so homogeneous all through that the same models of reality, essentially, would be presented to the mind whatever its psycho-physical position in the universe. If the Brownian movement has demonstrated the actual corpuscularity of matter, Voltaire's Micromegas, placed amongst atoms as we are placed amongst grosser bodies, would

[1] *Senescence and Rejuvenescence*, pp. 10–12.

[2] *Loc. cit.*, p. 13.

[3] *A History of European Thought in the Nineteenth Century.* vol. iii., ch. ii.

[4] *Ibid.*

presumably sense and apprehend essentially the same world of separated bodies in space and time as we do. But it seems obvious, if we are constrained by a mental habit to model our conceptions on the world of things as it happens to appear to us, that we ought to consult this habit and carefully consider it before permitting it to commit us to any "concept of nature."

It certainly seems strange for us to be asked to substitute, as the fundamental cosmic unit, the EVENT for the substantial body in space and time and simultaneously warned not to consider mental procedure as a factor in our experience. Our thought about nature is certainly controlled by an analogical mental habit; in our common-sense, spontaneous, and habitual estimate of the world we are certainly presented with substantial bodies, framed in space and time, as a model of reality. We are compelled, if we desire a "concept of nature," to remember our minds with their methods of knowing as well as the reality they know, to endeavour to understand how our versions of experience are, or may be, affected by our mental procedures, and, if we do conclude that research, supplemented by thought, delivers reality into our hands, to justify our conclusion.

ESSAY-REVIEWS

TACTIC, by G. B. Mathews, M.A., F.R.S. : on Combinatory Analysis, by Major P. A. MacMahon, F.R.S., D.Sc., LL.D. [Vol. I (1915), pp. xx + 300 ; Vol. II (1916), pp. xx + 340.] (Cambridge : at the University Press.)

The term " combinatorial analysis " hardly admits of exact definition, and is not used in the International Schedule of pure mathematics. Broadly speaking, it has come to mean the discussion of problems which involve selections from, or arrangements of, a finite number of objects ; or combinations of these two operations. For the purpose of this article it will be convenient to use Sylvester's term " tactic " as a synonym for " combinatorial analysis."

A few typical examples will illustrate these remarks. The number of changes that can be rung on a peal of n bells of different pitches is the product $1 . 2 . 3 . . . n$, or, as it is usually written, $n\,!$; this is the same as the number of arrangements of n different things in a straight line. If n_r means the number of ways of choosing r things from n different things, then $n_r = n\,! / (n-r)\,!\, r\,!$, with the convention that $0\,! = 1$. (Of course r cannot exceed n.) These two functions, $n\,!$ and n_r, are fundamental in the theory.

Another classical problem is that of the magic square. The digits 1 to 9 may be arranged in the square :

$$\begin{matrix} 2 & 9 & 4 \\ 7 & 5 & 3 \\ 6 & 1 & 8 \end{matrix}$$

where the sum of each row, column, and diagonal is the same, namely 15. Such an arrangement is called a magic square ; and the general problem is that of arranging the numbers $1, 2, 3 . . . n^2$ in a square satisfying the above conditions, and of enumerating the possible solutions when n is assigned. The literature of magic squares is extensive, but, so far as I know, the theory has not been completed. For certain forms of n there are rules for obtaining at least one solution ; the most intractable values of n are those which are of the form $4m + 2$, that is to say, $n = 2, 6, 10$, etc. There is no solution for $n = 2$, and I do not remember seeing a solution for any number of this class, or a proof that it is impossible, except for $n = 2$.

Many problems of tactic were suggested by games of chance. With two ordinary dice the number of different throws is 36, because each die, independently, can fall in six different ways. But the sum of the pips on the top faces can only range from 2 to 12 ; and if we reckon the number of different throws giving the same sum, we can construct the table

$$\begin{array}{rl} \text{sum} = & 2,\ 3,\ 4,\ 5,\ 6,\ 7,\ 8,\ 9,\ 10,\ 11,\ 12 \\ \text{number of throws} = & 1,\ 2,\ 3,\ 4,\ 5,\ 6,\ 5,\ 4,\ 3,\ 2,\ 1 \end{array}$$

For instance, $5 = 1 + 4 = 2 + 3 = 3 + 2 = 4 + 1$. This table enables us to estimate the probability of a random throw leading to a given sum s ;

thus if $s = 6$, the probability is 5/36; if $s = 7$, it is 6/36, or 1/6, and so on. Since all problems of annuities, insurance, and the like, ultimately depend upon calculation of this kind, it is clear that some parts of tactic, at any rate, have important practical applications. Chess has provided a number of tactical problems; the best known are those of the knight's tour and the eight queens. In the former, a knight is placed on any square, and has to move so as to occupy successively each square once and only once; in the latter, eight queens have to be placed on the board in such a way that no two of them attack each other.

Since every combinatorial problem is ultimately one of enumeration, the whole theory is, strictly speaking, a branch of arithmetic. Strange as it may seem, some of the most important theorems in tactic have been deduced from algebraical identities. As a first illustration, we may take the binomial expansion (n a positive integer)

$$(1 + x)^n = 1 + nx + n_2x^2 + \ . \ . \ . + n_rx^r + \ . \ . \ . + x^n$$

Since the coefficients are unaltered if written in the reverse order, it is clear that if we square the expression on the right the coefficient of x^n will be

$$1^2 + n^2 + n_2^2 + \ . \ . \ . + n_r^2 + \ . \ . \ . + n^2 + 1 = \Sigma n_r^2$$

But the square being $(1 + x)^{2n}$, the coefficient of x^n must be $(2n)_n$; so we have the theorem that $\Sigma n_r^2 = (2n)_n$. For instance, if $n = 4$,

$$(2n)_n = 8_4 = 70 = 1^2 + 4^2 + 6^2 + 4^2 + 1^2$$

Very many theorems relating to the binomial coefficients have been discovered by algebraic methods more or less similar to the above.

Euler was one of the first to apply algebraic analysis to tactical problems; and his work on infinite products acquired new and unexpected importance after the discovery of the theta-functions. Both as infinite products and as infinite series, theta-functions have been of great service in yielding theorems about partitions of numbers. By their aid Jacobi was able to prove the famous theorem of Fermat, that every number is expressible as the sum of four or fewer squares; not only so, but he also enumerated the number of such partitions for a given integer. It is significant that Jacobi thought it worth while to give subsequently a purely arithmetical proof without any use of infinite series or products.

With the advent of invariant-theory came a new development of tactic. If a_0, a_1, a_2, etc., are the coefficients of a form, the product $a_0^p\, a_1^q\, a_2^r\ .\ .\ .$ is said to be of degree $(p + q + r + \ .\ .\ .)$ and of weight $(q + 2r + \ .\ .\ .)$, the last being obtained by adding the products of each suffix by the corresponding exponent. The problem of enumerating all products of given degree and weight is a fundamental one in the theory. With coefficients a_0, a_1, a_2, a_3, for example, the products of degree 4 and weight 6 are

$$a_0^2a_3^2,\ a_0a_1a_2a_3,\ a_0a_2^3,\ a_1^3a_3,\ a_1^2a_2^2$$

Invariants and seminvariants may be defined as homogeneous isobaric functions which satisfy certain linear partial differential equations.

It is time to end this preamble, because the main object of this article is to give an outline of the work of a single man. Captain MacMahon, as he was in the old days, soon showed an extraordinary power of dealing with problems of what we may call rational symmetrical algebra, and he has devoted many years to the subject, with uninterrupted and brilliant success. An attempt will now be made to classify some of his most conspicuous achievements.

Invariant-theory, in many ways, brought into a new light the old theory of symmetric functions. A seminvariant of a form is a symmetric function (S.F.) of the differences of the roots, and as such has to satisfy an equation $\Omega\phi = 0$, where Ω is a differential operator, called the annihilator of ϕ. One of MacMahon's discoveries may be explained as follows. Taking forms of indefinitely high degree (n), let

$$f = 1 - na_1x + n_2a_2x^2 - n_3a_3x^3 + \ldots$$
$$\phi = 1 - a_1x + \frac{a_2x^2}{2!} - \frac{a_3x^3}{3!} + \ldots$$

then every seminvariant of f is, up to a numerical factor, equal to a non-unitary S.F. of the roots of ϕ, and conversely. For instance, the semi-variant $\Sigma(\alpha - \beta)^2$ for f is $n^2(n-1)\,(a_1^2 - a_2)$, where $(a_1^2 - a_2)$ is the value of $\Sigma\alpha^2$ for ϕ. In this connection MacMahon greatly simplified the theory of perpetuants, and published a conjectural enumerating function for them, afterwards verified by Stroh. His researches on differential operators bore fruit in various directions, notably in the theory of reciprocants and other differential invariants. In the theory of symmetric functions they are of primary importance, and have illuminated the subject to a surprising degree.

In his two volumes *Combinatory Analysis*, Major MacMahon has codified, with additions, his previous work on tactical problems, properly so called. The results are so numerous, and many of them so technical, that it must suffice to give a general outline, and a few particular examples. Vol. I deals with symmetric functions, combinations, and partitions of numbers. The principal method is that of constructing and transforming enumerative (or generating) functions; and the theorem which the author calls the "Master Theorem" is probably one of the most general results in rational algebra that have ever been proved. Graphical methods, such as those of Durfee, Franklin, etc., are occasionally used; among particular problems considered are S. Newcomb's problem, and various chess-board arrangements. In Vol. II we have a great extension of graphical methods, and a most original discussion of partitions of numbers from various points of view. A sub-section deals with Diophantine inequalities; here it is possible to explain the nature of the problem in simple terms. Suppose that it is required to enumerate all pairs of positive integers (α, β) such that $779\alpha \not< 207\beta$; the answer is given by

$$\alpha = a + b + c + d + 2e + 3f + 4g + 21h + 38i + 207j$$
$$\beta = \phantom{a+{}} b + 2c + 3d + 7e + 11f + 15g + 79h + 143i + 779j$$

where $(a, b, \ldots j)$ are arbitrary positive integers (some of which may be zeros). More complicated problems of a similar kind will be found in the book.

In Chapter III mention is made of two theorems stated, without proof, by Ramanujan. One of these may be put into the following form. Firstly, let any given integer (n) be expressed in all possible ways as the sum of integers, every pair of which differ by 2 at least; secondly, let the same integer (n) be expressed in all possible ways as the sum of integers (with or without repetitions) selected from

$$1, 4, 6, 9, 11, 14 \ldots$$

i.e. all integers of the form $5m \pm 1$. Then the number of representations is the same in each case for all values of n. For instance, if $n = 11$, the partitions are

(i) $11,\ 10.1,\ 9.2,\ 8.3,\ 7.4,\ 7.3.1,\ 6.4.1$

(ii) $11,\ 9.1^2,\ 6.4.1,\ 6.1^5,\ 4^2.1^3,\ 4.1^7,\ 1^{11}$

There is little doubt that the theorem is correct, and that there is some way of deducing it from theta-function series; but it was not proved in 1916, and I have seen no proof myself.[1] Not improbably, some graphical method will provide the key to the puzzle.

Major MacMahon gives other instances of conjectural theorems, and in this way the work is certain to stimulate research. In the matter of magic squares, too, he has a very interesting and valuable chapter. In fact, he does obtain a "crude" enumerating function, but the reduction of this to the true form, even for small values of n, seems hopelessly laborious.

Enough, I hope, has been said to give some idea of the nature and extent of Major MacMahon's achievements. One great advantage which the *Combinatory Analysis* has for a student is that he can start reading it with a very small amount of mathematical knowledge; and everything possible is done for him by way of elucidation. This is not to say that the book can be read as you run; the subject is too difficult for that; but hardly anyone of capacity can fail to be fascinated by the subject-matter, and by the way in which it is presented.

[1] Since the above was written I have seen the obituary notice of Ramanujan in *Proc. L.M.S.* (May 1921). From this it appears that several of his theorems (probably including the above) have been recently proved by Prof. Rogers and Messrs. H. B. C. Darling and L. J. Mordell.

REVIEWS

MATHEMATICS

The Theory of Functions of a Real Variable and the Theory of Fourier's Series. Second Edition, in two volumes. Vol. I. By E. W. HOBSON, Sc.D., LL.D., F.R.S., Sadleirian Professor of Pure Mathematics, and Fellow of Christ's College in the University of Cambridge. [Pp. xvi + 671.] (Cambridge: at the University Press, 1921. Price 45s. net.)

THE first edition of this work appeared in 1907. The time which has elapsed since then has been a period of great activity in the development of the Theory of Functions of a real variable. In order to give an account of the present position of the subject, Dr. Hobson has expanded his book into two volumes, of which only the first has at present appeared. This volume, in which the matter has been partly rewritten and considerably enlarged, treats the theory of number, the descriptive and metric properties of sets of points, transfinite numbers and order-types, functions of a real variable, the Riemann integral and the Lebesgue integral, and, finally, non-absolutely convergent integrals. In the chapter on Transfinite Numbers and Order-types the situation with respect to the controversy connected with axiom variously called the Multiplicative Axiom, the Principle of Selection, and Zermelo's Axiom is reviewed, but no attempt has been made to give dogmatic decisions. The status of this axiom remains undefined. The author has, however, made a point of giving, where possible, proofs of theorems which do not depend on the assumption of the axiom, and of pointing out in other cases that the axiom has been assumed.

D. M. WRINCH.

The Principle of Relativity, being Original Papers by A. EINSTEIN and H. MINKOWSKI, translated into English by M. N. SAHA and S. N. BOSE, with an historical introduction by P. C. MAHALANOBIS, University of Calcutta. [Pp. xxiii + 186.] (Calcutta: Published by the University, 1920.)

THE University of Calcutta has done English-speaking people a service by putting into their hands an English translation of what will always be the three classical papers in the literature of the Principle of Relativity, a principle which must be recognised to have come to stay and to be of the utmost significance for the future of physical science, since it makes possible the reconciliation of Newton's simple system of dynamics with what the philosophers have always held must be true in regard to the relative nature of position and motion. We have here first the paper of 1905, in which Einstein first put the view that the recognition of the relative nature of a scale of time acts as a sufficient explanation of the difficulties of reconciling the electro-magnetic theory of light with the experiments of Michelson and Morley. In this paper he completes and rounds off the electron theory of Lorentz as a theory of the constitution of matter, replacing the hypothesis of the contracting electron

by the more general and at the same time more satisfying hypothesis now known as the Special Principle of Relativity. (*Annalen der Physik*, 1905.)

The second paper is that in which the late H. Minkowski showed that the work of Einstein brought the two concepts of space and time into such a close unity that he was able to say in a lecture at Cologne (Sept. 21, 1908), "Henceforth, the old conceptions of space for itself and time for itself are reduced to mere shadows, and only some sort of unity of the two will be found consistent with the facts." This lecture, delivered under the title of "Raum und Zeit," is here produced.

Finally we have the paper (*Annalen der Physik*, **4**, 49, 1916, in which Einstein gave the first complete account of his generalised theory of relativity, showing how it made a natural place for the gravitational phenomena which had always eluded the electro-magnetic theory, and how the new theory of gravitation automatically removed the only outstanding discrepancy between theory and observation.

E. C.

The Absolute Relations of Time and Space. By A. A. Robb, Sc.D., D.Sc., Ph.D. [Pp. viii + 80.] (Cambridge: at the University Press, 1921. Price 5*s*. net.)

This little book is intended to introduce to a larger public the main outline of the fuller discussion of the same matter which the author has published under the title, *A Theory of Time and Space*, also through the Cambridge University Press. Dr. Robb was dissatisfied, as well he might be, with the ordinary physicist's naïve view of space and time, a view in most cases quite unconsciously based on the acceptance of Newtonian theory as being the last word in physical science. Another few years of acquaintance with the theory of relativity may well bring about such a change in this respect that Dr. Robb's discussion, instead of appearing abstract and academic, will be taken very much for granted.

The main purpose of the work is to analyse all temporal and spatial relations in terms of the single idea of one event being before or after another. It is taken for granted that the only purpose of science is "to analyse the relations of order among the instants of which I am directly conscious." Dr. Robb confesses that he started this work through a sense of dissatisfaction with Einstein's standpoint, which seemed to take all meaning out of such a familiar concept as "simultaneity of two events." His own conclusion is that the only really simultaneous events are events which occur at the same place. It must be confessed that Dr. Robb seems to leave even less meaning in the word than do those who treat it as a relative term. Nevertheless, Dr. Robb does us a great service in insisting that it is order relations in an individual view of the universe which form the subject matter of science, that there is a real difference in those relations between "before" and "after," and that geometry in the physical world is also an empirical science. But the mathematician has not yet succeeded in producing a technique for discussing order relations before introducing the idea of measurement. He has not yet faced up to the fact that measurement is itself merely a statement of certain order relations already perceived to exist.

Dr. Robb attempts at the conclusion of his book to make a bridge from purely descriptive order relations to the use of co-ordinate numbers; but he comes here to all the difficult problems of the logical bases of geometry, and there is a sense of insufficiency in his treatment of this part of his argument which is not apparent in the earlier part of the argument. Nevertheless, it would be well if there were more among our physical scientists who had the patience to follow out the consequences of their own premises as logically as Dr. Robb has done.

E. C.

Higher Mechanics. By Horace Lamb, Sc.D., LL.D., F.R.S., Honorary Fellow of Trinity College, Cambridge; lately Professor of Mathematics in the Victoria University of Manchester. [Pp. x + 272.] (Cambridge: at the University Press. Price 25*s*. net.)

Prof. Lamb's book, which treats of three-dimensional Kinematics, Statics, and Dynamics, is very welcome. It is to some extent a sequel to *Statics* (Cambridge, 1912) and *Dynamics* (Cambridge, 1914), but it is not dependent on these. Prof. Lamb has adopted, as his principle of selection in this book, that matters of genuine or dynamical importance be included, and developments whose interest is purely mathematical or mainly historical be omitted. Thus he treats the Theory of Screws, of Null Systems, and of Least Action, and omits brachistochrone problems, and the general theory of the Differential Equations of Dynamics.

Statique Dynamique. By M. Stuyvaert. [Pp. 205.] (Ghent: Van Rysselberghe & Rombaut, 1920. Price 20 fcs.)

In this comparatively small volume the author has succeeded in giving a very clear account of the fundamental principles of mechanics. Statics finds its basis in Newton's laws of motion, and from this point of view the discussion of the equilibrium of a particle and of extended bodies follow in a natural sequence. There is a brief treatment of graphical statics and the attraction of spheres, and the volume concludes with chapters devoted to the fundamental principles of particle and rigid dynamics. Without entering into practical details the experimental basis of the science is well brought out. To attempt to cover so wide a field in one small book naturally leads to considerable compression, yet the excellent arrangement and printing makes it easy to trace the development of the subject, and it should prove a valuable introduction to mechanics for those who come to it for the first time with some knowledge of the calculus. G. B. J.

The Scientific Papers of Bertram Hopkinson, C.M.G., M.A., F.R.S., Fellow of King's College, and Professor of Mechanism and Applied Mechanics in the University of Cambridge. Collected and arranged by Sir J. Alfred Ewing, K.C.B., F.A.S., and Sir Joseph Larmor, F.R.S. [Pp. xxvii + 479.] (Cambridge: at the University Press, 1921. Price 63*s*. net.)

This book contains all the more important papers of Professor Hopkinson, with the exception of his memoir on the *Theory of Vibrations of Systems having One Degree of Freedom*, which forms the introductory volume of the series of Cambridge Tracts in Engineering. The collection and arrangement of the papers on mathematical and engineering science, published in various journals from 1898–1914, has been in the hands of Sir J. Alfred Ewing and Sir Joseph Larmor. The value of the book is very great; and it is of real interest to have on record in the obituary notices of Sir Alfred Ewing and Mr. A. V. Hill (reprinted from the *Proceedings of the Royal Society* and the *Alpine Journal* respectively), many details of the life and career of so brilliant a scientist. Apart even from the scientific value of the book, we are glad to welcome it as a memorial to a man in whom the passion for research was strong.

PHYSICS

The World of Sound. By Sir William Bragg, K.B.E., D.Sc., F.R.S., Quain Professor of Physics in the University of London. [Pp. viii + 196, with 93 figures.] (London: G. Bell & Sons, 1920. Price 6*s*. net.)

Sir William Bragg has used the material of his Royal Institution lectures on Sound to make one of the most delightful nature books we have ever

read; a book which will appeal to the general reader by its simplicity and clarity as much as it will those familiar with its subject matter by the many curious facts that it contains. The lectures were intended for a juvenile audience, and very rarely indeed does the treatment pass beyond their understanding, a fact which, in itself, is no small triumph for the author. The whole production, with its attractive cover and little pencil insets by Miss Audrey Weber, strikes a new note for a work on a physical subject; it appears, indeed, to have been modelled on the books of Dr. Seton-Thompson and other writers of animal biographies.

The first two chapters contain an account of the elements of the subject, *i.e.* a study of wave motion in general and of the vibrations which produce sound in particular. These rather forbidding matters are dealt with in a most interesting manner by frequent appeals to the evidence of the ripple tank, and by many ingenious and striking experiments as, for example, the five springs, maintained in vibration by an electric current, which render visible the mode of vibration of the air in closed and open pipes. The rest of the book is divided by a happy inspiration into chapters on the characteristic sounds of the town, the country, the sea, and of war; the subject-matter under each heading serving as a means of introducing many fundamental phenomena in novel dress. Thus, among the sounds of the town is the familiar "plop" of water as it drips into a bath or bucket. Sir Richard Paget has measured up the cavities in the water made by the falling drops and constructed models of them in plasticine. The noise of the drops can then be reproduced by blowing across the mouth of the model cavity so that the source of this sound is made quite clear. Again, there is an account of the novel phenomenon which underlies the use of the ocarina, namely, that with similar vessels of the same size that which has the widest opening gives the note of highest pitch. There is also, in this chapter, a discussion of the Doppler effect, of architectural acoustics, and of singing flames whose note is more familiar as the roar of the fire up the chimney.

In the next chapter we are told of the sounds of the insects and of the trees; of the grasshopper's tambourine, of the cricket's bow, and of the knockings of the "Death-watch" beetle. We learn the secrets of æolian tones and of the difference between the sounds made by the leaves of the trees; why the poplars rustle, and why the sound of the wind in a pine wood is high-pitched and soft. The sea does not provide so many striking examples, for its depths are silent and outside our experience; but in the fish we find the crudest form of ear, and the author traces for us the development of this organ and tells us something of its amazing powers. The last chapter on "Sounds in War" contains an account of some of the devices used for the detection of submarines and for the location of guns and mining galleries. It is remarkable that, while before 1914 sound had been a particularly "pure" science, yet in the next five years almost everything that had previously been discovered found some useful application.

In conclusion, we can only advise those who have not yet placed this book on their shelves to fill the gap at the earliest opportunity.

D. O. W.

Laboratory Projects in Physics. By FREDERICK F. GOOD, A.M., Instructor in the School of Practical Arts, and in the School of Education, Columbia University, New York City. [Pp. xiv + 267, with 104 figures.] (New York: Macmillan & Co., 1920. Price 9*s.* net.)

HERE is a course in practical physics of the every-day kind which has been worked out to the smallest detail by the author for the use of beginners—both boys and girls—in a modern "High School." It is not clear from the preface that the course has actually been used in its entirety, but it is clearly of a

practical character and forms a very complete contrast with the course prescribed by the syllabus of our Head Master's Association with its deadly dull measurements of distances, area, volumes, relative densities, and so forth, which is probably responsible for much of the present-day indifference to science in this country. Youngsters following Mr. Good's course are kept in constant touch with their experience outside the laboratory. They experiment with a pendulum and then assemble and study the parts of a dissectible clock. They study liquid pressure (rather casually it must be admitted) by comparing the heights of the water and mercury columns which balance in a U-tube and proceed to measure the pressure of the gas supply, the water supply, and the blood in their own arteries; they make up models of various kinds of pumps, and even of an hydraulic elevator, with simple glass apparatus. In like manner they make models of various types of hot-water systems, they test gas and water meters, and are initiated into the mysteries of the vacuum-cleaner, the sewing-machine, electric motors and dynamos, photography, etc., finishing up with quite a thorough investigation of the working of a Ford car!

The book has been written with an eye to the needs of the teacher; there is a complete specification of all the apparatus required, with the reference numbers in the catalogues of the appropriate manufacturers and the approximate cost. There is even a list of books for the laboratory bookshelf, with a statement of the number of copies of each required! A boy working through a selection of the experiments described in the book and *honestly attempting to answer the questions set at the end of each experiment* (otherwise the course would degenerate into play) should obtain an intelligent knowledge of the mechanical and electrical world around him. He would know what every boy should know on leaving school for ordinary life; but this knowledge would be qualitative only, and would need to be supplemented by a good deal of training in the elements of accurate and systematic physical measurement to make it suitable as a preliminary to work in the technical college or university.

D. O. W.

The Mathematical Theory of Electricity and Magnetism. By J. H. Jeans, M.A., F.R.S. [Pp. viii + 627, with 138 figures. Fourth Edition.] (Cambridge: at the University Press, 1920. Price 24*s.* net.)

A Text-book of Physics. By W. Watson, C.M.G., D.Sc., F.R.S. [Pp. xxvi + 976, with 580 diagrams and figures.] Seventh Edition, revised by Herbert Moss, M.Sc., D.I.C. (London: Longmans, Green & Co., 1920. Price 21*s.* net.)

Prof. Jeans's book on Electricity and Magnetism is so admirably suited to the needs of the physicist who desires a treatment rather more advanced than that provided by Sir J. J. Thomson's *Elements*, and who, at the same time, is unable to bring to its study the equipment of the expert mathematician, that it is not surprising that a fourth edition should already be demanded. The main changes in the new edition consist in a rearrangement of the later chapters dealing with the Electromagnetic Theory and the Motion of the Electrons and, in addition, an entirely new chapter on "Relativity" which the author hopes "will provide a suitable introduction . . . for the student who approaches the subject for the first time equipped with such knowledge of general electrical theory as can be gained from the rest of the book." In this short notice we can only say that the matter in this chapter is presented to the reader in a style which, for lucidity and conciseness, loses nothing by comparison with the high standard set by the treatment of the other branches of the subject.

This edition of Watson's well-known text-book shows signs of a good deal of revision by its new editor, Mr. H. Moss, Lecturer in Physics at the Imperial College of Science, London. It has been his aim to "ensure that the standard of all parts of the book should be at least equal to that of the pass degree of any university." This aim has been achieved in so far that it is probable that anyone having a thorough knowledge of the whole of the book would succeed in scraping through his degree examination; but he certainly would not have any idea even of the existence of many phenomena with which a university teacher would consider it essential that his pass students should have at least a nodding acquaintance. Further, he would be seriously disturbed by many of the questions which are to be expected in, say, the pass papers in the London B.Sc. The editor can verify this by looking up some of these papers and then referring to his treatment of viscosity and radiation pressure, to take two examples at random. It is, of course, obvious that to revise and add to a book of this type, standard, and dimensions is an enormous task only to be completed slowly edition by edition; nevertheless, there are certain points which might have received attention. We find, for example, that the old definition of the coefficient of thermal conductivity is still retained, although it contains a really inexcusable misstatement of fact; the definition of entropy is equally misleading, and the curves showing the variation of the resistance of metals with temperature are still shown as straight lines passing through the absolute zero of temperature with no indication of the results obtained by Kamerlingh Onnes at very low temperature. A book published in 1920 and intended for pass students should contain a brief mention of work of this character. These things apart, the book remains a magnificent and enduring monument to the ability of its original author. When we express a desire for improvement we also express our conviction that it will continue in demand and growth. Finally, it may be pointed out that Messrs. Longmans would do well to overhaul the original blocks and the original type in the next edition.

D. O. W.

Physical and Chemical Constants. By G. W. C. KAYE. O.B.E., M.A., D.Sc., and PROF. T. H. LABY, M.A. [Pp. viii + 161. Fourth Edition.] (London: Longmans, Green & Co., 1921. Price 14*s.* net.)

THERE is no need, in a review of the fourth edition of these tables, to praise them; they have been long since tried and trusted friends in every physical and chemical laboratory. It is, however, fitting that we should express our gratitude to the authors for the trouble they took in the original compilation and that they are taking in keeping it up to date. The principal changes appear in the sections relating to terrestrial constants—gravitational and magnetic—in Heat and in Electricity. Further, it is stated that some 700 additions and alterations have been made in the physical constants of chemical compounds. The recent work on X-ray spectra has necessitated the addition of three pages of the wave-lengths of the lines of the K, L, and M series spectra of the elements, while two more pages contain most useful tables of spark-gap voltages for sparks between needle-points and spheres of various sizes. The spark-gap method for measuring large potential differences now being standard for voltages above 70 kilovolts. The changes in the table of Atomic and Radioactive constants since the first edition was published in 1911 are remarkably few, the charge associated with the electron becomes $1{\cdot}59 \times 10^{-20}$ coulombs instead of $1{\cdot}57 \times 10^{-20}$, the mass $\frac{1}{1845}$ of the mass of the hydrogen atom instead of $\frac{1}{1700}$. Avogadro's constant $2{\cdot}705 \times 10^{-19}$ molecules per c.c. instead of $2{\cdot}75$ and the number of coulombs carried by

the chemical equivalent weight of an ion in electrolysis 95706 instead of 96470.

The printing and paper in the new edition are the same as in the first; it is to be regretted that the publishers find it necessary to make the price so much greater.

D. O. W.

The Dynamical Theory of Gases. Third Edition. By J. H. Jeans, M.A., F.R.S., formerly Stokes Lecturer in Applied Mathematics in the University of Cambridge; sometime Professor of Applied Mathematics in Princeton University. [Pp. vi + 442.] (Cambridge: at the University Press, 1921. Price 30*s.* net.)

The Mathematical Theory of Electricity and Magnetism. Fourth Edition. By J. H. Jeans, M.A., F.R.S. [Pp. vi + 627.] (Cambridge: at the University Press, 1920. Price 24*s.* net.)

We welcome the third edition of Mr. Jeans's book on *The Dynamical Theory of Gases* and the fourth edition of his *Mathematical Theory of Electricity and Magnetism*. To the first the author has added an entirely new chapter on Quantum Dynamics, incorporating the recent work of Ehrenfest, Sommerfeld, and Epstein; he has, further, enlarged and rewritten the chapter on Radiation and the Quantum Theory, which appeared in the second edition. The fourth edition of *Electricity and Magnetism* differs from its predecessors principally in the addition of a wholly new chapter on the Theory of Relativity.

It is not necessary to say anything to recommend either of these books. The fame of the author as a brilliant and clear expositor is as widespread as his reputation as a penetrating and wonderful thinker in mathematical physics.

D. M. Wrinch.

CHEMISTRY

The Experimental Basis of Chemistry. By Ida Freund. Edited by A. Hutchinson and M. Beatrice Thomas. [Pp. xvi + 408.] (Cambridge: at the University Press, 1920. Price 30*s.* net.)

In Miss Ida Freund the teaching of elementary chemistry has lost one of its most able exponents. The teaching of elementary chemistry—that is, the fundamental principles of chemistry; the term "elementary" is too often taken to be synonymous with "easy"—has in England never received the attention it deserves from the leaders of chemical thought. They have been content to leave to the pedagogue the application of modern methods of teaching to the exposition of the subject, and, owing to lack of appreciation of scientific method, the best results have not always been obtained. Miss Freund was not only an enthusiastic educationist, but possessed that keen critical faculty and sound knowledge of scientific principles which caused her to revolt against some of the irrational ideas of the heuristic method of teaching science, and teachers of chemistry are fortunate in having her views so clearly expressed in the present volume.

The book, founded on a laboratory course, as the editors explain in their Preface, is the result of a desire on the part of Miss Freund "to bring to the notice of other teachers her views as to the manner in which students might be helped to realise that chemistry is a science based on experiment, and that the logical interpretation of experiment leads directly to the generalisations known as the laws of chemistry." Unfortunately, the author's death left it incomplete, but the more important part was at that time almost ready for the press, and, being complete in itself, is now published under the editorship of two of the author's personal friends.

In the Introduction, perhaps the most valuable part of the book, the method is explained, and great stress is laid on the fundamental differences between students' illustrative examples and research work. The illustrative examples are experiments, often based on historical ones which have helped to establish the laws of chemistry, for the student to perform, modified in such a way as to bring them within the range of his experimental ability and of the apparatus at his disposal in a well-equipped laboratory. The sources of error and tacit assumptions in such experiments are carefully pointed out, the quantitative results obtained by students compared with the standard values, and the students' method contrasted with the research methods by which these standard values were obtained. In this way the author indicates the absurdity of the claim sometimes made, that pupils of school age can *discover* the composition of water or *prove* the law of constant proportions.

The adoption of the term "standard value" instead of the frequent "theoretical value," is an excellent indication of the author's attitude; quantitative relations in chemistry are experimental results liable to correction, as experimental methods improve, and not theoretical deductions from absolute mathematical laws.

A valuable section deals with the errors of measurement and their relation to the value of results. The rest of the book is devoted to the development of the fundamental ideas by means of illustrated examples on the nature of chemical change; compounds and mixtures; combustion; the conservation of mass; the laws of fixed, multiple, and reciprocal ratios; equivalents; atomic weights, and the law of combining volumes. In every case the sources of error of the method are emphasised, and the value of the results as evidence carefully considered.

One can find little fault with the logic of the treatment of the subject, though a few minor errors of fact and expression have escaped the notice of the editors. The peculiar value of the book lies in its insistence on the appreciation of the effect of experimental errors of method on the value and meaning of results, and, while it is unlikely that many teachers will wish to adopt the system suggested in its entirety, it should be read by every teacher and serious student of chemistry. Every page is suggestive, and, if its teaching is taken to heart, students will less frequently assure their mentors that they have found, by volumetric analysis, that a coin contains 92·4846 per cent. of silver.

O. L. B.

La Chimie et la vie, par GEORGES BOHN, Directeur de Laboratoire a la Sorbonne, et ANNA DRZEWINA, Docteur ès Science. [Pp. 275.] (Paris: Ernst Flammarion, 1920. Prix 7 Fr. 50.)

THE purpose of this book, which is one of the well-known "Bibliothèque de Philosophie Scientifique," is to show that the study of life and vital processes is, and must be, firmly founded on the science of chemistry.

Prof. Bohn and Dr. Drzewina are at great pains to interpret all the basic phenomena of life in terms of chemistry, more particularly of physical chemistry, and whilst both chemists and biologists may find in this book much with which to disagree, yet all readers will feel that, substantially, the point of view taken up by the writers is sound.

Although the original crude idea of vitalism—the so-called "vitalist theory"—was shattered in 1828 by Wöhler's synthesis of urea from purely inorganic materials, and has been put even more completely out of court by the rise of synthetic organic chemistry, yet at the present day there are many who wish to argue that the laws governing living matter differ from those which control dead matter.

The physico-chemical investigations of Arrhenius and others on the rates

of action of ferments, and the researches quoted by the authors on the temperature co-efficients of vital processes, show quite definitely that the laws of chemical action, which prevail elsewhere, are also obeyed by the living organism, in so far as it is possible to disentangle individual processes from the complex of functions which comprise life.

The book raises many topics of great interest, which would require many pages to discuss; it will suffice to indicate here the contents of the more important chapters which deal with Catalysts and Ferments, the Temperature Co-efficient of Vital Processes, Defensive Ferments, Chemical Fertilisation, Determination of Sexual Characteristics, Determination of Form, the Chemical Activity of the Brain, Chemical Polarity and Depolarisation, and the Chemical Specificity of Animal and Vegetable Organisms.

Any one of these subjects would suffice to open a discussion, and those interested in the subject will be well advised to read the work through carefully.

The authors have achieved no inconsiderable task in putting the book together, and fully deserve the thanks of their readers. F. A. M.

(1) **A Text-book of Inorganic Chemistry for University Students.** By J. R. Partington, M.B.E., D.Sc., Professor of Chemistry at the East London College, University of London. [Pp. xii + 1062, with numerous illustrations and diagrams.] (London: Macmillan & Co, 1921. Price 25*s*. net.)

(2) **Inorganic Chemistry.** By E. I. Lewis, M.A., B.Sc., with an introductory note by Sir R. Threlfall, K.B.E., F.R.S. Third revised and enlarged edition. [Pp. xv + 443, with numerous diagrams.] (Cambridge: at the University Press, 1920. Price 9*s*. net.)

Professor Partington's text-book is primarily intended for students who have completed an introductory course of matriculation standard, although the more elementary parts of the subject are included so as to make the book complete in itself. It is not written for any special examination, but should meet the requirements in Inorganic Chemistry of students preparing for the examinations of the Intermediate and Pass B.Sc. of British Universities. Brief accounts of technical processes and the elements of physical chemistry are included.

For the most part the book follows the arrangement usual in books of this type and does not call for any special comment in this respect. It is quite up to date, and clearly written, with numerous illustrations and worked examples. The only adverse criticism one may make is that, perhaps, almost too great detail has been attempted, so that the student is in danger of not being able to see the wood for the trees. This, however, probably applies to nearly all text-books of this type.

Prof. Partington has done his work carefully and conscientiously, and the book may be confidently recommended to those requiring a sound and up-to-date knowledge of inorganic chemistry, as being one of the best of its kind published as yet.

The new edition of Lewis's "Inorganic Chemistry" is a sufficient indication that it forms an acceptable introduction to the study of chemistry.

As the book is the outcome not only of a carefully-arranged scheme suitable for teaching an elementary class of boys but also of many years of actual experience in the use of the book, teachers in search of a new elementary text-book will feel safe in giving the present volume a careful trial. The subject-matter is well arranged and clearly put, and is suitable for private study as well as for class teaching, which cannot be said of all text-books.

F. A. M.

Silica and the Silicates. By JAMES A. AUDLEY, B.Sc., F.I.C. Industrial Chemistry series, edited by S. Rideal. [Pp. xii + 374, with 27 figures.] (London: Baillière, Tindall & Cox, 1921. Price 15s. net.)

IN his Preface to this volume the author tells us that "no previous English author appears to have attempted to cover the same range of subjects within the limits of a single volume." That he has attempted too much, particularly as he is writing on technological subjects with which he has only a limited acquaintance, and that he has evidently not had access to the scientific literature while writing the book, has resulted in the production of a work which is more likely to mislead than to instruct those into whose hands it may fall.

The scientific treatment of the subject can only be described as inaccurate, and even as incoherent. Under the heading Silica one might have expected to find a concise statement of our knowledge of the properties of that substance. The Transformations of Silica are dealt with in sections scattered about the book (pp. 33–6, 180, 260–3), and, though our knowledge of the subject is not altogether satisfactory, the author is not justified in telling us, within the limits of a few lines, that, in silica bricks, quartz is transformed into tridymite at 1,600°, that it is not altered by heating to high temperatures, and that, in steel furnaces, quartz is altered into tridymite if the temperature exceeds 1,400°. Fenner's paper, published in the *Journal of the Society of Glass Technology* in August 1919, sums up the actual situation quite clearly. Of the specific heat of silica we are told practically nothing beyond the fact that it "exhibits peculiarities." Nothing is said about the thermo-chemistry of the silicates; and the section dealing with the Expansion of Silica (p. 13) is quite unintelligible. The second section, headed Silicates, describes a number of compounds with little regard to their technical importance. The account of carborundum, ferro-silicon, and similar products should not have been included in an already overcrowded work.

The technical sections of the work are devoted mainly to a description of processes, but they are so condensed that it is too often quite impossible to grasp the author's meaning. In dealing with the firing of ceramic products and bricks (pp. 200–4, 251–4) he describes down-draught, up-draught, stack, and regenerative kilns; but no one unacquainted with the industries could gather the faintest idea of the construction and working of these kilns. Grinding machines are figured and described, but there is no indication of the purpose or service of the different types of plant.

The author has ventured to write upon glass, of which he has neither a practical knowledge nor even a knowledge of the literature of the subject. He retails Grenet's obsolete ideas on the process of softening and annealing, and ignores Twyman's scientific treatment of the subject, based upon Maxwell's work on the relaxation of strain in viscous bodies. His treatment of Devitrification indicates that he has not read Bowen's paper on the subject, to which he makes reference at the end of the paragraph dealing with it. His statements with regard to glass and glass-making are often wildly inaccurate, and it is quite evident that he knows nothing about glass furnaces, either regenerative or recuperative.

The editor tells us in his Preface to this volume that works of this kind will provide "mental munitions for the coming industrial war." If it represents the mentality of our leaders, that war is now lost.

MORRIS W. TRAVERS.

Anthracene and Anthraquinone. By E. DE BARRY BARNETT, B.Sc., F.I.C. [Pp. xii + 436.] (London: Baillière, Tindall & Cox, 1921. Price 27s. net.)

THE appearance of this book is particularly welcome, and the author is to be congratulated on his success in a difficult and laborious task. During the last

few years the discovery of the great value of the vat dyes derived from anthracene has given a great impetus to the study of this compound and its derivatives. Most of the results obtained, however, are hidden in the patent literature amid a mass of irrelevant matter and, as far as the writer is aware, previous to the publication of this volume no modern work existed in which the chemistry of anthracene has been treated as a coherent whole. The subject is treated from the theoretical standpoint, and although the chief anthraquinone dyestuffs are fully dealt with, manufacturing details are not entered upon.

The book deals with anthracene and its substitution products, anthraquinone, its reduction products anthrone and anthranol, and the syntheses of the anthraquinone ring. After an account of the benzanthraquinones and the substitution products of anthraquinone, the amino and hydroxy-anthraquinones and the dyes obtained from them are described. A chapter is devoted to the important benzanthrones and others to various heterocyclic derivatives. Indeed, practically the whole field of anthracene chemistry is covered with the exception of the naturally occurring derivatives, of which, as the author explains in the Preface, a recent account has appeared elsewhere. The amount of information contained in the book and the very numerous references to journals and to the patent literature render it a most valuable résumé to the subject for the research worker and the dye chemist.

A few minor adverse remarks may be made. The system of notation for the anthraquinone ring, the author's modification of that of Pfaff, has the merit of simplicity; but it is very desirable that the atom of a substituent (such as the amino group) actually attached to the ring should be placed immediately beneath the vertical line. In the formulæ on page 234, for example, the amino-group is so placed that it is uncertain to which ring the substituent is attached. Occasionally the author's evident desire for brevity leads to lack of clarity, a difficulty inherent in a work so exhaustive. However, the book is so valuable that such minor faults are far outweighed and it should make a wide appeal to all interested in this branch of organic chemistry both in its theoretical and technical aspect.

O. L. B.

The Application of Dyestuffs to Textiles, "Paper," Leather and Other Materials. By J. Merritt Matthews, Ph.D. [Pp. xvi + 768, with numerous illustrations.] (London: Chapman & Hall, 1920. Price 57*s*. 6*d*. net.)

The present volume is a development and extension of the author's earlier laboratory manual of dyeing and textile chemistry, and, while still retaining many of the text-book features in order to adapt it to the needs of the student, it has been greatly broadened so as to appeal to all concerned in the application of dyes, especially in the textile industry.

The book contains such a mass of useful information, valuable alike to the student and the technical man, that one cannot hope to do justice to it in a short review.

It is to be regretted that a little more space has not been devoted to the minor uses of the natural and synthetic colours, such as spirit colours, lakes, and so on, as it is just these matters that are often so difficult to track down when details are wanted quickly.

The book is copiously illustrated with photographs and diagrams of plant used in the dyeing industry, almost too copiously in fact, as there does not appear in many cases to be any adequate description of the machinery illustrated or explanation of the special uses to which it is put.

There is an unfortunate error in the diagram on page 434, where it is stated that phthalimide is obtained from phthalic anhydride by the action of *strong*

nitric acid (sic). This error is due to the source from which the diagram is taken; it is a pity that a more reliable authority was not consulted.

No mention appears to be made of the well-known Lodge-Evans process for applying sulphur dyes to woollen goods. A useful feature is the inclusion at the end of each chapter of a list of the more important dyes of the class dealt with on the preceding pages, and the bibliography on the pages 733 to 750 should prove of value. In addition to the usual index there is also a special index of experiments which should be useful.

Unfortunately, owing to the confusion of names in the dye world, the author has decided for the present to keep to the old German names; but there would appear to be no reasons why some of the more important names of dyes given by British and American makers should not be included as well; German dyes get more than enough free advertisement as it is.

Dr. Matthews is to be congratulated on having completed a very useful and painstaking work, which will without doubt become a standard on the subject it deals with.

F. A. MASON.

The Volatile Oils. Vol. II. By E. GILDEMEISTER and FR. HOFFMANN. Second Edition, by E. GILDEMEISTER. Authorised translation by EDWARD KREMERS. [Pp. xx + 686, with 4 maps, 3 tables, and numerous illustrations.] (London: Longmans, Green & Co.; printed 1916, published 1920. Price 32*s*. net.)

THIS volume, the authorised English translation of the second volume of the second German edition of Gildemeister and Hoffmann's classical work, deals with a number of the individual essential oils. The same plan has been adopted as in the first edition, but the information available has grown so greatly that it will require a third volume to complete the work.

The nature and value of this book are so well known that it hardly calls for much detailed review. In common with all books of this type, however, it lags some years behind the times, and it is becoming more and more urgent for a system to be devised for the publication of works of reference, which will enable them to be kept up to date year by year. A method which will not be too costly is difficult to find, but it seems desirable that some international body should take up the whole question of the production of standard works of reference.

The present work has been printed by Schimmel & Co., under whose auspices it was compiled, and its excellent get-up is in marked contrast to the poor work being put into many similar books in this country. The phraseology of the book is a worrying feature to the English reader, as it reflects too closely the construction of the original German. The subject, however, is dealt with in the thorough way which one would expect, and the book should be in the reference library of all those interested in this branch of chemical technology.

O. L. B.

Organic Medicinal Chemicals (Synthetic and Natural). By M. BARROWCLIFFE, M.B.E., F.I.C., and FRANCIS H. CARR, C.B.E., F.I.C. [Pp. xiv + 331, with 25 figures in the text]. (London: Baillière, Tindall & Cox, 1920. Price 15*s*. net.)

THIS volume is one of the series of Industrial Chemistry under the general editorship of Dr. S. Rideal. A novel and refreshing feature of the work is that it deals not only with the synthetic products turned out by the large chemical works, but also with the preparation and properties of the large number of natural products, both of plant and animal origin, which are largely used in

medicine, and we know of no book of this size or kind which contains such information in so readily accessible a form. Educationally this is a very great asset, inasmuch as the book is one which is likely to be read largely by students in search of information concerning the organic "synthetics"; such students, as a rule, have access only to books which ignore the other and equally if not more important subject for natural products. This circumstance is, however, remedied in the book under review, and the intellectual equipment of the rising generation of chemists will be considerably improved as a consequence. The various chemicals dealt with are grouped in sections according to their therapeutic uses, an arrangement which has much to recommend itself, for, although it is not based on any scientific foundation, it tends to associate once more the synthetic products with their older established natural companions in medicinal action. The whole subject is set forth very clearly, with as much practical detail as is consistent with the size of the book, or as is in some cases possible to give, where the processes are more or less secret. The last section, entitled, "Other Substances of Interest," includes Pituitary and Thyroid Extracts, Vitamines and Saccharines, and contains up-to-date information not usually found in text-books at the present day. The book may be thoroughly recommended as a most useful and valuable contribution to the literature of organic medicinal chemicals which makes accessible to the student a great deal of information which he has hitherto been deprived of from lack of opportunity, or time, or knowledge whence to acquire it.

P. H.

Principles of Biochemistry. By T. BRAILSFORD ROBERTSON, Ph.D., D.Sc. [Pp. xii + 633, illustrated with 49 engravings.] (Philadelphia and New York: Lea & Febiger, 1920.)

THIS is an extraordinarily interesting and suggestive book; it is professedly designed for students of medicine, agriculture, and related sciences, and, as stated in the preface, emphasis has been placed upon the practical applications in medicine, industries, and general biology. The book is divided into six parts, entitled The Foods, The Properties of Protoplasm, The Chemical Correlation of the Tissues, The Chemical Processes which underlie and accompany Life Phenomena, The Products of Tissue Activity, and The Energy and Balance of the Organism. A mere recital of these headings conveys no idea of the diversity of subjects described, and we know of no one book on Biochemistry which deals with so many diverse aspects of the subject. To those who work in any one branch of Biochemistry the author introduces a host of new ideas from other branches. To many readers it will come as a surprise to find what a number of applications are to be found for Biochemistry; in this respect Part IV is perhaps one of the most striking, and a perusal of its 140 odd pages shows how chemical principles can be applied in some way or another to such varied subjects as Bioluminescence, The Influence of Race, Sex, and Environment on the Growth Process, Memory and Sleep, etc. The reader may open the book at any page and be confident of finding some interesting and useful information, always set forth with extreme clearness, however difficult or unpromising the subject may be, and the book should be welcomed and appreciated by an ever-increasing circle of readers. P. H.

An Introduction to the Chemistry of Plant Products. Vol. I. By P. HAAS, D.Sc., and T. G. HILL, Ph.D. [Pp. xiii + 417. With diagrams. Third Edition.] (London: Longmans, Green & Co., 1921. Price 16s. net.)

THE collaboration of a plant chemist and a vegetable physiologist has resulted in the production of a book of use to all biologists. The histologist, cytologist, general zoologist, and of course the botanist, will all find something useful.

This book gives accounts of various physico-chemical phenomena as applied to organisms, such as will not be found elsewhere. The chapter on "The Colloidal State" will delight all biologists, and should be read carefully by senior students of both botany and zoology.

In this edition the original book has been broken up into two parts, the first herein reviewed, treating of the more chemical side of the subject; Part II, now in preparation, being devoted to more purely physiological problems.

This "Introduction" to the chemistry of Plant Products may be welcomed as a reliable statement of the present-day status of our knowledge of vegetable physiological chemistry.

J. B. G.

Soil Alkali: Its Origin, Nature, and Treatment. By F. S. HARRIS, Ph.D., Director Utah Agricultural Experiment Station. [Pp. xvi + 258, with 33 illustrations.] (New York: John Wiley & Sons; London: Chapman & Hall, 1920. Price 13s. 6d. net.)

THIS book is intended for students, agricultural advisers, and the better trained farmers, concerned with farming alkali lands. It attempts to discuss the widely scattered literature on a subject of ever-increasing importance, and bristling with urgent and unsolved problems.

Of the total land of the earth, no less than half is under arid conditions and is therefore liable to drought, and alkali, or both. European investigators have paid little or no attention to the subject, since Europe is practically free from soil alkali. But it is the only continent in this happy position.

The increasing demand for food has necessitated an increase in the land under cultivation, mainly in arid areas, and there the alkali problem, as the author points out, is twofold. Contaminated land must be reclaimed, and land free must be irrigated in such a way that alkali does not develop. A proper understanding of the nature and origin of alkali is necessary before any reliable advice can be given on its treatment. One of the main dangers is that increased evaporation, due to irrigation, brings up soluble salts and concentrates them at the soil surface. Hence, drainage is as important as irrigation. This is especially the case now that continuous irrigation is replacing the basin method. The latter only permits one crop per year, but at the same time it carries the deleterious salts downwards; the newer method, although allowing more crops, produces almost continuous evaporation and consequent accumulation of salts. This can be prevented, to a certain extent, by proper cultivation of the land, and by using less irrigation water. The book would be improved if the double value of reduced irrigation—both in the saving of water and the better crop—were discussed more fully. Material for this discussion already exists both in America and in the work of the Howards at Quetta, India.

The practical man will find the chapters dealing with native vegetation as an indication of alkali, and suitable crops for alkali land, of considerable interest and use. The chemist who has to analyse the soil for its alkali content will also find the appropriate chapters of interest, but for an entirely different reason. The author has described and compared the best known methods, and a perusal of them will show the chaotic condition of this branch of the subject. Under these circumstances the six whole pages of tables dealing with the electric bridge method of determining total salts could well be omitted. Tables are expensive to print, and the description given of the instrument is hardly sufficient to warrant their inclusion. In addition, recent work on conductivity of solutions throws considerable doubt on the accuracy of the results hitherto obtained. It is to be hoped that future editions of the book will contain, in place of the tables, a fuller discussion of Osterhout's views on antagonism.

The chapters dealing with the effect of alkali on physical conditions and the movements of salts through the soil are well suited to their purpose.

The book can be recommended, as an account, by a prominent worker in the subject, both of the present position of the alkali problem, and of practical methods of coping with it.

B. A. KEEN.

Plantation Rubber and the Testing of Rubber. By G. STAFFORD WHITBY, Ph.D., M.Sc., A.R.C.Sc. [Pp. xviii + 559, with plates and diagrams.] (London: Longmans, Green & Co. Price 28s. net.)

WHEN, in the early seventies, the first experiments were made on the cultivation of the wild Brazilian rubber trees on plantations in Ceylon, the promoters of the scheme can have little suspected that their enterprise would be so successful as to displace, within a space of forty odd years, the main source of natural rubber from the tropical forests of South America to the plantations of the old world. While so recently as 1906 the production of plantation rubber only amounted to less than 1 per cent. of the world's total output from Brazil and elsewhere, the percentage had increased to 79·5 in 1917. During this short period the production of Brazilian wild rubber had remained almost steady, while that of wild rubber from other sources had fallen to less than half its original value, and the enormously increased demand for rubber was therefore met almost entirely from the plantations of the Malay Peninsula, Sumatra, Java, and Ceylon. The circumstances of the production of the crude rubber from the two sources being so entirely different, it is not surprising that the methods of treating the material in the two cases are different; the scientific methods of production and control practised in the plantations cannot, of course, be applied in the case of rubber collected by natives working in more or less inaccessible regions of the forests on the Amazon. As the title states the present volume is concerned with the plantation industry, and a perusal of the book clearly demonstrates the complexity of the problems involved in standardising the methods of production and testing. The book is divided into two parts, devoted respectively to "The Preparation of Plantation Rubber" and "The Testing of Rubber." In the first part are set forth the very large number of factors which enter into the problem of obtaining a uniform material. Rubber is such a sensitive material that at all stages of its production circumstances may arise which will influence its physical properties and may increase the difficulties of its subsequent treatment; to mention only one example, there is the tendency of the latex to coagulate to "lumps" before it reaches the coagulating station, which means that precautions have to be adopted to check this objectionable property.

The second part of the book deals with a number of investigations made by physicists and others into the physical properties of rubber and the immensely important subject of vulcanisation and the various factors which influence the rate of vulcanisation, or "cure" as it is technically known. In the opinion of the author some of the physical properties of rubber have not yet received their due amount of exact experimental study. The book contains a great deal of information, and forms a valuable addition to the gradually increasing number of volumes issued under the general heading of Monographs in Industrial Chemistry.

P. H.

GEOLOGY

The Nomenclature of Petrology. By ARTHUR HOLMES. [Pp. 284.] (London: Thomas Murby & Co. Price 12s. 6d. net.)

PROBABLY few sciences are burdened with a nomenclature so unsystematic and unscientific as that of petrology. Rock names have been based on mineral characters, texture, mode of formation, colour, locality, and numerous

other principles. At the present time most new rock species are named after the locality in which the rock is first discovered, a method which has obvious disadvantages. Such terms as *garéwdite*, *fasibitikite*, and *onkilonite* are merely a tax on the memory and fail to give any suggestion of their connotation. Numerous attempts to systematise the nomenclature have been made, but all may be said to have failed. The announcement of Dr. Holmes's book raised hopes that a more successful effort was about to appear, but such hopes have not been realised. The title of the book is a misnomer, as practically no attempt at clearing up the question is made. A more appropriate title would have been a "Glossary of Petrological Terms," for the book is little more.

The appearance of such a glossary, however, is welcome, as most of the existing ones are somewhat out of date, and many new names have been introduced in the past few years. The definitions, so far as can be judged, are accurate and include not only rock-names but practically all the petrographic terms referring to structure, texture, mode of origin, and so forth. While no omissions of importance have been noted, future editions might include such terms as "clunch," "crowstone," "bullion," "flint-clay," and "gaize," which, though perhaps local in their application, are yet freely used in memoirs and papers.

While the reader is referred to Lœwinson-Lessing's *Lexique pétrographique* for the original papers in which many of the terms are defined, a series of recent references is appended to the definition of each term. Although in general the references are well chosen, the selection in a few instances is difficult to understand, important papers being omitted where others of less importance are inserted. For example, under "Fireclay," no mention is made of Mellor's work on the constitution of that rock, while Bowen's work does not appear under "Petrogenesis."

To those geologists who are not specialists in petrography, and to students the book should prove of great value, as much of the information could only be obtained by a diligent search through the literature. The author is to be commended, both for the accuracy and the completeness with which he has carried out his work. A very useful appendix of French and German terms, with their English equivalents, appears at the end of the book. Misprints are few and unimportant, the most noticeable occurring on pages 62, 76, 147, and 201.

A. S.

Notes on Geological Map-reading. By ALFRED HARKER. [Pp. 64, with 40 text-figures.] (Cambridge: W. Heffer & Sons. Price 3*s*. 6*d*. net.)

To the student of geology the subject of map-reading is of such importance that it is impossible for much progress to be made without a clear understanding of geological maps and their interpretation. Mr. Harker's book is written from the point of view that all the information contained in a geological map is to be read off directly, the drawing of sections being merely for the purpose of illustration. Despite the fact that an elementary knowledge of geometry is all that is required for the interpretation of such maps, students often find themselves in difficulties. This book should go far to dispel these difficulties, as the methods described are such as to eliminate the necessity for using trigonometrical formulæ, or even a protractor. After a preliminary discussion of typographical maps, the author deals with geological ones, in stages of increasing complexity, so that finally the reader should be capable of interpreting any published map. The descriptions are exceedingly lucid, and the diagrams which illustrate the methods very clear.

A. S.

A Text-book of Geology. By Philip Lake, M.A., F.G.S., and R. H. Rastall, M.A., F.G.S. [Pp. xiv + 508, with 33 plates and 134 text-figures.] Third Edition. (London: Edward Arnold, 1920. Price 21s. net.)

In the ten years which have elapsed since the appearance of the first edition, this book has proved its value to geological students. The publication of the third edition, therefore, is very welcome, especially as this is no mere reprint of the previous ones, but rather an enlarged and thoroughly up-to-date treatise on the subject. While the arrangement of the earlier editions has been retained, the various sections have undergone considerable revision, much recent work having been incorporated and several new sections added. Amongst the former may be noted the chapter on ore-deposits, which has been entirely rewritten, and which, though necessarily brief, gives an excellent summary of the subject. In the section on coral reefs an account is given of the recent American work, which has gone far to substantiate Darwin's theory of their formation.

The chapter on the sedimentary rocks contains a new section on "petroleum" and another on "concretions," in which, however, Liesegang's hypothesis might have been extended to these structures in sediments as well as in igneous rocks. At the end of the book a useful chapter on the history of igneous activity in the British Isles is appended. The occurrences of igneous rock are classified into five groups—Pre-Cambrian, Ordovician, Caledonian, Armorican, and Tertiary—and the relationship of each to the earth movements of the period indicated. While the Carboniferous volcanic and intrusive masses are described with the Caledonian group, no mention is made of the interesting Permo-Carboniferous masses of Ayrshire.

The book is excellently printed and has numerous well-chosen and well-reproduced illustrations; a full index is also provided.

A. S.

Mineralogy: An introduction to the Study of Minerals and Crystals. By E. H. Kraus, Ph.D., Sc.D., and W. F. Hunt, Ph.D. [Pp. xiv + 561, with 696 text-figures.] (New York: McGraw-Hill Book Co., 1920. Price 27s.)

This book provides, for the elementary student, a useful survey of the whole field of mineralogy, and, as the information is given in a clear and attractive manner, it should have a wide circle of readers. The first thirteen chapters are concerned with the general aspect of the subject, and include an elementary account of crystallography, a section on the polarising microscope, and discussions of the physical and chemical properties of minerals, concluding with a description of the methods used in qualitative blowpipe analyses. The trigonal system is included in the hexagonal, and no mention is made of the reference of crystals in the former to three axes instead of four. The substitution of the phrase "crystal-elements" by "elements of crystallisation" is unnecessary, and does not make for greater clearness. In the section on hardness, only Moh's scale is described, the more exact methods of determination being ignored.

In the descriptive part about one hundred and fifty minerals are treated, the properties, mode of origin, and localities being given for each. Notes on the uses to which each mineral is put are included, a feature which greatly improves the interest and utility of the book. The tendency to regard minerals as purely museum specimens is unfortunate, and it is to be hoped that, in the future, greater attention will be paid to the economic values of these substances. The last part of the volume is taken up with tables for the determination of the minerals described. While the illustrations, which include portraits of a number of distinguished mineralogists, are, on the whole, well reproduced, the photographic reproductions of crystal models and mineral specimens are not

very successful, and in some instances quite fail to show the essential features. The book is clearly printed, but the number of misprints is unnecessarily large. A. S.

BOTANY

The Coco-nut. By EDWIN BINGHAM COPELAND, Professor of Plant Physiology and Dean of the College of Agriculture (Retired), University of the Philippines. Second Edition, revised. [Pp. xvi + 225, with 28 illustrations.] (London: Macmillan & Co., 1921. Price £1 net.)

THIS work—the second edition of a book originally appearing in 1914—deals with the coco-nut more especially from the point of view of the cultivator; an interesting account of the manufacture of some of the more important products is provided, but this is not intended to be exhaustive and is included mostly because this cultivator at the present day is becoming more and more the provider of the finished products. A chapter is inserted on the physiology of the plant, to serve as an introduction to the consideration of the climatic and soil conditions necessary for its growth and to the study of its diseases, very full and helpful descriptions of the latter being provided. The selection and treatment of seed is next described, introducing us to a considerable section on field culture. The book is clearly and carefully written, and will prove useful to those who are interested in the coco-nut industry.

Cocoa and Chocolate: Their Chemistry and Manufacture. By R. WHYMPER. Second Edition. Revised and Enlarged. [Pp. xxi + 568, with 16 plates and 38 text-figures.] (London: J. & A. Churchill, 1921. Price 42*s*. net.)

THE aim of this book is to give a full and complete account of the different processes used in the manufacture of cocoa and chocolate and of the methods used in the analysis of the various products, including an account of the ways in which these analyses should be interpreted.

The Preface compares the method of manufacture and the position of the cocoa trade before 1914 and in the present period; while the introduction adduces evidence against the view that cocoa and chocolate are merely drugs, and gives reasons for regarding them as foods as well as stimulating beverages; the food value is dealt with more fully later in the book.

The first portion is taken up with a consideration of the history, botany, and agriculture of cacao—for it is the author's wish to give a general but not full account of the plant and its cultivation—the remaining two portions being concerned with the manufacture of chocolate and cocoa powders and the chemistry of cacao respectively. The treatment of the agricultural processes is more shortly described than the methods of manufacture and analysis, and at times is not sufficiently critical to be of practical value: no distinction is made between temporary shade for cacao plants and permanent shade-plants, the account of pruning is too short to be really helpful, and the account of the diseases of the crop certainly seems to confuse parasites with epiphytes and gives us no information regarding the shade trees which are suspected, as the coco-nut is, of passing an infectious disease to the cacao.

The two main sections of the work—and they constitute the real subject-matter—are very fully treated, a complete account being given of all the processes of manufacture from the cleaning and roasting of the cacao to the finishing of the various products. The last part describes the chemistry of the component parts of the beans, the microscopical examination of the products, and various methods for detecting impurities. An appendix of provisional definitions of cacao preparations, a bibliography, and an index are provided. E. M. C.

Botany with Agricultural Applications. By J. N. Martin, Ph.D. Second Edition. [Pp. xii + 604, with 490 figures.] (London: Chapman & Hall, 1920. Price 21*s.* net.)

The scope of this text-book is approximately that of the elementary college course, with stress laid on those aspects which more particularly concern the economic applications of Botany. The author begins by treating of the flowering plant, and the student is led from a consideration of the reproductive organs to a study of the morphology and physiology of seedlings. Chapters then follow dealing with cells and tissues, roots, stems, buds, in which pruning and grafting are described, and leaves. In all of these the appropriate physiological aspects are treated, though often very briefly.

The second part is concerned with the various plant groups, Variation, Heredity, and Evolution. There is also a short chapter on the ecological classification of plants and a concise account of plant-breeding methods. Throughout, economic plants are largely used for purposes of illustration.

A particular feature we would emphasise is that, though written for a special class of student, the broader view-point is not lost sight of. Although this cannot be too clearly evident, we venture to think that space might have been found for a more adequate account of the soil in relation to plants and for a fuller description of the bacteria.

A large number of types are dealt with in the section on the various groups, and here one rather doubts the educational value of including such specialised forms as *Pediastrum* and *Hydrodictyon*, or the details of sexual reproduction in the red algae, charales, etc.

The illustrations are well chosen, and the whole should prove a useful text to students of agriculture and others for whom the further matter on heredity and evolution in the present edition is a welcome addition. E. J. S.

Studies in Fossil Botany. By D. H. Scott, LM.A., L.D., D.Sc., Ph.D., F.R.S., F.L.S., F.G.S., F.R.M.S. Third Edition, Vol. I, Pteridophyta. [Pp. xxiii + 434, with 190 illustrations.] (London: A. and C. Black, 1920. Price 25*s.* net.)

The title of this book should be more properly "Studies in *Palæozoic* Fossil Botany," for, although mention is made of them here and there, few Mesozoic or Tertiary plants find their places in its pages. It is true that this classic work makes no claim to be a general text-book of Palæobotany, and, as was said in the Preface to the First Edition, it intends "to present to the botanical reader those results of palæontological inquiry which appear to be of fundamental importance from the botanist's point of view."

This was written when the first edition was published over twenty years ago, and in these twenty years this book has, without any question, held the leading place and influenced what little palæobotanical teaching there has been in our country. It has, therefore, during that time, set its mark upon and moulded the type of thought of British botanists and palæobotanists: although it has moulded them on a model of meticulous accuracy and lucid detailed exposition of the chief of those Palæozoic plants known to us from their anatomical structure, it has tended, at the same time, to narrow and to some extent warp our outlook on the whole subject of Palæobotany.

On glancing through the Index and the Contents Table one perceives at once examples to illustrate the above comment. For instance, sixty pages are devoted to the group of the *Equisetales*, and, although that family flourished in the Mesozoic times, and exists, of course, down to the present, less than two pages are devoted to any mention of plants other than Palæozoic specimens.

Even Palæozoic plants, if they chance not to be preserved so as to show their anatomical structure, tend to be relegated to a secondary place of interest. For instance, on page 110 appears a short description of that ancient and

exceptionally suggestive genus of Professor Nathorst's, *Pseudobornia*; but no picture of it is given, although Professor Nathorst's original publication was accompanied by a plate which would lend itself readily to excellent reproduction. A picture of this genus is needed because the original Swedish memoir is not too accessible, so that students do not generally see the paper and so obtain no clear mental picture of the peculiarities of the genus.

Then, again, 130 pages are devoted to the *Lycopodiales*, and in all these pages there is but a mere mention of the Mesozoic forms. That the Mesozoic is not systematically eliminated is seen when we turn to the chapters on the ferns, for there we find that both the Tertiary and the Mesozoic species of the *Osmundaceæ* are considered: very rightly; for their structure, which has been so well demonstrated in recent years from good material, affords beautiful illustrations of various points of morphological interest. But so also does the structure of *Tempskya*, though of this fern not a word is said. Yet *Tempskya* is known from beautiful anatomically preserved material, and it is a form of very widespread and frequent occurrence, which has also within recent years been made the subject of detailed work, revealing its extraordinary and unique stem morphology. This fern i quite as curious and as full of interest as anything in Palæobotany, but it finds no place in these pages.

The author says: "Space forbids us to extend our studies of the ferns to later epochs"; but the reader cannot forbear to ask, Why? seeing that this book of studies has expanded itself from one small single volume to its present form in two greatly enlarged volumes.

No other text-book of Palæobotany exists in which are presented the leading forms of interest with such detail, such accuracy, and such illuminating insight, so that it is all the more to be regretted that the vision is myopic.

To the influence of this book may be traced the high quality of much of the Palæobotanical work done in our country, and also our national defect in Palæobotany—that is, our lack of appreciation of the balance of the *whole* of our science.

ZOOLOGY

A Text-book of Biology. By William Martin Smallwood, Ph.D. (Harvard). [Pp. xvi + 308, with 229 engravings and 3 plates in colours.] (Philadelphia and New York: Lea and Febiger. Price $3.50.)

This is the fourth edition of Professor Martin Smallwood's text-book. It is meant for students in medical and general technical courses, but it is not extensive enough for any of the First Medical or Science Biology examinations in this country. The new edition is attractively written, though the English is often very bad—especially so from a Professor in a Liberal Arts College. Here and there the accounts given of various life histories are slipshod and inaccurate. This applies especially to the author's remarks on malaria (page 218); the correct adjective corresponding to Anopheles is "anopheline." Fertilisation is not "the union of the male chromatin (chromosomes) and the female chromatin." The manuscript should really have been read by an expert biologist, and such mistakes deleted. Otherwise the book is most readable, and should prove a useful adjunct to the student already provided with proper text-books of Biology.

J. B. G.

Zoology. An Elementary Text-book. By Sir A. E. Shipley, Sc.D., and Prof. E. W. MacBride, F.R.S. [Pp. xx + 752, with 360 illustrations. Fourth Edition.] (Cambridge: at the University Press, 1920. Price 20*s.* net.)

This is the fourth edition of this well-known and useful work. The present volume has been revised by Prof. MacBride, and contains new matter on the

physiology of the bivalve mollusca, the development of centra according to Ridewood's results, and a revision of the Anthropology section following the results of Elliot Smith, Ripley, and Keith.

This book is particularly suitable for the large body of non-zoologists, such as medical men, who would like to know something more about zoology and comparative anatomy. J. B. G.

The Nature of Animal Light. By E. Newton Harvey, Ph.D. [Pp. x + 182, with 35 figures and 13 tables.] (Philadelphia and London: J. B. Lippincott & Co., 1920. Price $3.50.)

This, the fifth volume of the "Monographs on Experimental Biology," fully maintains the high level set by the previous four, both in the matter presented, the production and arrangement. Although it is probably realised in a general way that "phosphorescence" has been studied to a certain extent, it is only on reading a volume like the present with its extensive bibliography and its wide treatment that the full significance and extent of the problem becomes clear. The headings of the various chapters give a fair idea of the scope of subject:—Light-producing Organisms, Luminescence and Incandescence, Physical Nature of Animal Light, Structure of Luminous Organs, the Chemistry of Light Production, and the Dynamics of Luminescence. The old term "phosphorescence," often used in regard to animals, should no longer be employed. By the physicist this is applied to the phenomenon of the emission of light by bodies after previous illumination or radiation, and is not found in the animal world. It should be more accurately termed chemiluminescence or oxyluminescence since it is absolutely dependent upon a proper oxygen supply. Since in animals it is associated with structural peculiarities as well as chemical ones, it is perhaps better to employ the term bioluminescence to the phenomenon. In certain cases at any rate, it has been shown that it is due to the interaction of two substances—Luciferin and Luciferase—in the presence of water and oxygen. The two substances differ in themselves, and are also slightly different in different animals. Luciferase is a protein allied to the albumins, while Luciferin is not digested by proteolytic ferments and is dialysable.

Quite a large number of animals exhibit bioluminescence, and it is interesting to note that their luminous efficiency may reach ·96 as compared with the ·032 of a 600 c.p. 20 amp. metal filament electric lamp.

The whole subject is one of great interest, and zoologists and comparative physiologists are under a debt to Professor Harvey for the masterly way in which he has included so much interesting matter within a comparatively small compass. C. H. O'D.

Amœboid Movement. By Asa A. Schæffer, Ph.D. [Pp. vii + 156, with 46 illustrations.] (Princeton, U.S.A.: University Press, and Oxford Press, 1920. Price $2.50.)

As is well known, Professor Schæffer has spent a great deal of time in studying various species of *Amœba* from numerous points of view, and the present volume is a statement of his observations and conclusions on the subject of Amœboid movement. Strangely enough, the discoverer of *Amœba*, Rösel v. Rosenhof, when describing the animal in 1755, made one of the first and most important observations on this physiological question. He pointed out that the changes in form are associated with a streaming of the endoplasm. The whole question is one of importance, since in *Amœba* we undoubtedly have a quite primitive animal, and moreover, in view of the importance of the leucocytes and phagocytes in the higher animals, anything that adds to our knowledge of their activities is to be welcomed. There is a considerable amount of work on the question by numerous observers and all of this is reviewed critically

in the light of the author's own experiments. The book is divided into a series of chapters each dealing with some special phase of the movement, *e.g.* Transformation of Endoplasm into Ectoplasm; Experiments on the Surface Layer; Streaming, Contractibility, etc. In addition to this, Professor Schæffer also directs attention to a noteworthy fact, namely that the Amœba in progression follows an orderly path representing a projection on a plane surface of a helical spiral unless it is interfered with by external stimuli. This is developed further, and it is pointed out that it is related to the similar movement of free-swimming organism like ciliates, flagellates, rotifers, worm larvæ, etc., and may even be reproduced in the higher animals, including man himself, if the organs of orientation are not functioning. The similarity of the effects of alteration of temperature upon those orderly paths in different animals suggests that there is an underlying physical reason for this mode of progression. The book is well and thoughtfully written, and contains a useful bibliography. C. H. O'D.

Territory in Bird Life. By H. ELIOT HOWARD. [Pp. xiii + 308, with 11 plates by G. E. Lodge and H. Grönvold, and 2 plans.] (London: John Murray. Price 21*s*. net.)

MR. HOWARD'S present book is an elaboration of his theory of bird territory as outlined in his earlier celebrated work on the British Warblers. Mr. Howard sets out to prove that the possession of territory is the dominating impulse that accounts for most, if not all, of the actions of a male bird in the mating season, even the almost universal habit of migration being attributed to this stimulus. The theory is an exceedingly interesting one, and gains weight from the mass of careful observations that the author marshals in its support.

That birds appear actually to own and hold sole sway over certain tracts of land during the breeding season, the centre of which is, during the main part of the period at all events, their nest, is well known to all field workers. That the owner resents the intrusion of other individuals of his own, and frequently unrelated species, is equally well known. Mr. Howard demonstrates that the selection of a territory is the first step towards mating and reproduction, and that that selection takes place before a mate is secured. Already at that stage it is the cause of innumerable battles, and therefore these, almost universally attributed to the influence of the female, are in reality fights for the possession of land, and not a mate. In this connection the author points out that, of most migratory species, the males are the first to come, the females in some cases arriving at a much later date. Having established this point, he dilates on the importance of the possession of a definite territory in securing a mate. He points out that in this manner the males are fairly equally distributed over their range, and the females, when they do arrive, have no difficulty in finding a mate. Without the territory and its consequent effect of equal distribution, the females might have great difficulty in attaining this end. Thus time and energy are saved, and early and successful reproduction for the majority is ensured. To the importance of song as an advertising medium for the site of the males and their stations, a chapter is devoted. The relation of different species to each other, and the overlapping of their ranges, is treated at length in another chapter. Finally, the possession of a territory is cited as the main inducement for the northern migration in the spring—a truly startling conclusion.

Though we may not accept Mr. Howard's views in their entirety, we must congratulate him on a piece of work of exceptional interest. Throughout he supports his statements with accurate observations, and from these deduces his arguments with very sound logic. There is no doubt that the work is an important addition to ornithological literature.

The plates by Lodge and Grönvold are in photogravure, and are the best examples of this pleasing type of illustration that we can recall.

A rather incomplete index is provided. W. R.

A Naturalist in Himalaya. By R. W. G. Hingston, M.C., M.B., Fellow of the Royal Geographical and Zoological Societies; Captain, Indian Medical Service. [Pp. xii + 300, with 16 plates and several text figures.] (London: Witherby, 1920. Price 18*s.* net.)

In this delightful volume the author gives us some excellent pictures of insect and vertebrate life from an interesting and little-known corner of the world, the Himalayan Valley of Hazara.

The greater part of the book is devoted to insects, and in dealing with them and their ways our author is in his happiest mood. It is difficult to conceive of a writer on this topic making his subject more real and living than Captain Hingston has succeeded in doing. To the initiated the habits of insects are always full of interest. By the man in the street they are usually considered extremely dull. It is safe to say that the latter will derive as much enjoyment from this volume as the most ardent entomologist. It is more fascinating, and a great deal more wholesome, than many a present-day novel.

To the scientist *A Naturalist in Himalaya* will yield a vast amount of fresh information. The observations on ants, termites, and spiders in particular are those of an enthusiastic and untiring observer and one who knows his subject intimately. Some of the conclusions drawn are distinctly original and are supported by the evidence of many laborious and ingenious experiments. One striking fact continually brought out by the author is the apparent lack of intelligence displayed by the organisms under observation. The marvellous division of labour amongst the ants, the mathematical precision of the geometrical spiders, the care of wasps for their eggs, and many other seemingly intelligent actions, are born of instinctive routine, not of intellect. The instinct cannot be thwarted, nor can the insect interrupt its routine. It is incapable of rising to an emergency because its acts are not intelligent.

Numerous experiments were carried out to test the senses of the various insects dealt with which have revealed some extremely interesting facts. The light produced by the Glow-worm, the music of the Cicada, the weaving of the geometrical web, the unimpeded passage of the spider over its own sticky snare, were a few of the phenomena particularly investigated with especial reference to the mechanisms that make them feasible. But the volume is so full that it is impossible to attempt even an outline of its scope.

There is a single chapter on mammals and another on birds, the author concluding with a geological sketch of the district involved. This portion of the book includes a fine series of photographs in illustration of the section dealing with emotional expression in the Leopard. An index is provided, which would, however, be more valuable were it somewhat fuller. This is a pity in a book otherwise, as is the custom of Messrs. Witherby, perfectly produced. W. R.

MISCELLANEOUS

Geometrical Investigation of the Formation of Images in Optical Instruments, Embodying the Results of Scientific Researches conducted in German Optical Workshops. Edited by M. von Rohr. Translated by R. Kanthack. [Pp. xxiii + 612, with 133 text figures.] (London: Printed and published for the Department of Scientific and Industrial Research by His Majesty's Stationery Office, 1920. Price £2 5*s.* net.)

Soon after the establishment of a Committee of the Privy Council for Scientific and Industrial Research, their attention was called by their Advisory Council,

who were assisted by the Standing Committee on Glass and Optical Instruments, to the deficiency of books in the English language dealing with geometrical and technical optics. Accordingly they authorised, in the interests of the scientific and industrial development of optics, translations of Dr. Gleichen's theory of modern optical instruments, and Dr. von Rohr's theory of optical instruments. The translation of the former work was first published by the Stationery Office for the Department of Scientific and Industrial Research in 1918. On the recommendation of the Standing Committee on Glass and Optical Instruments, Mr. R. Kanthack was invited to translate Dr. von Rohr's book.

Based upon the fundamental geometrical laws of light, this work deals with the methods of computation of optical systems. It embodies the exhaustive researches made by eminent men of science, amongst whom British investigators occupy an important place. The translator says in his preface that the inception of the work with which he was entrusted represented to him far more than the preparation of an English version of a work which may be regarded as the most valuable modern elucidation of the principles of constructive optics. He felt convinced, from personal observation as well as the testimony of others competent to express unbiassed opinions, that the achievements of German workshops and the sympathetic development of the cognate theoretical studies could not be accounted for solely by education and by State assistance. In a very large measure the success of the Germans as opticians is the outcome of a far-seeing policy, requiring for its conception something of the imagination and courage of an idealist. The famous Zeiss works at Jena and its history furnish no more than a somewhat overshadowing example of a widespread and single-minded association of scientific investigation with workshop practice and industrial aims. It is not enough to equip workshops with all the best that modern technique and organisation have to offer, and to secure the services of the best available scientific brains for carrying out the industrial programme of an establishment, leaving to the *savants* all scientific effort which has no direct bearing on production. The hope which the translator cherished when he undertook the work is that the book may appeal to the imagination of British opticians, and help to stimulate further the rising spirit of scientific enthusiasm which looks beyond the immediate needs of successful production.

This book is not one which can be subjected to criticism. It is, in fact, the embodiment of all that is known of theoretical optics in relation to those parts of the subject with which it deals. It has been studied by a competent committee, and at their instigation the translation has been produced.

The book covers the whole range of matter as indicated by its title, and each section of it has been dealt with by an acknowledged authority on each branch of the subject. The translator has been extraordinarily careful in eliminating errors, and the mathematical part has been most efficiently dealt with.

There is an author's index as well as a general index to the work, and this provides a practically complete bibliography of the subject. The list of symbols also is a great aid to a reader, as by its means the mathematical formulæ can be interpreted without difficulty.

The book may be regarded as indispensable to anyone who is studying the theory of optical instruments, either as to its theoretical side or in its practical application.

J. E. B.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

The Theory of Functions of a Real Variable and the Theory of Fourier's Series. By E. W. Hobson, Sc.D., LL.D., F.R.S., Sadleirian Professor of Pure Mathematics and Fellow of Christ's College in the University of Cambridge. Second Edition, revised throughout and enlarged. Vol. I. Cambridge: at the University Press, 1921. (Pp. xvi + 671.) Price 45*s*. net.

Lectures on the Principle of Symmetry, and its Applications in all Natural Sciences. By F. M. Jaeger, Ph.D., Professor of Inorganic and Physical Chemistry in the University of Groningen, Netherlands. Second Augmented Edition. Amsterdam: Publishing Company "Elsevier," 1920. (Pp. xii + 348, with 173 diagrams and 3 portraits.)

The Elementary Differential Geometry of Plain Curves. By R. H. Fowler, M.A., Fellow of Trinity College, Cambridge. Cambridge: at the University Press, 1920. (Pp. vii + 105.) Price 6*s*. 6*d*. net.

The Dynamics of the Airplane. By Kenneth P. Williams, Ph.D., Associate Professor of Mathematics, Indiana University. Mathematical Monographs, edited by Mansfield Merriman and Robert S. Woodward. No. 21. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. viii + 138.) Price 13*s*. 6*d*.

The Mechanical Principles of the Aeroplane. By S. Brodetsky, M.A., Ph.D., F.Inst.P., A.F.R.Ae.S., Reader in Applied Mathematics, University of Leeds. London: J. and A. Churchill, 7 Great Marlborough Street, 1921. (Pp. vii + 272, with 119 illustrations.) Price 21*s*. net.

Fermat's Last Theorem. Proofs by Elementary Algebra. By M. Cashmore. Third Edition. London: G. Bell & Sons, 1921. (Pp. 67.) Price 2*s*. 6*d*. net.

Calculus for Beginners. A Text-book for Schools and Evening Classes. By H. Sydney Jones, M.A., Head Master, Barnstaple Grammar School. London: Macmillan & Co., St. Martin's Street, 1921. (Pp. ix + 300.) Price 6*s*. net.

Meteorology. An Introductory Treatise. By A. E. M. Geddes, O.B.E., M.A., D.Sc., Lecturer in Natural Philosophy in the University of Aberdeen. London: Blackie & Sons, 50 Old Bailey, 1921. (Pp. xx + 390, with 20 plates and 103 diagrams.) Price 21*s*. net.

Periodic Orbits. By F. R. Moulton, in Collaboration with Daniel Buchanan, Thomas Buck, Frank L. Griffin, William R. Longley, and William D. MacMillan. Washington: The Carnegie Institution of Washington, 1920. (Pp. xiii + 524.)

The Elements of Theoretical and Descriptive Astronomy for the Use of Colleges and Academies. By Charles J. White, A.M. Third Edition, revised by Paul P. Blackburn, Commander U.S. Navy. New York: John Wiley & Sons; London: Chapman & Hall, 1920. (Pp. xi + 309, with 9 plates and 84 figures.) Price 17*s*. 6*d*. net.

Relativity, the Electron Theory, and Gravitation. By E. Cunningham, M.A., Fellow and Lecturer of St. John's College, Cambridge. Second Edition. London: Longmans, Green & Co., 39 Paternoster Row, E.C., 1921. (Pp. vii + 148.) Price 10*s*. 6*d*. net.

A Brief Account of Radio-Activity. By Francis P. Venable, Ph.D., D.Sc., LL.D., Professor of Chemistry, University of North Carolina. New York: D. C. Heath; London: George G. Harrap & Co. (Pp. vi + 54.) Price 3*s*. 6*d*. net.

The Elements of the Mathematical Theory of Electricity and Magnetism. By Sir J. J. Thomson, O.M., M.A., Sc.D., D.Sc., LL.D., Ph.D., F.R.S., Master of Trinity College, Cambridge, Professor of Physics in the University of Cambridge, Professor of Natural Philosophy in the Royal Institution, London. Fifth Edition. Cambridge: at the University Press, 1921. (Pp. 410.) Price 25*s.* net.

Ammonia and the Nitrides, with Special Reference to their Synthesis. By Edward B. Maxted, Ph.D., B.Sc., F.C.S. London: J. and A. Churchill, 7 Great Marlborough Street, 1921. (Pp. vi + 116, with 16 illustrations.) Price 7*s.* 6*d.* net.

The Chemistry of Synthetic Drugs. By Percy May, D.Sc., F.I.E. Third Edition, revised. London: Longmans, Green & Co., 39 Paternoster Row, 1921. (Pp. xv + 248.) Price 12*s.* 6*d.* net.

This edition differs but little from that of three years ago. Most of the typographical errors have been corrected, though two on p. 50 have escaped notice; a few paragraphs have been modified, and some minor additions made.

Creative Chemistry. Descriptive of Recent Achievements in the Chemical Industries. By Edwin E. Slosson, M.S., Ph.D. London: University of London Press, 18 Warwick Square, E.C.4, 1921. (Pp. xii + 311, with 38 illustrations.) Price 12*s.* 6*d.* net.

The Electronic Conception of Valence and the Constitution of Benzene. By Harry Shipley Fry, Ph.D., Professor of Chemistry and Director of the Chemical Laboratory, University of Cincinnati. London: Longmans, Green & Co., 39 Paternoster Row, 1921. (Pp. xviii + 300.) Price 16*s.* net.

Applied Colloid Chemistry. General Theory. By Wilder D. Bancroft, Professor of Physical Chemistry at Cornell University. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. viii + 345.) Price 18*s.* net.

The Chemistry of Plant Life. By Roscoe W. Thatcher, M.A., D.Agr., Dean of the Department of Agriculture and Director of the Agricultural Experiments Station, University of Minnesota. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. xvi + 268.) Price 18*s.* net.

Chemistry of Pulp and Paper Making. By Edwin Sutermeister, S.B. New York: John Wiley & Sons; London: Chapman & Hall, 1920. (Pp. vii + 479, with 55 figures.) Price 36*s.* net.

This book contains chapters on Cellulose; Fibrous Raw Materials; The Soda Process; The Sulphate Process; Bleaching; Sizing; Colouring; Testing Wood Pulps, and many other processes connected with paper-making.

Chemical Technology and Analysis of Oils, Fats, and Waxes. By Dr. J. Lewkowitsch, M.A., F.I.C., late Consulting and Analytical Chemist, and Chemical Engineer Examiner in "Soap Manufacture" and in "Fats and Oils" to the City and Guilds of London Institute. Edited by George H. Warburton. Sixth Edition, entirely rewritten and enlarged. In Three Volumes. Vol. I. London: Macmillan & Co., St. Martin's Street, 1921. (Pp. xviii + 682, with 59 figures.) Price 36*s.* net.

Vocational Chemistry for Students of Agriculture and Home Economics. By John J. William, Ph.D., Assistant Professor of Agricultural Biochemistry, School of Agriculture, University of Minnesota. Farm Life Text Series. Edited by K. C. Davis, Ph.D., Philadelphia and London: J. B. Lippincott Company. (Pp. viii + 294, with 70 illustrations.) Price 8*s.* 6*d.* net.

Experimental Organic Chemistry. By Augustus P. West, Ph.D., Professor of Chemistry, University of the Philippines. London: George G. Harrap, 2 and 3 Portsmouth Street, Kingsway, W.C., 1921. (Pp. xiii + 469.) Price 10*s*. 6*d*. net.

Thermodynamics and Chemistry. By F. H. MacDougall, M.A., Ph.D., Associate Professor of Chemistry, University of Minnesota. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. v + 391.) Price 30*s*. net.

Famous Chemists: The Men and their Work. By Sir William A. Tilden, F.R.S., D.Sc., LL.D., Sc.D., Professor Emeritus of Chemistry in the Imperial College of Science and Technology. London: George Routledge & Sons; New York: E. P. Dutton, 1921. (Pp. vi + 296, with 38 illustrations.) Price 12*s*. 6*d*. net.

A Text-book of Geology. For Use in Universities, Colleges, Schools of Science, etc., and for the General Reader. Part I. Physical Geology. By Louis V. Pirsson, late Prof. of Physical Geology in the Sheffield Scientific School of Yale University. Part II. Historical Geology. By Charles Schuchert, Professor of Palæontology in Yale University, and of Historical Geology in the Sheffield Scientific School. Part I. Second Revised Edition. New York: John Wiley & Sons; London: Chapman & Hall, 1920. (Pp. vii + 470, with 311 figures.) Price 17*s*. 6*d*. net.

Field Methods in Petroleum Geology. By G. H. Cox, Ph.D., E.M., C. S. Dake, M.A., and G. A. Muilenburg, M.A. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. xiv + 305.) Price 24*s*. net.

Electrolytic Deposition and Hydrometallurgy of Zinc. By Oliver C. Ralston, Metallurgist, Hooker Electrochemical Co., Niagara Falls, N.Y. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. vii + 201.) Price 18*s*. net.

The Geology of the British Empire. By F. R. E. Reed, M.A., Sc.D., F.G.S., F.R.G.S. London: Edward Arnold, 1921. (Pp. viii + 480, with maps.) Price 40*s*. net.

Catalogue of the Fossil Bryozoa (Polyzoa) in the Department of Geology, British Museum (Natural History). The Cretaceous Bryozoa (Polyzoa). Volume III. The Cribrimorphs. Part I. By W. D. Lang, Sc.D., F.G.S. London: Printed by order of the Trustees and sold by Longmans, Green & Co., 39 Paternoster Row, and at the British Museum (Natural History), Cromwell Road, S.W.7, 1921. (Pp. 268, with 8 plates.) Price 30*s*.

Studies in French Forestry. By Theodore S. Woolsey, Jr., Consulting Forester, and Executive Member Interallied War Timber Committee, Paris, 1917-19. With two chapters by William B. Greeley, Forester, U.S. Forest Service, and formerly Chief, Forestry Section, C. and F., S.O.S., American Expeditionary Forces. New York: John Wiley & Sons; London: Chapman & Hall, 1920. (Pp. xxvi + 550.) Price 26*s*. net.

How to Teach Agriculture. A Book of Methods in this Subject. By Ashley V. Storm, Ph.D., and Kary C. Davis, Ph.D. London: J. B. Lippincott Company. (Pp. vii + 434, with 223 illustrations.) Price 12*s*. 6*d*. net.

Germination in its Electrical Aspect. A Consecutive Account of the Electro-Physiological Processes concerned in Evolution, from the Formation of the Pollen-grain to the Completed Structure of the Seedling, together with some Further Studies in Electro-Physiology. By A. E. Baines. London: George Routledge & Sons. New York: E. P. Dutton & Co., 1921. (Pp. xxv + 185, with 130 figures.) Price 12*s*. 6*d*. net.

Le Mouvement Biologique en Europe. By Georges Bohn, Directeur de Laboratoire à la Sorbonne, Paris: Libraire Armand Colin, 103 Boulevard Saint-Michel, 1921. (Pp. 144.) Price 4 frs.

British Mammals. Written and illustrated by A. Thorburn, F.Z.S., with 50 plates in colour and pen-and-ink sketches in the text. In Two Volumes. Vol. I. London: Longmans, Green & Co., 39 Paternoster Row, 1920. (Pp. iii + 84.) Price 30*s*. Orders taken for complete work only.

A Text-book of Oceanography. By J. T. Jenkins, D.Sc., Ph.D. London: Constable & Co., 10-12 Orange Street, W.C.2, 1921. (Pp. x + 206, with 42 figures.) Price 15*s*. net.

The Resources of the Sea. As shown in the Scientific Experiments to Test the Effects of Trawling and the Closure of Certain Areas off the Scottish Shores. By William Carmichael McIntosh, M.D., LL.D., D.Sc., F.R.S., Director of the Museum and of the Gatty Marine Laboratory, and Member of the Fishery Board for Scotland. Second Edition. Cambridge: at the University Press, 1921. (Pp. xvi + 344, with 19 plates.) Price 35*s*. net.

The Natural History of South Africa: Mammals. Including the Vervet Monkeys, Baboons, Galagos, Fruit Bats, Insectivorous Bats, Lions, Leopards, Serval Cats, Black-footed Cats, African Wild Cats, Caracals, and Hunting Leopards. By F. W. Fitzsimons, F.Z.S., F.R.M.S., etc., Director, Port Elizabeth Museum. In Four Volumes. London: Longmans, Green & Co., 39 Paternoster Row, 1919. (Pp., Vol. I, xix + 178, with 52 illustrations; Vol II, xi + 195, with 48 illustrations; Vol. III, xiii + 278, with 58 illustrations; Vol. IV, xix + 271, with 59 illustrations.) Price, Vols. I and II, 9*s*. each; Vols. III and IV, 12*s*. 6*d*. each.

Monograph of the Lacertidæ. By George Albert Boulenger, LL.D., D.Sc., F.R.S. Volume I. London: Printed by order of the Trustees of the British Museum (Natural History), Cromwell Road, S.W.7, 1920. Pp. x + 352.) Price 40*s*.

Man and his Past. By O. G. S. Crawford. London: Oxford University Press, 1921. (Pp. xv + 227, with 13 illustrations.) Price 10*s*. 6*d*. net.

Principles and Methods of Physical Anthropology. By Rai Bahadur Sarat Chandra Roy, M.A., Honorary Member, Folklore Society, London. Patna University Readership Lectures. Patna: Superintendent, Government Printing, Bihar and Orissa, 1920. (Pp. vii + 181.) Price 5 Rs.

The Physiology of Protein Metabolism. By E. P. Cathcart, M.D., D.Sc., F.R.S., Gardiner Professor of Chemical Physiology, University of Glasgow. Formerly Professor of Physiology in the University of London. London: Longmans, Green & Co., 39 Paternoster Row, 1921. (Pp. ii + 176.) Price 12*s*. 6*d*. net.

The Physiology of Nervous Ailments. A Brief Description of the Freudian Analytic Method of adjusting Nervous Disturbances. By Joseph Ralph. Published by the Author, "Glenthorne," Rousdown Road, Chelston, Torquay. (Pp. 62.) Price 1*s*. 6*d*. net.

The Origin and Problem of Life. A Psycho-physiological Study. By A. E. Baines. London: George Routledge & Sons: New York: E. P. Dutton & Co. (Pp. xii + 97, with 15 figures.) Price 3*s*. 6*d*. net.

Œdema and Nephritis. A Critical, Experimental, and Clinical Study of the Physiology and Pathology of Water Absorption in the Living Organism. By Martin H. Fischer, Doctor of Medicine, Eichberg, Professor of Physiology in the University of Cincinnati. Third and Enlarged Edition. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. xvi + 922.) Price 55*s*. net.

Essentials of Physiology. By F. A. Bainbridge, M.D., D.Sc., F.R.C.P., F.R.S., and J. Acworth Menzies, M.A., M.D. Fourth Edition. London: Longmans, Green & Co., 39 Paternoster Row, 1920. (Pp. iv + 497, with 194 illustrations.) Price 14*s*. net.

Encyclopædia Medica. Under the General Editorship of J. W. Ballantyne, M.S., C.M., F.R.C.P.E. Volume VII. Intestines to Labour. Edinburgh and London: W. Green & Son, 1921. (Pp. vii + 611.) Price 26*s*.

The Mothers' Clinic. For Birth Control. By Marie Carmichael Stopes, D.Sc. Ph.D. London: The Mothers' Clinic, 61 Marlborough Road, Holloway, N., 1921. (Pp. 12.)

This little pamphlet gives the history of the formation of a Clinic founded by the author of the pamphlet and her husband, Humphrey Verdon Roe. The Clinic was opened on March 17, 1921, and includes many well-known names in its list of patrons.

Wireless Telegraphy, with Special Reference to the Quenched-spark System. By Bernard Leggett, A.M.I.E.E., late Wireless Technical Staff, Messrs. Siemens Bros. & Co., late Officer in Charge of Trench Wireless, 1st Corps, B.E.F. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1921. (Pp. vi + 485, with 230 figures.) Price 30*s*. net.

Opportunities in Engineering. By Charles M. Horton. London: University of London Press, 18 Warwick Square, E.C.4, 1921. (Pp. iv + 90.) Price 3*s*. 6*d*. net.

Artificial Light: Its Influence upon Civilisation. By M. Luckiesh, Director of Applied Science, Nela Research Laboratory, National Lamp Works of General Electric Company. London: University of London Press. (Pp. xiv + 366, with 41 illustrations.) Price 12*s*. 6*d*. net.

Electrical Engineering. By T. F. Wall, D.Sc., D.Eng., Assoc.M.Inst.C.E., A.M.I.E.E. Department of Applied Science, University of Sheffield, London: Methuen & Co., 36 Essex Street, W.C., 1921. (Pp. xi + 491, with 4 plates and 461 figures.) Price 21*s*. net.

Gas Torch and Thermit-welding. By Ethan Viall, Editor, American Machinist. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. xi + 434, with 342 illustrations.) Price 22*s*. net.

Electric Welding. By Ethan Viall, Editor, American Machinist. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. xii + 417, with 329 illustrations.) Price 22*s*. net.

Elements of Fuel, Oil, and Steam Engineering. A Practical Treatise dealing with Fuel Oil for the Central Station Man, the Power Plant Operator, the Mechanical Engineer, and the Student. By Robert Sibley, B.S., and C. H. Delany, B.S., M.M.E. Second Edition. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. xix + 466, with 248 illustrations.) Price 28*s*. net.

Copper Refining. By Lawrence Addicks, Consulting Engineer, New York City. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. xi + 206, with illustrations.) Price 17*s*. net.

Gasoline Automobiles. By James A. Moyer. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. vii + 261, with 212 illustrations.) Price 11*s*. net.

Technical Methods of Analysis, as employed in Laboratories of Arthur D Little, Cambridge, Mass. Edited by Roger Castle Griffin. New York McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. xv + 666, with 29 illustrations. Price 33*s*. net.

Principles of Human Geography. By Ellsworth Huntington, Research Associate in Geography, Yale University; and Sumner W. Cushing late Head of the Department of Geography in the State Normal School of Salem, Mass. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. xiv + 430, with 118 figures.) Price 21*s*. net.

Annual Magazine Subject-Index: 1919. A Subject-Index to a Selected List of American and English Periodicals and Society Publications. Edited by Frederick Winthrop Faxon, A.B. (Harv.). Compiled with the co-operation of Librarians. Boston. The F. W. Faxon Company, 1920. (Pp. 241.)

Dictionary of British Scientific Instruments. Issued by the British Optical Instrument Manufacturers' Association. London: Constable & Co., 1921. (Pp. xii + 334, with 312 illustrations.) Price 21*s*. net.

The Mystery of Space. A Study of the Hyperspace Movement in the Light of the Evolution of the New Psychic Faculties and an Inquiry into the Genesis and Essential Nature of Space. By Robert T. Browne. London: Kegan Paul, Trench, Trübner & Co., New York. E. P. Dutton & Co. (Pp. xvi + 395.) Price 15*s*. net.

The Great Riddle, or, the Action and Effects of Natural Forces and Conditions in the Creation. By Frank Horridge. London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co., 1921. (Pp. 99.) Price 3*s*. 6*d*. net.

The Essentials of Mental Measurement. By William Brown, M.A., M.D., D.Sc., and Godfrey H. Thomson, D.Sc., Ph.D. Cambridge: at the University Press, 1921. (Pp. x + 216.) Price 21*s*. net.

Studies in the History and Method of Science. Edited by Charles Singer. London: Clarendon Press, 1921. (Pp. xxii + 560, with 55 plates and 104 figures in the text.) Price 48*s*. net.

The Unity of Science: A Sketch. By Dr. Johan Hjort, F.R.S., Professor at the University of Kristiana. London. Gyldendal, 11 Burleigh Street, Covent Garden, W.C.2, and Copenhagen and Christiania. (Pp. vi + 176.) Price 6*s*. net.

Laboratories: Their Planning and Fittings. By Alan E. Munby, M.A., F.R.I.B.A., with an Historical Introduction by Sir Arthur E. Shipley, G.B.E., Sc.D., LL.D., F.R.S. London: G. Bell & Sons, 1921. (Pp. xix + 220, with 165 figures.) Price 25*s*. net.

Counsels and Ideals from the Writings of William Osler. Second Edition. London: Oxford University Press, 1921. (Pp. xxiv + 355.) Price 8*s*. 6*d*. net.

Scientific Theism *versus* Materialism. The Space-time Potential. By Arvid Reuterdahl, Dean of the Department of Engineering and Architecture, the College of St. Thomas. New York: The Devin-Adair Company, 1920. (Pp. 298.)

Moral Theory: an Introduction to Ethics. By G. C. Field, M.A., B.Sc., Lecturer in Philosophy in the University of Liverpool. London: Methuen & Co., 36 Essex Street, W.C. (Pp. x + 214.) Price 6*s*. net.

Geography, Physical, Economic, Regional. By James Franklin Chamberlain, Ed.B., S.B. London: J. B. Lippincott Company. (Pp. xviii + 505, with 210 illustrations.) Price 15*s*. net.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury.

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PURE MATHEMATICS. By Dorothy M. Wrinch, D.Sc., Fellow of Girton College, Cambridge, and Member of the Research Staff, University College, London.

In the current number of the *Proceedings of the Royal Society* (A. 697, issued May 2), Prof. Eddington makes an important extension of Einstein and Weyl's theories of the gravitational field. The quantities $g_{\mu\nu}$, which occur in the Riemann geometry become, according to Einstein, potentials in the gravitational field. The extension of this interpretation, due to H. Weyl, is less well known in this country, and is, in fact, very recent. We may describe the magnitude which is represented by the quantities $g_{\mu\nu}$ as the "metric," and what Weyl showed was that the metric can be made to include the four potentials of the electromagnetic field, if a certain restriction made in the Riemannian geometry—not in itself a natural type of restriction—is removed.

Prof. Eddington starts from the position that Weyl's geometry is still restricted in an unnecessary way, and that something very much more comprehensive can be found, and begins with a question. What more general can be expected, since gravitation enters the scheme of equations with Einstein, when we discard the geometry of Euclid, and then, when we adopt Weyl's, electromagnetic forces enter the scheme also? Prof. Eddington contends that the forces of non-Maxwellian type which hold an electron together must be, in all probability, the next to enter into a suitable further generalisation; but he does not contend that the present paper has necessarily done this already. But at least an important restriction, of a purely mathematical or geometrical type, now disappears from the aspect of the theory of relativity which most concerns the pure mathematician.

Weyl's theory deals with gauge-systems, or systems of unit standards of interval length, or distance in space-time, and is based on the view that any particular system can only be used for a special time and place. The gauge system of the universe is, in fact, arbitrary, much in the same sense as the axes of coordinates. But Weyl admitted a specific gauge-system of fundamental importance in the geometrical relations of the universe, about which more will be said later.

The logical implications of such assertions are at least as important to the pure mathematician as to the physicist, and this may serve as our excuse for discussing a theory already mentioned from other aspects, under other headings in SCIENCE PROGRESS. Prof. Eddington begins by the development of a geometry of tensors of an extremely general kind, which is entirely mathematical, and only proceeds later to the construction of a physical theory by identifying the tensors with quantities which we measure experimentally; the natural gauge-system provides the link which enables this to be accomplished.

The properties of tensors in relation to any transformation of co-ordinates are now generally understood. Some of those now introduced are invariant for alterations of gauge-system, and Prof. Eddington calls them in-tensors, and their simplest illustration is a displacement, say dx_μ, whose components are differences of co-ordinates (pure numbers) and not related to any gauge. These in-tensors are essentially a new mathematical development in the older Riemann geometry, with possible consequences of great interest.

The most immediate question raised by Prof. Eddington is as follows: Given an in-vector representing a displacement at any point P, can we find an exactly equivalent displacement at a point P^1 infinitely near to P? Such an equivalence must be assumed as existing, if a physical theory is ultimately to be established, for otherwise the continuum is structureless, and there are no resemblances in nature.

The points of a continuum are enumerated, in order, by any system of co-ordinates, and no change of co-ordinates changes this order, and some preliminary statement of order must be made in the very definition of a displacement, or it cannot be a mathematical expression of any physical or structural relation. Dr. Robb appears to have solved this problem of preliminary order in a work recently published, which we notice later. Prof. Eddington clearly holds the belief that the basis of truth in the quantum theory will ultimately be elucidated by the present method. Without expressing belief or disbelief in this conclusion, we may at least agree with his statement that it is probable, and that one or two sentences in what has already been postulated generally might require modification if phenomena were really discontinuous in the quantum sense.

Naturally, in an article of this nature, we cannot give a detailed consideration of the generalised geometry of tensors which Prof. Eddington develops. We can only refer readers to it, with a statement of the fact that it is quite clearly a marked advance upon the geometry of Riemann, and yet, as the author feels, not yet the final possible advance. We have

said that Prof. Eddington sees the possibility of elucidating the forces which bind the component parts of an electron together—a point raised by Sir Oliver Lodge and others in every recent discussion of the ultimate structure of matter, and a point which, as it is becoming clear, rests with the pure mathematician in the last resort—and mathematical readers of Prof. Eddington's paper cannot but feel that this at least is proved, and that the line of generalisation of Einstein's and Weyl's theories gives more hope than any *ad hoc* theories developed from the physical side. Prof. Eddington clearly has hopes that the ultimate mathematical meaning of the quantum may emerge from a yet more extensive generalisation. It is possible, but conviction is not immediate, though at least it seems more likely to emerge in this way than in any *ad hoc* manner.

The work of Riemann has perhaps never been appreciated at its true value, even by the pure mathematician, and especially on the geometrical side. The debt of Einstein to Riemann is under-estimated by all but Einstein. The small volume of Riemann's collected works has inspired an amount of later work of which it is difficult to over-estimate the importance. Prof. Eddington's paper goes far towards redressing this wrong.

A critical point of the new treatment arises in the introduction of the natural gauge of the world, which is determined by the aid of material or optical appliances which measure space and time. As Prof. Eddington points out, any such apparatus is part of the world we measure, so that we introduce the assumption that the world is self-gauging. In other words, the tensor of type $g_{\mu\nu}$ is not really extraneous and arbitrary, but is a tensor already contained in some way in the world geometry. Only one such tensor is found to be available, and it serves to define " natural length," and to clarify and make precise some of the implications of the older theory otherwise difficult to understand.

Physical space-time can be identified after the gauging equation appears, as what the author calls an *alias* of the law of gravitation. The next step is to identify " things," which have three types of attributes requiring a geometrical tensor with these properties inherent in it *by virtue of mathematical identities*. The author succeeds in identifying readily the energy, momentum, and stress in a region by an energy tensor, electromagnetic potential and force by another, and the electric charge and current vector by a third, showing their correspondence with Maxwell's equations and the law of gravitation together. These identifications appear to be unique, but are not definitely proved to be so.

A vector in general alters length on describing a circuit,

but zero length is unaltered on Weyl's theory in this manner—is, in fact, unique and requires no route of comparison. It does not remain unaltered in Prof. Eddington's theory, and he states that this was at first his chief obstacle in generalising the theory, as it seemed that the uniqueness of zero length and a unique track for light propagation were bound up together. The real relation between these two is now known.

As regards the electron, the author succeeds in proving that it is a structure which cannot exist in a resultant field of electromagnetic force, but can resist differential force of this type. An isolated electron in no external field and at rest remains a "miracle," but it is shown that an accelerated electron in an external electromagnetic field is *exactly* the same miracle, and nothing more. He concludes, further, that the ordinary mass of an electron is not directly a source of gravitational attraction, and yet it *has* gravitational mass. The electron is to be regarded as a region of abnormal world curvature, in which the gravitational mass arising is in a constant ratio to the electrical—but not of necessity the same for positive and negative electrons.

But it is not in our province to deal with these more physical aspects of the theory. Only a further remark or two needs to be addressed to the mathematical readers. Einstein's theory is not to be regarded as a mere approximation, for the discussion of the present theory proves that his postulates and deductions are exact—that, in fact, the natural geometry of the world is that of Riemann and Einstein, and not Weyl or Eddington. For what the two latter are finding is not the geometry of actual space and time, but the geometry of the world-structure which is the common basis of space and time and things. Here we quote the exact words of the author, who clearly feels that this distinction has not hitherto been really appreciated.

Dr. A. A. Robb, in his new work, *The Absolute Relations of Time and Space* (Camb. Univ. Press), gives an introduction, in concise form, to his larger treatise, and also some interesting preliminary discussion of the relation of his point of view to that of Einstein. The work is clearly very fundamental, and is not, as he states, strictly a relativity theory at all. Its relation, in one important aspect, to the theory of relativity, is brought out very clearly, and is worthy of a somewhat extended statement in these notes, for the relativity theory alone can apparently never be sufficient in itself without the introduction of a geometrical analysis of the present type, which forms a very severely logical scheme. The theory of time and space is developed in terms of the relations of "before" and "after," and the possibility of doing this is very remarkable. These relations are absolute and not dependent on the particular

observer, and have been ignored—except by Prof. Eddington—in other discussions of relativity theory. Preliminary postulates of the theory have physical interpretation, but a strict mathematical structure is built up from them, and the question raised by the relativity theory, in its recent generalised form, is as to whether the postulates of Dr. Robb *as interpreted* are correct expressions of physical fact or only approximations.

In a very valuable, though short appendix, the author gives a more analytical discussion of the relation between the two theories. Let us imagine that the relations of " before " and " after " have an interpretation which might differ from the optical one previously used by the author. On one of his inertia lines, when measurement is introduced, it is found that the formula—

$$ds^2 = dt^2 - dx^2 - dy^2 - dz^2$$

is valid, which in polars is

$$ds^2 = - dr^2 - r^2 d\theta^2 - r^2 \sin^2\theta \, d\phi^2 + dt^2$$

A modified interval measure may now be introduced which changes ds^2 into ds_1^2 where

$$ds_1^2 = ds^2 \left\{ 1 - \frac{2m}{r - 2m}\left(\frac{dr}{ds}\right)^2 - \frac{2m}{r^2}\left(\frac{dt}{ds}\right)^2 \right\}$$

and from this follows Schwarzschild's formula belonging to the region round a single spherical body according to Einstein's gravitation theory. The author compares this with the brachistochrone problem, where the modified interval measure is the time taken to traverse it.

The author then shows quite simply that all the difficult geometric systems involved are constructed easily from his relations of before and after, by the use of a modified interval measure. These relations appear, in fact, though the author gives as yet no complete proof, to be in some sense true with or without the presence of matter, but that the postulates of the theory can only be interpreted strictly in the optical manner when appreciable quantities of matter are present. The essential thing is the four-dimensional manifold, and the geometries become analytical developments which are suitable for various special purposes. The author is clearly opposed to the conception of " curvature of space," which, from this standpoint, is not an implication involved in the use of any such geometries.

One very fundamental feature of the logical scheme developed by Dr. Robb is that the idea of " congruence " is an essential and intrinsic part of the system, appearing in a natural manner, and not as something brought in from outside to a

system otherwise complete. The conception of "conical order," on which Dr. Robb founds his geometrical scheme, is already familiar from his other treatises, and it is not necessary that we should expound it here. But in the present small work much that is new and suggestive appears, and it would seem to be an essential part of the equipment of every geometer of the future who is interested in the foundations of his subject. The author promises us, in the near future, a further generalisation of his work, which he has already carried out to a certain extent, though not published as yet. It was a common opinion recently that relativity theory had, on the mathematical side, gone as far as it could, and that its future lay with the physicist. But works such as these by Prof. Eddington and Dr. Robb clearly prove the contrary, as also does a long paper by Sir Joseph Larmor, given to the International Congress of Mathematicians at Strasburg last year, and now printed. We propose to notice this in detail, together with others promised by their authors in the near future, in subsequent articles.

ASTRONOMY. By H. SPENCER-JONES, M.A., B.Sc., Royal Observatory, Greenwich.

Photographic Action and Astronomical Measurement.—Photographic methods are to-day of great and increasing importance in astronomical observation. For the determination of stellar parallaxes and of the wave-lengths of lines in the spectra of celestial bodies they have almost entirely displaced visual methods, whilst for the determination of stellar magnitudes and positions they possess many advantages and are largely used. But in many problems precautions have to be taken to prevent the introduction of systematic errors, which may arise from the complicated nature of the laws of photographic action. Thus, for example, doubling the exposure on a given source does not result in a doubling of the photographic sensation. For the determination of stellar magnitudes it is not, therefore, permissible to compare the exposure-times required to produce images of a certain density or size. Detailed investigation of the mechanism of photographic action and of the resulting phenomena is in consequence desirable. Investigation on these lines is in course of progress at the Research Laboratory of the Eastman Kodak Company, and several papers dealing with various phenomena have been published recently by F. E. Ross in the *Astrophysical Journal.*

"Photographic Photometry and the Purkinje Effect" (*Astrophysical Journal*, **52**, 86, 1920) deals with the photographic analogue of the Purkinje Effect. It has long been known that if two sources of different colour produce equal visual sensations, then if the intensities of both sources are

increased or decreased in the same ratio, the resulting visual sensations are no longer equal. Two intensities are considered to produce equal photographic sensations when they produce equal blackening or density in the same time or, in the case of stars, discs of equal size. If the photographic sensations are equal, it is assumed that the two intensities are equal. But will the sensations remain equal if both intensities are increased or decreased in the same ratio or if the common exposure-time is increased or decreased ? In the first paper, the effect of a difference in colour or wave-length of the two sources is investigated.

In the study of this problem, it is the contrast phenomena of the photographic plate which are of importance. The contrast, technically termed *gamma* (γ), is defined as the rate of growth of density with logarithmic increase of exposure-time. In astronomical applications the rate of increase in the diameter of a stellar image with logarithmic increase of exposure-time (termed *astrogamma* [Γ]), is more often used. Both constants for a given type of plate depend somewhat upon developer and degree of development. For similar conditions as regard development, they depend further upon the emulsion and upon the wave-length of the incident light.

If for a given emulsion the densities are plotted as ordinates against the logarithms of the exposures as abscissæ, a " characteristic curve " of the emulsion is obtained, and such curves may be constructed for different wave-lengths. In general the characteristic curve consists, first of a " toe " in which, near the threshold sensitivity of the plate, the rate of increase in density with time is small; then, after a certain density, the slope of the curve increases and remains nearly constant through a wide range of exposure-time; finally, for exposures greater than a certain value, the slope decreases and finally is reversed, denoting a stage in which increase in exposure results in decrease in intensity. The slope of the straight portion of the curve gives the value of the contrast (γ) for normal exposures. If the contrast is plotted against wave-length, the curve obtained depends upon the emulsion. The contrast increases with increase of wave-length in the blue region of the spectrum, but after a certain wave-length the rate of increase in general becomes smaller, and for some emulsions is then followed by a decrease. The behaviour of astrogamma is generally similar. If, then, a blue star and a red star are found to give the same photographic sensation for a given exposure-time and the exposure-time is increased, the resulting sensations will not in general be equal, owing to the dependence of γ and Γ upon the wave-length. Also two other stars, blue and red, whose magnitudes are greater or less by the same amount

than the blue and red stars just considered, will not in general produce equal sensations. For ordinary emulsions, Ross finds that if the photographic sensations are equal for a certain exposure-time for stars of types A and K, then a tenfold increase in exposure will result in a magnitude difference of 0·26, the red star being estimated the fainter.

To avoid such errors, it is desirable to use the horizontal portion of the gamma wave-length curve, and this is possible by matching each emulsion with a suitable filter. It is further found that orthochromatic or panchromatic plates are more suitable for accurate photometry than are ordinary or blue-sensitive plates.

The cause of this behaviour of the photographic plate has been investigated by Ross. He finds that it is to be attributed partly to a difference in the penetration of radiations of different wave-lengths into the film. The penetration in the case of blue light is much less than for light of longer wave-length, and, other things being equal, the contrast is directly proportional to the penetration. The low penetration for short wave-lengths is due to absorption and scattering in the emulsion.

The second paper of the series is entitled, "Image Contraction and Distortion on Photographic Plates" (*ibid.*, **52**, 98, 1920). If image-diameter is plotted against the logarithm of the exposure-time, the shape of the resulting curve depends upon the size of the geometrical or threshold image of the object. For short exposures, the diameter has its geometrical value, but following this induction period there is a lag or even a drop in diameter before the normal growth of the image with exposure ensues. The point of maximum lag occurs with a diameter of the threshold image of about 5 mm. It follows that there exists a contracting influence which at first prevents the normal growth of the image.

This phenomenon, on investigation, was found to be due to a contraction of the photographic image during drying. A test-object, consisting of a large circular hole in a piece of cardboard surrounded by a series of smaller holes, was photographed, and the photographs after development were measured both wet and dry. A plug could be inserted in the central hole, so that exposures could be made with or without the central image, and the influence of the latter on the position of the adjacent images determined. In this way it was found that, with the large central image absent, the measures with the plate wet or dry agreed, but that, with the central image present, there may be a contraction of the central image during drying accompanied by a drawing together of the star images near its edge, the latter effect decreasing with increase in distance from the edge of the central image. The magnitude

of this effect depends upon the developer, being greatest with pyro-metol. Examination of sections of images developed with different developers shows that these effects are produced by strains in the gelatine resulting from unequal drying. The developed image has a much smaller water content than that of the surrounding gelatine, and therefore dries more rapidly. This gives rise to stresses, parallel to the supporting plate, which are greatest near the edge of the image. The stresses cause the edge to move inwards, dragging along the surrounding gelatine by an amount which decreases approximately exponentially with the distance from the edge. This accounts for the translatory motion of the star images. The amount of the contraction depends upon the size of the image, its density, and upon the developer.

This phenomenon is of importance in photographic measures of close double stars. The contraction will cause the measured distance to be too small. The same is true of measurements of close spectral lines, and of the relative positions of stars in dense clusters. For such purposes, therefore, developers such as caustic hydroquinone and pyro should not be used, but preferably metol- or chlor-hydroquinone, with which very little contraction results.

A third paper deals with " Photographic Sharpness and Resolving Power " (*ibid.*, **52**, 201, 1920), and investigates the conditions required to obtain sharp images and high resolving power. The two important quantities for determining the sharpness are γ and Γ, defined above. Γ is the rate of increase of diameter of image with logarithmic exposure, and may be used as a measure of the " turbidity " of the emulsion. If the turbidity is high, the photographic image will rapidly extend beyond the limits of the geometrical image; γ provides a measure of contrast, defining the rate of increase of density. It is found that the decrease of intensity at the edge of an image follows an exponential law, with a coefficient which is inversely proportional to the turbidity, whilst the " sharpness," which may be defined as the rate of change of density with distance from the geometrical edge of the image, is equal to the ratio of the contrast to the turbidity. Practical investigation, based upon the study of enlargements of knife-edge contact prints exposed to light of varying wave-length, confirms this conclusion in a general way, although actually the phenomena are complicated somewhat by secondary actions. The result indicates, however, that in order to secure sharp images, emulsions should be chosen which have a high contrast and a low turbidity. It is also important that the characteristic curve of the emulsion should have a minimum amount of " toe," so that the slope (which defines the contrast γ) should

reach its maximum value at a comparatively low density. Fine-grain plates have usually high contrast and low turbidity, and therefore give sharp images; the sharpness, however, is not due primarily to the fineness of grain, as is commonly supposed. The sharpness arising from a random distribution of grains is of a higher order than occurs in real emulsions.

The resolving power of a photographic plate, on the other hand, depends directly upon the size of grain. Ross finds that the resolving power is proportional to $S\,(d^2 + a^2)^{-\frac{1}{2}}$, where S is the sharpness, d is a measure of the size of grain and a is a constant. This formula may be illustrated by the case of an average emulsion with chemical and physical development respectively. The turbidity factor in the two cases is the same, but the contrast factor is much larger for chemical than for physical development. The sharpness under physical development is therefore much less, as can be seen by eye. Nevertheless, the resolving power is greater under physical development, owing to the exceedingly fine grains which result. The resolving power varies with the wave-length, having in general a minimum value in the green region.

In astronomical work allowance has to be made for the turbidity arising from the optical system, due to the image of a point source not being a geometrical point. The optical turbidity in general overshadows the inherent turbidity of the emulsion, so that then the determining factor is the contrast factor of the photographic plate. Plates of high contrast will give maximum sharpness, and since γ is low in the violet but increases with the wave-length, isochromatic plates used with a yellow filter will give increased sharpness and resolving power. Yellow-dyed plates, on the other hand, are not suitable for astronomical work; they have low turbidity, the advantage of which is lost on account of the optical turbidity, and they have low contrast, resulting in poor resolving power and sharpness.

Ross queries whether astronomers are right in asking for a high-speed plate of fine grain. He states that the resolving power of modern plates is far ahead of that which can be obtained with the largest telescopes. An increase in speed of the plates will increase the resolution obtained with a large telescope; but it is doubtful whether an increase in the resolving power of the plate will do so. Provided that increased speed can be obtained without loss of the resolving power which the plates now have, he claims that it is more profitable for astronomical photography to work in the direction of attaining higher speed than attaining better resolving power.

"The Mutual Action of Adjacent Photographic Images" is dealt with in a fourth paper (*ibid.*, **53**, 349, 1921). The

effect of distortions in the gelatine during drying was considered in the second paper, but there are other causes which may affect the relative positions of adjacent images. The study of these is of utmost importance in the modern applications of photography to the determination of the relative positions of double stars and satellites, and to the measurements of adjacent spectral lines. The results obtained from previous investigations have been somewhat contradictory, at some times apparent attractions and at other times apparent repulsions having been indicated. The explanation of these discrepancies is to be found in the complex nature of the phenomena. There are three distinct phenomena concerned. (i) Owing to the turbidity of the emulsion there is a mutual light action in the case of two adjacent images. For two equal circular sources the outlines of neighbouring images, as the exposure is gradually increased, are a series of lemniscates which for sufficiently small exposures consist of two distinct ovals. The centres of these ovals are displaced towards each other relatively to the centres of the geometrical images, and the displacement increases with the exposure. Therefore, on this account, double stars and close spectral lines are subject to an apparent attraction whose amount depends upon the separation of the adjacent borders of the two images. This effect is negligible except for very small images, which are very close together. (ii) Actual displacements of the gelatine: it has been seen that these produce an apparent attraction between images, which can be brought into evidence by measuring the plate when wet and when dry. Where it is possible that such effects may enter and become of importance, they can be almost completely avoided by suitable choice of developer. (iii) The third cause of apparent displacement is a developer effect. During the process of development, reaction products are formed which have a restraining action on the development. Now consider a pair of adjacent images: as the development proceeds these reaction products are formed around both images and gradually diffuse to adjacent regions. In the space between the images these products enter from both sides, and their concentration is therefore greater than on the far sides of the images. The development at the adjacent edges is therefore retarded, with the result that there is an apparent repulsion of the centres of the images. This effect is present in varying degree with all developers, but is greatest with pyro and least with metol-hydroquinone. It increases with the time of exposure.

The net result of the three effects is that with over-exposed images there is repulsion, due to the large developer-effect, whilst with normal exposures a slight attraction is probable but the errors should not exceed two or three microns.

In the case of absorption lines the phenomena are slightly different. Both the gelatine effect and the developer effect then cause a repulsion ; the turbidity effect causes an attraction, and there is a further attraction due to a difference in the sharpness of the outer and inner edges caused by turbidity and halation. In general, the total effect is an attraction, about one micron for normal exposures, but larger for over-exposures.

The investigations of Ross on the mutual action of adjacent photographic images have resulted in the suggestion that the displacements of star images on the recent eclipse plates, which have been held to confirm Einstein's prediction of the deflection of light by a gravitational field, may have been in reality a photographic effect of a similar nature. Under the conditions of the eclipse plates, Ross's results would not indicate any distortion of the amount required; but the matter has been finally disposed of by a paper read by F. Slocum at the last meeting of the American Astronomical Society. Two pairs of photographs of the Pleiades were obtained, and one plate of each pair was exposed to a circle of light, so that an image 31 mm. in diameter and somewhat denser than the corona was printed on it. One pair of plates was developed with pyro, the other with hydroquinone. Fourteen faint stars, symmetrically distributed around the central image, were measured and the coefficient of distortion, which was assumed to vary inversely with the radial distance from the disc, was determined. The values in each case were small and of the order of their probable errors, and were only a few per cent. of the coefficient of distortion, which would be necessary to account on this hypothesis for the measured displacements on the eclipse plates. The photographic distortion in the case of the eclipse plates is therefore negligible.

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PHYSICS. By JAMES RICE, M.A., University, Liverpool.

Weyl's Generalisation of the Einstein Theory.—In a former article in SCIENCE PROGRESS (vol. xiv, p. 557), the writer endeavoured to give a brief and elementary exposition of Einstein's General Relativity Theory. It was emphasised that for the mathematical treatment the introduction of the concept of the "separation" between "two events" was of prime importance. In his earlier or "restricted" theory, Einstein had adopted $\sqrt{(\delta x_4^2 - \delta x_1^2 - \delta x_2^2 - \delta x_3^2)}$ as the value of the separation between two events whose co-ordinates in a definite frame of reference are x_1, x_2, x_3, x_4 and $x_1 + \delta x_1$, etc., respectively. It is implied that the space co-ordinates x_1, x_2, x_3 are Cartesian. The justification for the use of this measure lies in the fact that if the co-ordinate system be altered to another which is in *uniform* relative motion to the first, then $\sqrt{(\delta x_4^2 - \delta x_1^2 - \delta x_2^2 - \delta x_3^2)} = \sqrt{(\delta x_4'^2 - \delta x_1'^2 - \delta x_2'^2 - \delta x_3'^2)}$ of the Lorentz transformation equations are employed. That is, the expression in question is *invariant*. This invariance, in fact, takes the place of the assumed individual invariance of $\sqrt{(\delta x_1^2 + \delta x_2^2 + \delta x_3^2)}$

and δx_4 in pre-Relativity Theory, an assumption now dropped. This conception enabled Einstein to generalise dynamical theory, so as to cover the relativity of mass and force to the frame of reference of the observer. Of course, if one chooses to employ a frame of reference in rotational or accelerated motion relative to any of those dealt with, the expression for an element of separation becomes the root of a more general quadratic expression in δx, viz. $\Sigma g_{\alpha\beta}\, dx_\alpha\, dx_\beta$. (In this α and β are summed independently over 1, 2, 3, 4, and there are apparently sixteen terms; these, however, compress to ten by reason of the equalities $g_{\alpha\beta} = g_{\beta\alpha}$. The g-coefficients are, in general, functions of the co-ordinates depending on the relations of the new frame of reference to the old.) This change in the mathematical form of the expression for an element of separation is paralleled physically by the appearance in the new frame of mechanical effects absolutely similar to the effects of introducing a field of gravitation. Einstein's Principle of Equivalence assumed that the similarity extended to all physical processes, in particular the propagation of light. Of course such gravitational fields were purely "fictitious" or "geometrical." The mathematical procedure in choosing such a frame is similar to that employed in two-dimensional geometry, when, for one reason or another, we prefer to use polar, elliptical, or some other form of curvilinear co-ordinates instead of Cartesian. It is always possible to revert to the simpler procedure if necessary. Now, in two-dimensional geometry, the possibility of using Cartesian co-ordinates depends on the axioms we start from and the nature of the surface treated. In surfaces which can be developed into a plane (such as the cylinder or cone), co-ordinate systems can (if Euclid's axioms are excepted) be laid down which permit of the distance between two neighbouring points being expressed in the simpler form; but this is not so on surfaces which cannot be so developed (sphere, ellipsoid, etc.). No co-ordinate system based on a duplicate meshwork of curves on the surface can be found which permits distance to be expressed as $\sqrt{(\delta x_1^2 + \delta x_2^2)}$ *everywhere* on the surface. (It is possible in the neighbourhood of an assigned point.) Riemann had already suggested that similar considerations might actually be true of the three-dimensional manifold consisting of the points of an observer's space. Einstein seized this suggestion and applied it to the geometry of the "World," the four-dimensional manifold consisting of the "events" in an observer's space-time frame of reference. His *geometrical* assumption was that the ten g-coefficients could not in general be transformed by any possible change of the co-ordinates to the simple values $g_{11} = g_{22} = g_{33} = -1$, $g_{44} = +1$, and the rest zero (except for a minute region of space-time neighbouring to an assigned event). His *physical*

assumption was that the values of these g- " potentials " were fundamentally determined by the gravitational properties of the matter in the universe. Of course, if we change our frames of reference, a mathematical transformation is involved which alters the values of the g-coefficients ; but that is natural, for in the new frame we have a new (fictitious) gravitational field imposed on the former one. The essential point is that, at events not distantly removed from all matter, it is impossible to choose co-ordinates so as to transform away all the differences between the g-coefficients and the simple (Lorentz) values.

On a surface there are natural " tracks " for a particle between two points, viz. the " geodesics," or curves for which

$\sqrt{(g_{11}\ dx_1^2 + 2\ g_{12}\ dx_1\ dx_2 + g_{22}\ dx_2^2)}$ between the two points

is " stationary " (maximum or minimum). Einstein took it for granted that the track of the " event," consisting of a moving particle, in space-time could be determined by a similar law. $\int\sqrt{\left(\sum_1^4 g_{\alpha\beta}\ dx_\alpha\ dx_\beta\right)}$ between two given point-instants would be stationary for it. The actual orbit of the particle in any observer's space would then be the projection on his space of this " world-line," or " world-geodesic." The solution of problems in orbits would then become a matter of Differential Geometry of four dimensions, provided the value of the g-potentials were known. As a guide to the discovery of these, Einstein submitted a certain set of differential equations, known as his law of gravitation. We can briefly summarise this by introducing certain symbols, as follows :

The determinant

$$\begin{vmatrix} g_{11} & g_{12} & g_{13} & g_{14} \\ g_{21} & g_{22} & g_{23} & g_{24} \\ g_{31} & g_{32} & g_{33} & g_{34} \\ g_{41} & g_{42} & g_{43} & g_{44} \end{vmatrix}$$

is called g ; and the co-factor of any constituent $g_{\lambda\mu}$ of the determinant when divided by g is called $g^{\lambda\mu}$.

$$[\lambda\mu,\ \nu] = \tfrac{1}{2}\ (\delta g_{\lambda\nu}/\delta x_\mu + \delta g_{\mu\nu}/\delta x_\lambda - \delta g_{\lambda\mu}/\delta x_\nu)$$

$$\{\lambda\mu,\ \nu\} = \sum_{a=1}^{4} g^{\nu a}\ [\lambda\mu,\ a].$$

Now Riemann had proved that in the geometry of any manifold there were certain conditions that the manifold should be " flat," or " homeloidal," i.e. that co-ordinate systems analogous to Cartesian should be possible for it. They were the following differential equations :

$$\partial/\partial x_\mu \{\lambda\nu, \kappa\} - \partial/\partial x_\nu \{\lambda\mu, \kappa\} + \sum_{a=1}^{4} \{\lambda\nu, a\}\{a\mu, \kappa\} - \sum_{a=1}^{4} \{\lambda\mu, a\}\ \{a\nu, \kappa = 0\ (1)$$

There are apparently 256 of these when we give κ, λ, μ, ν the

values 1, 2, 3, 4 independently; but they actually reduce to 20 conditions. Einstein recognised that space-time is not "flat," i.e. the transformation to quasi-Cartesian co-ordinates is not in general possible, so he proposed as a guide to the determination of the g-coefficients that *outside matter* they should satisfy the differential equations

$$\sum_{\epsilon=1}^{4} B_{\lambda\mu\epsilon}^{\epsilon} = 0 \quad (2)$$

where $B_{\lambda\mu\nu}^{\epsilon}$ is a symbol employed to denote the left-hand side of the equation (1). Equations (2) are clearly less stringent than (1). The values of the g-coefficients corresponding to "fictitious" fields, satisfying (1) as they do, would, of course, also satisfy (2); but (2) would also include possibilities wider than those admitted by (1); and Einstein's assumption is that these correspond to "real" fields which cannot be resumed under (1). Equations (2) are generally written in the form

$$G_{\lambda\mu} = 0. \qquad (2a).$$

On account of the equality of $G_{\lambda\mu}$ and $G_{\mu\lambda}$ there are ten of them. For points *within matter*, Einstein has proposed the equations

$$G_{\lambda\mu} = U_{\lambda\mu} \qquad (3),$$

where $U_{\lambda\mu}$ are another set of ten quantities calculated from the density of the matter, its momentum and energy. Owing to the form of the ten expressions $G_{\lambda\mu}$ it can be proved that they satisfy four conditions, and, when they are replaced by $U_{\lambda\mu}$ in these conditions, four equations are obtained which are simply the laws of conservation of energy and momentum. Thus Einstein's complete Law of Gravitation, (3) (viz. the equality of the "gravitational tensor" and the "matter-tensor") are really a set of *dynamical equations* in which gravitational effects are inherent and not imposed by external "forces." Another important feature is that if we replace x_1, x_2, x_3, x_4 by any four functions of these in

$$ds^2 = \sum_{1}^{4} g_{\alpha\beta}\, dx_\alpha\, dx_\beta,$$

so that we obtain a new set of g-coefficients; and if at the same time we make the necessary changes in the energies and momenta in $U_{\lambda\mu}$ corresponding to a new frame of reference, the new g-coefficients still satisfy equation (3); so the principle of General Relativity is quite satisfied.

This brief résumé may serve to show what is meant by the statement that Relativity has led to the "geometrisation" of physics. But there is one point lacking in the complete geometrisation. We can illustrate it by the example of the

geodesic world-line of a particle. If the particle is subject to mechanical pressures and pulls, or to electro-magnetic forces, the path in a space-frame no longer corresponds to the geodesic world-line. If we accept the very prevalent belief that all mechanical forces, "cohesions," "affinities," etc., are at bottom electro-magnetic, we see that a great step forward in the geometrising process would be effected if we could connect the Maxwell equations of the electro-magnetic field with the metrics of the world, as clearly as Einstein's equations of the gravitational field are connected. This has been attempted by Hermann Weyl of Zürich. His original papers are published in the *Sitzungberichte d. Preussische Akademie* (1918) and in the *Annalen der Physik*, Band 59, pp. 101–133 (1919). A very full account of his theory is contained also in his book *Raum, Zeit, Materie* (Springer, Berlin). English sources, of as "popular" type as is possible, are Eddington's *Space, Time, and Gravitation* (C.U.P.) and the new edition of Cunningham's *Relativity and the Electron Theory* (Longmans).

The essence of Weyl's idea is this : The laying down of a co-ordinate system does not involve *measurement* at bottom, but only *numbering* according to an ordered and consistent scheme; but the statement that such and such a function of co-ordinate differences *measures* a distance, an interval of time, a separation between two events, obviously does, and implies the use of some *gauge* (a definite stick, e.g. for length, and the interval required for light in gravitation-free space to travel along it for time, and a "blend" of these [a "clock-ruler"] for separation). But how are different observers to know that they are using the "same gauge"? Only by direct comparison. This implies transportation of a gauge, or a strict copy of it, from one place in space-time to another for comparison with a gauge used there. But will the path by which it is transported have any effect on it? Weyl asks us to face the possibility that it does, to free ourselves from the *restricting* assumption that the path has no effect, to accept as conceivable that, e.g. a ruler on being carried from A to B by a certain path might be perfectly congruent with a ruler stationed at B, and yet might not be so if transported by another path; or, to put it still more strikingly, that a strict copy of a ruler at A might on moving to B and returning to A *by a different path* not fit true to its original. To give analytical expression to this idea we assume that the fraction by which the gauge alters in a displacement from a point-instant x_1, x_2, x_3, x_4 to a neighbouring one $x_1 + \delta x_1$, etc., is

$$\phi_1 \, \delta x_1 + \phi_2 \, \delta x_2 + \phi_3 \, \delta x_3 + \phi_4 \, \delta x_4$$

where $\phi_1, \phi_2, \phi_3, \phi_4$ are four functions of the co-ordinates.

Relativity has now to cover change of gauge as well as change of co-ordinates, i.e. laws of nature in the form of our differential equations of physics have to be invariant not only for arbitrary transformation of co-ordinates but also for arbitrary variation of gauges. The geometry of the world will now involve the four gauge-coefficients ϕ_α, as well as the ten frame-coefficients $g_{\alpha\beta}$. It is a much more complicated geometry, as anyone reading Weyl's work discovers to his cost. But, parallel to Einstein's physical assumption regarding the geometrical quantities $g_{\alpha\beta}$, Weyl has a *physical* assumption for his ϕ_α; they determine the electro-magnetic field; they are, in fact, the three vector-potential components and the scalar potential of the field in Maxwell's theory. This assumption serves to bring Weyl's generalised Riemann geometry into relation with physical occurrences, in a fashion similar to that by which Einstein brought Riemann's geometry into such relation. The whole attractiveness of the theory rests at present on its mathematical elegance and the feeling of pushing a particular method of investigation to still further bounds. No experimental tests, such as those which contributed so signally to the success of Einstein's own theory, seem available at the moment; although it is not impossible that the theory may one day have some statements to make concerning the size of our universe on the one hand, and the atomic nature of electricity on the other, which might conceivably be investigated experimentally. There is a possibility also that it may help to clear up the vexed question of the Einstein displacement of the solar spectral lines.

PHYSICAL CHEMISTRY. By W. E. Garner, M.Sc., University College, London.

Monomolecular Reactions.—Daniels and Johnston (*J.A.C.S.*, 1921, **43**, 53 and 72) have shown that the decomposition of nitrogen pentoxide is a monomolecular reaction, which takes place at room temperatures. It differs from the much discussed gaseous monomolecular reaction, the decomposition of phosphine, in that it is not catalysed by the walls of the reaction vessel. It is thus of considerable importance for the theory of chemical kinetics. The "critical increment," or heat of activation, is practically constant over the temperature range of 12·5 to 65° C., and this is in agreement with the Arrhenius' relation. The wave-length of the activating energy, calculated in accordance with the theories of W. C. McC. Lewis and Perrin, gives the value 1·16 μ. Light of this wave-length is, however, without action on N_2O_5; a result which is contrary to the Lewis–Perrin theory but not in disagreement with the treatment proposed by Tolman (*J.A.C.S.*, 1920, **42**, 2506).

The frequency of the theoretical absorption line is exactly five times as great as the most intense absorption line of N_2O_5.

Blue light, 400 $\mu\mu$ to 460 $\mu\mu$, accelerates the decomposition of N_2O_5, but only in the presence of N_2O_4, which acts as an autocatalyst. Since the adsorption lines of N_2O_4 and N_2O_5 overlap in the infra-red, the authors suggest that N_2O_4 adsorbs blue light, and emits light in the infra-red region, which causes the photochemical decomposition of N_2O_5. Baly and Barker (*Trans. Chem. Soc.*, 1921, **119**, 662), however, consider that the absorption by the pentoxide of energy, radiated by the peroxide, cannot take place on the quantum theory, unless the frequencies possessed by the two are exactly equal.

The photochemical decomposition of nitric acid (Reynolds and Taylor, *Trans. Chem. Soc.*, 1912, **101**, 131), which only occurs in the gaseous phase, is inhibited by the presence of concentrated sulphuric acid. The autocatalyst, nitrogen peroxide, is in this case removed from the system as soon as it is formed.

Tolman (*J.A.C.S.*, 1921, **43**, 269) points out that Dushman's equation for the velocity constant of a monomolecular reaction,

$$(1) \qquad k = \frac{Q_A}{Nh} . e^{-\frac{Q_A}{RT}}$$

$$\text{or } (2) \qquad \log k = 10{\cdot}0203 + \log Q_A - \frac{Q_A}{4{\cdot}57T}$$

(where Q_A denotes the heat of activation, N and R possess the usual significance, and h is the quantum constant) is the only equation which fits the results for the rate of decomposition of nitrogen pentoxide. This equation has been derived from

$$k = se^{-Q/RT}$$

which represents successfully the dependence of reaction velocity on temperature. Dushman, in agreement with Lewis and Perrin, assumes that $Q = Nhv$.

He also assumes that s, which possesses the dimensions of a frequency, is the same as v already introduced. This equation (Dushman, *J.A.C.S.*, 1921, **43**, 397) gives values for Q_A in substantial agreement with the experimental results. It has been applied, not only to the decomposition of N_2O_5 and PH_3, but also to those chemical reactions where one of the opposing reactions is monomolecular. Thus for reactions of the type

$$A_2 \rightleftharpoons 2A$$
$$\text{and } A.B \rightleftharpoons A + B$$

the velocity of the monomolecular reaction is calculated from

equation (2), and the velocity of the back reaction from the collision-frequency method developed by Trautz and others.

Equation (2) bears some resemblance to the approximate Nernst relation,

$$\log K_p = -\frac{Q}{4{\cdot}57T} + \Sigma n\ 1{\cdot}75 \log T + \Sigma nC$$

The results of the application of the theory to many chemical reactions indicate that the equation is only approximately true, and that there are other factors (steric) yet to be taken into consideration. The nature of the success that has been achieved by the application of the quantum theory in this field suggests that the problem has been partly solved.

Photochemistry.—Coehn and Tramm (*Ber.*, 1921, **54**, 1148) have investigated the photochemical dissociation of carbon monoxide. The equilibrium position of this reaction is displaced by exposure to ultra-violet light. The necessity for the presence of minimal quantities of moisture for the explosion of carbon monoxide and oxygen was recognised by Dixon, and it would be anticipated that water would be necessary for the photochemical reaction. This is not the case neither for the forward nor the back reaction. In fact, CO_2 is not even decomposed by light unless it is strongly dried. Carbon monoxide and oxygen, on the other hand, not only react when so dry that they will not burn, but combine at a rate independent of the moisture content. Coehn gives no explanation of this anomalous behaviour.

Baly and Barker (*Trans. Chem. Soc.*, 1921, **119**, 653) have studied the photochemical reaction between hydrogen and chlorine, from the point of view of the Einstein law of photochemical equivalence. They find that the deviation from the law increases rapidly with increase in the intensity of the activating light. The observation of Bunsen and Roscoe, that the mixture of hydrogen and chlorine remains activated for many minutes after removal of the activating light, has been confirmed. The peculiar expansion of the gaseous mixture observed on exposure to light ($400\,\mu\mu - 270\,\mu\mu$) is proportional to the rate of reaction. The results obtained are in agreement with the observations of Slade and Higson (*Proc. Roy. Soc.*, 1920, [A], **98**, 154) in their experiments with the photographic plate, who find that the amount of silver obtained is not proportional to the intensity of the light. The divergence from Einstein's law is due to the readsorption, by the unchanged hydrogen and chlorine molecules, of the energy radiated by the combining molecules. The authors suggest a new type of photocatalysis, whereby a photochemical reaction may be brought about by light, which is not absorbed by the reactants

but only by the photocatalyst. The photocatalyst must contain the same elementary atoms as the reactants and thus have common frequencies in the infra-red with the reactants.

Alkali Metals in Liquid Ammonia.—Kraus (*J.A.C.S.*, 1921, **43**, 749) has continued his investigations into the nature of the conduction processes occurring in solutions of the alkali metals in ammonia. The alkali metals do not form compounds in ammonia, and since the energy changes accompanying the solution are inconsiderable there does not appear to be very much complex formation. The electrical conductivity gives a minimum value at about 0·05 N, rising in the case of the stronger solutions to very high values. The nature of the ions carrying the electric current varies with the strength of the solution. In dilute solutions alkali metal is deposited on the cathode, but the cation only possesses about one-seventh of the speed of the anion. In the neighbourhood of N solutions, however, the speed of the anion is some hundred times greater than that of the cation. The anion is the same for all of the alkali metals. Solution of metals in ammonia forms a connecting link between electrolytic and metallic conduction. The cation is undoubtedly the metal, and the negative ion, being sub-atomic, is probably the electron. In dilute solutions the electron is probably associated with ammonia molecules, and hence its speed is comparable with that of the cation, but in concentrated solutions the complexes break down and the conductivity becomes metallic.

The Mass Effect in the Entropy of Solids and Gases.—Sackur first showed that the entropy of a perfect monomolecular gas is given by the relation

$$S = \tfrac{3}{2}R\,ln\,m + \tfrac{5}{2}R\,ln\,T - R\,ln\,P + So$$

where So is a constant independent of the substance and m is the molecular weight. Latimer (*J.A.C.S.*, 1921, **43**, 819) proposes a simpler formula for the entropy of elements in the solid state, which holds when its specific heat has reached the Dulong and Petit value of 6 per gram atom. Thus:

$$S_{298} = \tfrac{3}{2}R\,ln\,\text{at. wt.} + S'o$$

holds where $S'o = -0{\cdot}94$. This value is calculated from the value of $S_{298} = 297{\cdot}0$ for KCl. Assuming that the entropy of a compound is given by the sum of the entropies of the elements, values are obtained agreeing fairly well with those from specific heat and thermochemical data. An equation is also proposed for the mass effect in diatomic gases. Tolman (*J.A.C.S.*, 1921, **43**, 866) brings forward the objection to this formula that it is not in agreement with the theory of similitude.

Colour Changes in Disperse Systems.—Weigert (*Kolloid Zeit.*,

1921, **28**, 115) and Weigert and Pohle (*Kolloid Zeit.*, 1921, **28**, 153) have made important contributions to the study of the changes in disperse systems under the influence of coloured light. It is well known that a silver chloride gelatine film, which has been previously exposed to light and thoroughly washed with water, changes in colour when exposed to coloured light. The colour of the film is changed to a more reddish, greenish, or bluish tint after exposure to red, green, or blue light respectively. The extinction ($E = \log Io/I$) of the film for a particular wave-length of light is decreased by exposure to that wave-length and increased by exposure to any other wave-length. Plane polarised light so modifies the silver gelatine film that the extinction to coloured plane polarised light varies with the direction of the plane of polarisation. The film, which can thus be said to become dichroic, at the same time becomes double-refracting. Both effects are produced best by light of long wave-length. A chemical explanation of the phenomenon does not appear to be possible, since the number and amount of the silver particles are unchanged by illumination. An examination of a liquid emulsion by the ultramicroscope showed that the ultramicrons had little influence on the phenomenon. The coloured dichroic ultramicrons observed by Siedentopf in silver chloride could be produced by exposure of the colourless ultramicrons to coloured light. The colour of the particles was independent of the exciting light, and contrary to the production of dichroic and double-refracting films light of short wave-length was most effective. Weigert comes to the conclusion that large numbers of amicrons, which cannot be resolved by the ultramicroscope, are in some manner responsible for the colour changes which take place in the silver gelatine film.

Friend (*Trans. Chem. Soc.*, 1921, **119**, 932) has continued his work on the corrosion of iron. Iron corrodes less rapidly in a very rapid stream of water than in a slow stream. The author explains this behaviour on the assumption that ferric hydroxide catalyses the corrosion of iron, and that this substance would be removed by a rapid stream of water. He finds that the influence of dissolved electrolyte and protection of ferric hydroxide by colloids is in agreement with the theory.

Hahn (*Ber.*, 1921, **54**, 1131) claims to have discovered a new radio-active substance in ordinary uranium salt with the chemical properties of proto-actinium. It evolves β rays and possesses a half life period of 6·7 hours.

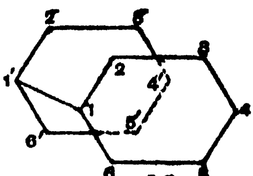
Aston (*Phil. Mag.*, 1921, **42**, 141) shows that iodine is a simple element with an atomic weight of 127. This is contrary to the theories of Broek and others. Kohlweiler has deduced that five iodines occur, and claims to have partially separated these by diffusion.

Baxter and Parsons (*J.A.C.S.*, 1920, **43**, 507) show that meteoric has the same atomic weight as terrestrial nickel.

ORGANIC CHEMISTRY. By O. L. Brady, D.Sc., F.I.C., University College, London.

New Types of Isomerism.—The preparation and investigation of an increasingly large number of organic compounds is bringing to light types of isomerism which cannot be explained by means of the usual structural formulæ.

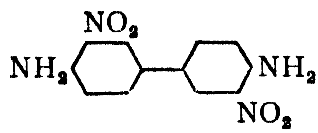
As early as 1912 Cain and his co-workers showed that the dinitrobenzidine prepared by Bandrowski by the nitration of diphthalyl-benzidine was not identical with that of Strakosch, obtained by the nitration of diacetyl-benzidine, although both contain the nitro-groups ortho to the amino-groups. The orientation of these two compounds has been established by their oxidation to 3 nitro-4 acetylamino-benzoic acid, and by their reduction and condensation with benzil to yield the same diquinaldine.



[I]

The isomerism is explained by these authors by supposing that in Kaufler's formula for diphenyl [I] the two phenyl rings are incapable of free rotation; it follows that two stereoisomeric dinitrobenzidines could be obtained both ortho-substituted to the amino-group, namely, a 3 : 3′-dinitro- and a 3 : 5′-dinitro-compound which can be represented in a plane thus :

3 : 3′-dinitrobenzidine
[II]

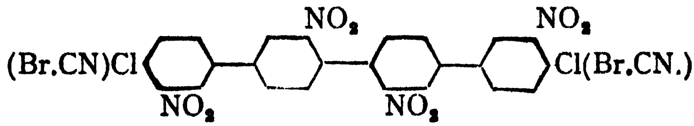


3 : 5′-dinitrobenzidine
[III]

Adopting the view that in general symmetrical compounds have a higher melting-point than unsymmetrical, formula [II] has been assigned to Bandrowski's, and formula [III] to Strakosch's dinitrobenzidine.

The isomerism extends to the acyl derivatives of the two dinitrobenzidines and to the dinitro-diphenyl obtained by removal of the amino-group. The two compounds differ in their behaviour in the Sandmeyer or Gattermann reaction.

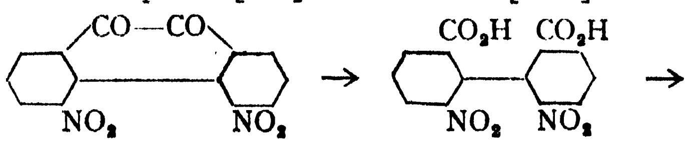
Although on diazotisation and treatment with potassium iodide they yield isomeric dinitro-diiodo-diphenyls where it is necessary to use a cuprous salt or copper powder, while the 3 : 3′-derivative gives the usual chloro-bromo-etc. compounds the 3 : 5′ dinitrobenzidine gives derivatives of benzerythrene [IV]



[IV]

An important confirmation of the existence of a new type of isomerism in the diphenyl series is supplied by the work of Kenner and Stubbings (*Trans. Chem. Soc.*, 1921, **119**, 593). The authors prefer not to decide at the moment between the merits of the various possible formulæ, but, for the sake of comparison, Cain's notation will be adopted here.

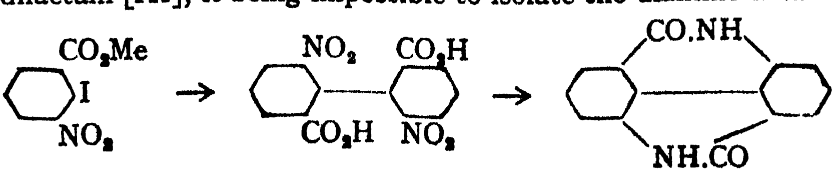
By the oxidation of a dinitrophenanthraquinone [V] Schulze (*Ann.*, 1880, **203**, 95) obtained a dinitrodiphenic acid, of which the orientation was established by Schmidt (*Ber.*, 1903, **36**, 3745) as [VI] by conversion into 2 : 2′-dinitrodiphenyl and through the diamino-compound [VII] into carbazole [VIII]



[V] [VI]

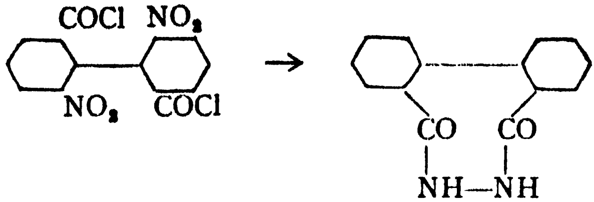
[VII] [VIII]

An attempt to synthesise the same acid by removal of iodine by means of copper from two molecules of 2-iodo-3-nitrobenzoic ester [IX] resulted in a new dinitro-diphenic acid [X] which differs markedly from the acid obtained by Schulze. On reduction Schulze's acid [VI] yields a diamino-dicarboxylic acid in the usual way, but the new acid gives a dilactam [XI], it being impossible to isolate the diamino-acid



[IX] [X] [XI]

The chloride of the 2 : 6′-dinitro-diphenic acid [XII] on treatment with hydrazine gives a hydrazide [XIII], although on hydrolysis it regenerates the original acid



[XII] [XIII]

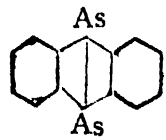
it is not clear, however, whether this compound is identical with the analogous one prepared from 6 : 6′-dinitro-diphenic acid ; if so, inversion has taken place apparently under the influence of hydrazine.

The further study of diphenyl compounds should yield results of interest and may suggest an alternative explanation to that of Cain which is not entirely satisfactory. The persistence of the isomerism under drastic chemical change and the apparent failure to convert one isomeride into the other are phenomena which would not be expected in the type of isomerism suggested. The production of the diquinaldine and of the benzerythrene derivatives in the case of Cain's isomerides and of the hydrazide in the case of the 2 : 6′-dinitro-diphenic acid seems to call for further explanation.

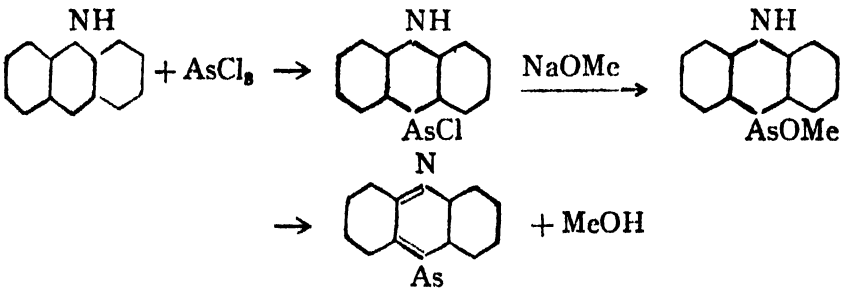
Another case of unexplained isomerism is recorded by Franzen and Helwert (*Ber.*, 1920, **53** [B], 319), who have obtained a new form of 1-chloro-4-nitronaphthalene in the preparation of this compound from 4-nitro-α-naphthylamine through the diazo-reaction. As, however, the second form was obtained in a unique experiment, and the authors failed to repeat its preparation, confirmation is necessary. This observation calls to mind the existence of two isomeric sulphides of β-naphthol described by Crymble Ross and Smiles (*Trans. Chem. Soc.*, 1912, **101**, 1146) which still awaits a satisfactory explanation.

Constitution of the Triphenylmethane Dyes.—Although the problem of the constitution of the triphenylmethane dyes attracted the attention of many famous chemists, little work has been done on this subject lately, and the quinonoid structure has been very generally adopted. Meisenheimer and Neresheimer (*Ann.*, 1921, **423**, 105) have, however, attempted to prepare optically active leuco-bases of the triphenylmethane dyes. If Rosentiehl's formula for these dyes is correct optically active leuco-bases on oxidation should give optically active dyes, but if a quinonoid compound is formed the optical activity should disappear. They tried to resolve compounds of the

type 4 : 4′-dimethyldiethyldiamino-triphenylmethane sulphonic acid but without success.

Organic Arsenic Compounds.—The number of organic arsenic compounds synthesised increases rapidly, and an up-to-date formula index of them would be of great use to workers in this field. Two interesting cyclic arsenic compounds have recently been obtained, one, phenarsazine  by Wieland and Rheinheimer (*Ann.*, 1921, **423**, 1) and the other arsanthrene  by Kalb (*Ann.*, 1921, **423**, 39).

The former is obtained by the following synthesis :

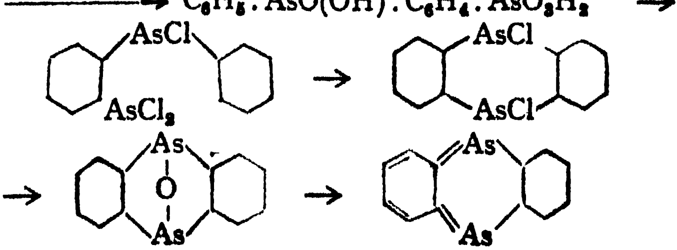


[XIV]

Phenarsazine is very reactive and readily adds water, alcohol, etc. ; the reactions are, however, reversible and on heating the compound formed in a high boiling, inert solvent the phenarsazine is reformed ; with hydrochloric acid phenarsazine chloride [XIV] is regenerated.

Arsanthrene, the arsenic analogue of phenazine, is synthesised from o-nitroaniline as follows :

$NO_2 . C_6H_4 . NH_2 \rightarrow NO_2 . C_6H_4 . AsO_3H_2 \rightarrow$
$NH_2C_6H_4 . AsO_3H_2 \rightarrow OH . N_2 . C_6H_4 . AsO_3H_2 \rightarrow$
$\xrightarrow{C_6H_5 . AsO} C_6H_5 . AsO(OH) . C_6H_4 . AsO_3H_2 \rightarrow$



BOTANY. By E. J. Salisbury, D.Sc., F.L.S., University College, London.

Indian Botany will receive a stimulus from the newly formed Indian Botanical Society, of which Shiv Ram Kashyap, M.Sc., of the Government College, Lahore, is the Secretary-Treasurer. It is not the present intention of the executive to start any publication, but one of the most useful activities contemplated is the establishment of a central exchange for botanical literature, and for aiding botanists generally. The promotion of research and the establishment of one or more biological institutes are also part of the official programme.

Morphology.—It would appear that the double stock, as other double strains, has arisen from the single type by a single mutation. In the *Journal of Genetics* Miss Saunders deals with some figures and statements respecting semi-doubles which might be regarded as incipient doubles, and therefore as evidence for the gradual assumption of the double condition. These are explained as an outcome of more or less perfect twinning of flowers produced during the period and in the region of maximum vigour.

The development of the leaf of mosses has been again investigated by M. Constantin (*Ann. Sci. Nat.*, April 1921). Working with a variety of species and genera this author has arrived at the conclusion that, as Hofmeister and Goebel observed, the leaf grows at first by an ephemeral initial-cell, the later growth being intercalary and with increasing age passing more and more towards the base. The leaf apex at an early stage shows signs of differentiation, and in *Mnium* the several-layered margin develops almost simultaneously throughout the length of the blade. All the mosses examined showed asymmetry of the leaf-base, a fact which can be attributed to the overlap of the leaves by one another.

Anatomy and Embryology.—In a study of the gametophyte and embryo of *Botrychium obliquum*, Campbell finds that the root arises endogenously, that the cotyledon and root are both bipolar, and that there is a suspensor, thus differing from *B. lunaria* and *B. virginianum* and approaching the condition in *Ophioglossum* (*Ann. Bot.*, April). Jeffrey and Torrey (*Bot. Gaz.*), in a further paper dealing with the derivation of herbaceous from arboreal Dicotyledons, conclude that there has been progressive loss of cambial activity in the foliar trace which, eventually extending to the remaining bundles, has brought about a monocotyledonous condition.

Macpherson, in the same journal, dealing with *Convolvulus sepium*, finds that polyembryony is the rule in this species rather than the exception, the supernumerary embryos being apparently developed from the synergids.

Genetics, etc.—In an extremely interesting paper on genetical

problems in the genus *Rosa* (*Trans. Nat. Hist. Soc. Northumberland, Durham, and Newcastle*) Heslop Harrison shows that the northern roses fall into eight series or "section-species," in each of which parallel forms are recognised, and which the author regards as examples of orthogenesis. Examination of the pollen showed that this was usually largely, or in part, abortive, indicating widespread hybridity. Several forms, especially those with high chromosome numbers, exhibit apogamy, and polyembryony occurs.

The same author and Miss Blackburn (*Ann. Bot.*, April) consider the cytological details, from which it appears that the normal haploid chromosome number in *Rosa* is seven. Failure of reduction, however, would appear to be frequent. *Rosa arvensis* and *R. rugosa* were found to be diploid; all the Eucaninæ, Afzelianæ, Rubiginosæ, Tomentosæ, and one hybrid were pentaploid, whilst *R. pimpinellifolia* × *R. tomentosa* v. *sylvestris* was found to be hexaploid.

Ecology.—Seeds of Douglas Fir from a variety of localities have been raised by Hoffman (*Ecology*, April). He finds a decrease in the percentage germination of the seeds and diminished vigour in the seedlings with increasing age of the parent tree or with increasing altitude of its situation. An unhealthy condition of the parents also has the same effect on the seeds and seedlings raised from them.

Somewhat diverse results have been obtained relative to dormancy in the Cocklebur (*Xanthium*). McHargue has reinvestigated the question and finds that the larger seed in the two-seeded burr normally germinates first and is the more vigorous, whilst the germination of the smaller may be delayed till the succeeding season. When removed from the burrs, however, there was no apparent difference in behaviour between the two.

Taxonomy.—Several papers of interest to British botanists have appeared in recent numbers of the *Journal of Botany*; these include: Forms of *Orchis maculata* (May), by T. and T. A. Stevenson; forms of *Matricaria inodora*, by Lester-Garland; Potamogetons of the English lakes, by W. H. Pearsall (June); and new British Sphagna and Hepatics are described by J. A. Wheldon and W. E. Nicholson (July). A useful key to the Harpidioid Hypna is being contributed to the *Naturalist* by J. A. Wheldon.

The North American species of *Piper* belonging to the section Ottonia are the subject of a paper by Trelease in the *Amer. Journ. Bot.* for April. Eight new species are described, and their variability with respect to what are usually regarded as important characters is a striking feature. Thus, whilst in most species the gynœcium consists of 3 carpels in

P. brachypus the number varies from 3 to 4, whilst 4 is normal for *P. ovatum*. Similarly, with respect to the stamens in two species, there are normally 6 stamens, in others there are 5, in still others the number varies from 5 to 6, whilst in *P. Heerianum* there are 3–6 stamens and in *P. ovatum* 4. The latter is the only species with pinnately veined leaves.

PLANT PHYSIOLOGY. By Prof. J. H. PRIESTLEY, D.S.O., B.Sc., F.L.S., Botanical Department, University of Leeds. (Plant Physiology Committee.)

Some Recent Work on Proteins in its Relation to Physiology.—One of the difficulties of the plant physiologist, and of the botanist who would follow the development of plant physiology, is the constant demand made upon him to assimilate and utilise the knowledge and technique developed in widely different fields of investigation.

As the writer thinks that a recent series of papers by Jacques Loeb are of the utmost importance in plant physiology, an attempt is made in the following pages, not to summarise the work, but to indicate by an elementary discussion the nature of the conclusions reached by Loeb. The papers themselves are cited at the end of this review, and references are also given to two valuable summaries of this work published by Loeb himself.

Loeb's work deals with the relation of proteins to electrolytes, and the key-note to the new standpoint is to be found in the attention paid to the effect of the hydrogen ion concentration upon the behaviour of the proteins.

This conception itself awaits application in almost every field of physiological investigation, and as it is not yet familiar to many botanists, it deserves further consideration.

It has long been realised that the acidity or alkalinity of the watery medium in which the reactions of the protoplast proceed is a factor of great physiological importance. The closer investigation of this question required a quantitative estimation of the sap reactions. The usual measure of acidity utilised by the physiologist has been the amount of standard alkali required to render the acid solution neutral, as indicated by the colour change of an indicator. The exact amount of alkali needed has therefore depended, to some extent, upon the indicator used, as different indicators would change colour upon the addition of different amounts of alkali.

Equivalent quantities of a "weak" organic or "strong" mineral acid give the same titration value for acidity if measured in this manner, although, as the common use just made of the terms "weak" and "strong" implies, there are real differences, not in the equivalence of the quantities of acid

present, but in their strength as acids. This "strength" of acidity which may be contrasted, as a kind of intensity factor, with the quantity of acid present and measured by titration, is expressed by the hydrogen ion or, more briefly, the hydrion concentration, and is the important factor from the physiological point of view. Methods of great delicacy have recently been placed at the service of physiologists (see Clark, *Determination of Hydrogen Ions*, Baltimore, 1920) for the measurement of hydrogen ion concentration in biological fluids, and this important physiological factor can now receive adequate numerical expression.

A neutral aqueous solution may be defined in terms of this factor as one in which the hydrion concentration equals the concentration of hydroxyl ions, as both these ions are produced in equal numbers from water by the reversible reaction

$$H_2O \rightleftharpoons H^+ + OH^-$$

The product of the concentrations of H^+ and OH^- ions is a constant at constant temperature, so that at constant temperature increasing acidity is measured by an increasing preponderance of H^+ ions over the OH^- ions.

At neutrality, the concentration of hydrogen ions expressed in terms of the chemists' "normal" solution (a solution containing one gram of hydrogen ions per litre under standard conditions) is approximately 1×10^{-7}N.

With increasing acidity this concentration increases rapidly, so that the minus index to the ten falls rapidly; but these numbers remain cumbrous to express and practically impossible to plot directly so as to show their relation to other changing factors. The custom has therefore grown up of plotting instead the logarithm of the reciprocal of the number expressing hydrion concentration, this number being preceded by the symbol pH. Thus, for true neutrality the pH value would be obtained as follows. Log of reciprocal of hydrion concentration

$$= \text{Log} \frac{1}{1 \times 10^{-7}} = \text{Log } 10^7 = 7 = \text{the pH}7.$$

When hydrion concentration, acidity, or alkalinity are therefore represented by figures preceded by the symbols pH, it must be remembered that increasing hydrion concentration or increasing acidity are represented by *smaller* figures, decreasing hydrion concentration or increasing alkalinity by *larger* figures; also that the change from pH6 to pH7 does not indicate a small arithmetic decrease in the number of hydrogen ions present, but means that there are *one-tenth* as many hydrogen ions present.

The first essential point in Loeb's work is that all his studies

of the behaviour of proteins in the presence of various electrolytes have been carried out at definite hydrion concentrations. In previous work care had only been taken to act upon equal amounts of protein with chemically equivalent amounts of different salts, and the resulting generalisations, such as the Hofmeister series in which cations or anions could be arranged in order of the magnitude of the effect they produced upon the physical properties of the protein, seemed purely arbitrary from the standpoint of fundamental chemical principles.

Loeb's work has been carried out upon gelatin, which has proved exceedingly suitable experimentally, as the gelatin can at any time be brought into solution by raising the temperature, and then the pH of the solution determined by the now well-known indicator methods (see Clark, *loc. cit.*).

The first result of working under conditions thus controlled was to show that gelatin, as every other amphoteric protein, had a definite pH value at which it appeared to be perfectly neutral.

At this value, pH 4·7, it combined neither with acid nor basic ions, and such iso-electric protein readily came out of solution (see also Lloyd, *Biochem. Journ.*, **14**, pp. 147, 170, and 584–5, 1920). In more acid solution, i.e. with pH $<$ 4·7, gelatin could only behave as a base and combine with anions whilst in more basic solution, with pH $>$ 4·7, it could only behave as an acid and combine with cations.

This behaviour of gelatin was beautifully illustrated in an experiment of Loeb's in which equal quantities of gelatin were soaked in different strengths of nitric acid, then washed with cold water to remove excess acid and transferred to $\frac{M}{64}$ silver nitrate.

The gelatin samples were then all thoroughly washed with ice-cold water to remove any silver nitrate not combined with the gelatin. By melting the gelatin masses, solutions were obtained from samples of which the pH could readily be determined, whilst by simply standing the gelatin tubes in the light, the darkening of the silver soon showed where silver had been retained by the gelatin. It was then clearly demonstrated that silver was present only in those tubes in which the pH was $>$ 4·7. Thus the gelatin only combined with the Ag^+ cation when it was in contact with it at a pH more alkaline than its own iso-electric point.

Very significant for the cytologist also are the results of experiments with acid and basic dyes. Basic dyes are only retained by gelatin, after thorough washing with cold water, in solutions with pH $>$ 4·7; acid dyes are only retained in solutions with pH $<$ 4·7.

With this point in the behaviour of an amphoteric protein cleared up it was possible to reinvestigate the behaviour of various salts with gelatin, bearing always in mind the pH of the reacting solution. It was now possible to show volumetrically that when gelatin combined with any cation or of a salt, the combination took place according to the ordinary mass action laws for reacting substances.

Thus the amount of combination depends upon the amount of acid or basic protein present; this depends in the first place upon the original concentration of the protein, and then upon the pH at which the reaction is proceeding.

The amount of cation or anion entering into combination will simply depend upon its concentration, dissociation, and valency; ions of equivalent valency will therefore produce the same effect upon the same protein solution if they are reacting at the same pH. The " Hofmeister series " therefore now disappears, and Loeb has shown that it may be replaced by a series of generalisations that can be briefly stated as follows:

(1) An amphoteric protein has a definite iso-electric point at which it is practically without reaction either with cations or anions. At hydrion concentrations of pH greater than this (more basic), it behaves as an acid and combines with cations; when pH is less (more acid), it behaves as a base and combines with anions.

(2) The amount of combination at any definite pH depends upon the laws of mass action and the valency of the combining ions, exactly as in the case of reactions between crystalloids in solution.

(3) The influence of different ions upon the physical properties of the proteins can be predicted therefore from the general combining ratios of these ions.

That the last statement is true has been established experimentally by Loeb in a series of experiments upon the swelling, viscosity, and osmotic pressure of gelatin in presence of different salts at known pH values. Loeb has shown that the physical explanation of this behaviour of different ions upon the physical properties of the protein may be traced to the Donnan Equilibrium obtained when two salt solutions are separated by a membrane impermeable to one ion but permeable to all the rest. In this case the membrane is the surface of the colloid particles in the heterogeneous protein solution, the ion refused passage is the gelatin ion of the gelatin salt, cation, or anion respectively according to the reaction of the medium. Loeb's original papers need careful study for a full appreciation of his conclusions and their experimental basis. Perhaps the strongest inducement to the botanist to undertake this task will be some tentative indications of the

possible significance of Loeb's views in relation to current problems of plant physiology.

Permeability.—It is fairly clear that the semi-permeable properties of the surface layer of the protoplast depend largely upon the protein constituents of this layer. Both physical and chemical properties of these protein constituents evidently depend, in view of Loeb's work, upon the reaction of any sap bathing this surface layer.

Long series of anions and cations, comparable with the Hofmeister series, given by various investigators from a study of the effect of salts upon the properties of the protoplasmic membrane, will obviously require revision from this standpoint (see, for instance, Borowikow, *Bioch. Zeitschr.*, **48**, p. 230, 1913). A difference in ionic penetrability may well be associated with different ionic diffusion velocities, but, in so far as these series are purely empirical, they may be expected to disappear on further analysis.

Staining Reactions.—The very difficult problems of vital staining will possibly be partly elucidated by a reconsideration of the behaviour of acid and basic stains in relation to the hydrion concentration of the medium, and this will be true even if Rubland's view (*Bioch. Zeitschr.*, **54**, pp. 59–77, 1913, and *Koll. Zs.*, **12**, pp. 113–124, 1913) is correct that penetration of these stains is essentially a question of pore diameter in the living membrane. Another possible application of Loeb's work appears to lie in the fact that different protein constituents of protoplasm probably have different iso-electric points. It may therefore be possible, by destaining stained material at different but definite pH values, to discriminate between one protein member of the protoplasmic complex and another.

Thus, a protein with iso-electric point at pH4 will not release a basic stain if washed in a medium with pH 4·5, whilst a protein with iso-electric point at pH5 will release a basic but retain an acid stain when destaining in the same medium. The possible significance of these considerations, in interpreting the phenomena of double or differential staining, will be obvious to every cytologist.

Reception of Tropic Stimuli.—The Donnan Equilibrium will be in existence at every protoplast surface, and as a result of the unequal distribution of ions on the two sides of the protoplast membrane an electric charge may be expected at each such surface. Reference to Bayliss (*Principles of General Physiology*, 1915 ed., pp. 643–50) will show that a similar charge is assumed to exist along the surface of the living cells of nerve or muscle. The local excitation of this tissue by a stimulus is assumed to cause a momentary increase in permeability, permitting the passage of the ion previously restrained and therefore re-

sponsible for the ionic distribution at the basis of the Donnan Equilibrium.

This free passage of all ions brings about their equal distribution and the disappearance of the electric charge at the excited area. This area then becomes of different electric potential to the rest of the surface of the organ, and this change is initially caused by a change in the *solid* medium at the surface of the living cell. Naturally this change is propagated along the solid surface as a wave of compression is transmitted along a stretched string, and then we have the transmission of a nervous impulse with its accompanying E.M.F. detectable by suitable galvanometric methods.

If in the plant cell we have a similar solid surface with the unequal distribution of ions anticipated and explained by Donnan, we may find the clue to the reception of tropic stimuli in this ionic distribution and its accompanying electric charge.

This is in effect what is suggested by Small (*New Phytologist*, **19**, pp. 49–62, 1920), when he associates the different response of stem and root to tropic stimuli to a different reaction of the cells of stem and root or to the different electric charge at the protoplast surface in stem and root, a natural consequence of the assumption of different sap reaction.

If protoplasts have this different charge in stem and root then the result of a stimulus which increases the permeability will be that in one case the stimulated region will become electro-positive, in the other electro-negative, to the surrounding regions. It therefore would seem natural that the same directive stimulus should produce directly opposite effects upon stems and roots.

Recent controversy (Blackman, *New Phytologist*, **20**, pp. 38–42, 1921, Small, *ibid.*, 20, pp. 73–81, 1921) has centred around the adequacy of the " creaming " mechanism assumed by Small. If it is remembered that the protoplast on which the surface charge is distributed is a turgid structure with a delicate solid surface, it would seem that the necessary difference in permeability might readily be produced by the new strains in this solid surface induced by changes in its position relative to gravity, without reference either to emulsions or to statoliths. To take this question further would be too speculative; the writer has only desired to show that in all directions Loeb's work upon the behaviour of proteins to electrolytes at different hydrion concentrations will be found most fertile in its application to problems of plant physiology.

Loeb's papers will be found in the *Journal of General Physiology*, 1918–1921, as follows: Vol. I, pp. 81–96, 237–54, 363–86, 483–504, and 559–80. Vol. II, pp. 273–96, 387–408,

563–76, 659–72, 673–88. Vol. III, pp. 85–106, 247–69, 391–414, 557–64, 667–90, 691–714.

Useful summaries by Loeb are (1) "Address to the Harvey Society, October 1920," in *Science N.S.*, 52, pp. 449–56, 1920. (2) "Chemical and Physical Behaviour of Proteins," *Chemical and Metallurgical Engineering*, p. 550, 1921.

Both these have been reprinted in the *Journal of the Society of Leather Trades' Chemists*, Vol. V, No. 5, May 1921.

ENTOMOLOGY. By A. D. Imms, M.A., D.Sc., Institute of Plant Pathology, Rothamsted Experimental Station, Harpenden.

Some of the more important contributions to entomology that have come to notice during the past half-year may be briefly referred to under the following headings:

General Entomology.—G. C. Crampton has contributed a number of papers bearing upon the general morphology of insects. In *Journ. N.Y. Ent. Soc.* (**29**, 63–100) he discusses the phylogenetic origin of the mandibles of insects and other Arthropoda and concludes, from the evidence afforded by these appendages, that the line of development of the higher Crustacea has accompanied that of the Insecta much further than has happened in the case of the Symphyla. This conclusion is in agreement with that derived from the study of other organs, and he regards the Crustacea, rather than Symphyla, as being ancestral to the Insecta. In *Trans. Ent. Soc.*, April 1921, the same author has a preliminary note on the interpretation of insectan and myriapodan structures. The maxillulæ (or superlinguæ), which are present in the more primitive insects, are regarded as being the homologues of the paragnaths of Crustacea. The work of Folsom on *Anurida* is discredited, and the view that the maxillulæ represent the appendages of a separate head-segment is not accepted. The author also disputes the often-repeated statement that the primitive biramous condition of the limbs in the lower Crustacea is preserved in the maxillæ of insects. Any criticism of this important article needs to remain in abeyance until the details, upon which its conclusions are based, are forthcoming. The presidential address to the Entomological Society of London by J. J. Walker, on the insect fauna of New Zealand, brings together a great deal of scattered information, and will prove useful to students of geographical distribution. A second edition of Houlbert's *Les Insectes* (Paris, 1920) has appeared. It is a handbook of 374 pages with 207 text-figs., and is intended for those who desire a general elementary knowledge of entomology. After an interval of several years Lief. 5 and 6 of Schröder's *Handbuch der Entomologie* has been published by Fischer of Jena.

Hymenoptera.—Brues (*Am. Nat.* **55**, 134–64) contributes an interesting survey of parasitism among Hymenoptera, with particular reference to the relation between taxonomic affinities and methods of feeding. The complete life-histories of three Chalcid ectoparasites of *Harmolita tritici* have been followed by W. J. Phillips and F. W. Poos (*Journ. Agric. Res.*, June 15, 1921). (Miss) M. Haviland (*Q.J.M.S.*, **65**, 101–127) has supplied a careful account of the biology of two species of *Lygocercus*, which parasitise the Braconid *Aphidius*. It is a useful contribution to what little is known of the economy of the Scelionidæ. A. C. Kinsey (*Bull. Am. Mus. Nat. Hist.*, xlii, articles v–vii) writes on American Cynipidæ. In article vii the author estimates that 86 per cent. of the known species of the family affect *Quercus* and are confined to that genus. Another 7 per cent. are confined to species of *Rosa*. The remaining 7 per cent. occur on plants belonging to various natural orders, and it is evident therefore that 93 per cent. of the known Cynipidæ are restricted to two plant genera only. The author concludes that alternation of generations is a more or less extreme development of seasonal dimorphism and is primarily due to seasonal environmental conditions. L. M. Alston (*Proc. Zool. Soc.*, 1920, 195–243) has contributed a detailed account of the life-history of two parasites of *Calliphora erythrocephala*. One of the species, *Alysia manductor*, parasitises the larval host and its imagines emerge from the puparia. This Braconid is a member of an anomalous group which was elevated by Ashmead to the rank of a separate family. The second species dealt with is the Chalcid *Nasonia brevicornis*, which is a pupal parasite of the *Calliphora*. G. Grandi (*Boll. Lab. Zool. Portici*, **14**, 63–204) has published a lengthy study of the morphology and biology of *Blastophaga psenes*. It is by far the most thorough investigation of any Chalcid yet attempted, and is accompanied by a remarkably complete bibliography, and a review of the subject of caprification with which this species of *Blastophaga* is so closely associated.

Diptera.—G. S. Cotterrill (*Proc. Zool. Soc.*, 1920, 629–647) gives a much-needed detailed account of the metamorphoses of the common yellow dung-fly *Scatophaga stercoraria*. It is remarkable that so familiar an insect should never have been adequately studied previously. D. Keilin (*Parasitology*, xiii, 180–83) contributes a further annotated list of larval Diptera known to prey upon Mollusca. J. E. Collin (*Trans. Ent. Soc.*, 1921, 305–26) describes the various British species of *Hammomyia* and *Hylephila* which, it may be added, are closely related to the cabbage-root fly and the onion-fly. Their biology is of interest in that they are closely associated with solitary bees, particularly of the family Andrenidæ. The female flies

haunt the burrows of the bees and their larvæ, and, so far as known, live on the pollen masses stored in the cells of these Hymenoptera. W. W. Marchard (*Monog. Rockfeller Inst. Med. Res.*, 13) has a bulky contribution of over 200 pages on the larval stages of the Tabanidæ. A. L. Melander (*Psyche*, **27**, 91–101) gives a synopsis of the family Psilidæ, and H. G. Dyar has a contribution on the mosquitoes of Canada (*Tr. Roy. Canad. Inst.*, xii, 71–120). W. R. and M. C. Thompson (*Proc. Ent. Soc. Washington*, xxiii, 127–39) write on the life-history of the Tachinid *Zenillia roseanæ*. This research was carried out in the South of France and forms part of a scheme for discovering suitable parasites for introduction into America with the object of controlling the injurious European Corn-borer. In its second larval instar this parasite bores through the wall of one of its host's tracheæ by means of its spinose anal extremity. An attachment is acquired, and the parasite is thus enabled to breathe the air taken in during the inspiratory action of its host.

J. E. Eyre (*Ent. News*, xxxii, 215–16) has a suggestive note with regard to the rearing of Anthomyiid larvæ on artificial media consisting of agar jelly containing a high percentage of extract of the larval food-plant. *Hylemyia antiqua* and *brassicæ* were reared by this means, and oviposition took place on the media adopted. This method very possibly has a future before it, particularly where it is desired to undertake rearing experiments under strictly controlled conditions which might otherwise be unobtainable.

Hemiptera.—The two most notable contributions under this heading deal with the Coccidæ. Prof. Silvestri has edited the final work of the late Gustavo Leonardo entitled *Monographia delle Cocciniglie Italiane* (Portici, 1920, vi + 555, 375 figs.). In this treatise 147 species are dealt with, and the volume is one which will find a place on the shelves of all European students of the family. The second work is by A. D. MacGillivray, whose monograph, *The Coccidæ* (Urbana, Ill., 1921, $6·00) runs to over 500 pages. The general introductory part might well have been longer, and we deplore the many nomenclatorial alterations that are introduced. The author recognises 120 new genera and includes very full tables of the major groups, genera, and certain of the species. R. E. Snodgrass (*Proc. Ent. Soc. Washington*, xxiii, 1–15) has reinvestigated the structure of the mouth-parts of the Cicada and discusses the difficult question of settling the homologies of these organs in the Hemiptera. J. Davidson (*Bull. Ent. Res.*, xii, June 1921) gives a complete account of the life-cycle of *Aphis rumicis*, with detailed figures illustrating the morphological characters of the different types of individuals. Work along these lines is greatly needed if we are ever satisfactorily to elucidate the problem of

species among the Aphides. The derivation of the family name for the latter insects is discussed by A. C. Baker (*Proc. Ent. Soc. Washington*, May 1921), who is of opinion that the correct spelling should be Aphiidæ rather than Aphididæ or Aphidæ. F. V. Theobald (*Journ. Pomology*, ii, 20 pp.) gives the results of his work on *Schizoneura lanigera*. For many years its life-cycle was supposed to have been known until the fact that the elm is its primary host was discovered in America. Theobald has observed the same cycle in Britain, and has established the fact that this insect migrates from the elm to the apple in July, which alters the correct time for spraying as well as explaining many past failures in treatment.

Lepidoptera.—Among faunistic works Holland's Lepidoptera of the Congo (*Bull. Am. Mus. Nat. Hist.*, 43, 109–369) is important and adds considerably to our knowledge of the distribution of those insects in Africa. A. M. Moss (*Nov. Zool.*, **27**, 333–54) has published a series of observations on the early stages, food plants, and habits of the Sphingidæ observed around Para, Brazil. Among morphological works is an investigation by Swaine on the nervous system of the larva of *Sthenopis thule* (*Canad. Ent.*, lii, 275–83). F. Brocher (*Arch. Zool. Exp.*, lx, 1–45) has investigated the circulation of the blood in *Sphinx convolvuli*, and S. Metalnikow (*Comp. Rend. Soc. Biol.*, lxxxiii, 817–20) has studied immunity in the larvæ of the wax-moth. He finds that they exhibit a remarkable resistance to the pathogenic organisms of many of the most virulent human diseases, but are very susceptible to others which are only slightly pathogenic. E. Minnich (*Journ. Exp. Zool.*, xxxiii) has studied certain chemotropic responses in *Pyrameis atalanta* and *Vanessa antiopa*, and describes organs of chemical sense situated at the distal ends of the tarsi of the four walking-legs. Part 24 of the *Lepidopterorum Catalogus*, dealing with the subfamily Nolinæ, has also appeared.

Coleoptera.—Verhoeff (*Arch. Naturges.*, 1919, A. 6, 1–111) contributes his fourth article on the morphology and biology of the Staphylinoidea and deals in the present instance with the larval structure. E. Warren (*Ann. Natal Mus.*, iv, 297–366) has an important paper on the comparative anatomy of *Paracorotoca*, a genus of beetles belonging to the same group. Blunck (*Zeits. f. wiss. Zool.*, **117**, 1–129) has published the second part of his detailed studies of the post-embryonic development of *Dytiscus*, and G. Steinke has investigated the spiracles in a number of coleopterous larvæ (*Arch. Naturges.*, 1919, A. 7, 1–58). Among other papers a considerable proportion are devoted to the Coccinellidæ. Davidson (*Ent. News*, xxxii, 83–9) has dealt with the life-history of a common American species, *Psyllobora tædata*, which is found associated with fungi

of the mildew type. It has the unusual habit of being fungivorous, and efforts to induce it to partake of a carnivorous diet proved unsuccessful. J. H. Gage (*Illinois Biol. Monog.*, vi, No. 4, 1–62) has investigated the life-histories of a number of species and (Mrs.) O. A. M. Hawkes (*Proc. Zool. Soc.*, December 1920) has published the results of observations in the biology and colour inheritance of the very common English lady-bird, *Adalia bipunctata*. In the *Annals of Applied Biology*, December 1920, are two biological studies of Coleoptera. (Miss) D. J. Jackson has a contribution dealing with the structure and bionomics of weevils of the genus *Sitones*, which are injurious to leguminous crops in Britain, and W. Ritchie has a very similar type of paper on the Longicorn *Saperda carcharias*. Both papers are admirably illustrated, and are records of a great deal of careful observation. Leng's recently published "Catalogue of the Coleoptera of America North of Mexico" (*New York*, 470 pp.) lists 18,547 species, and over 800 fossil forms are included. The order is divided into the two sub-orders Adephaga and Polyphaga comprising 22 super-families and 109 families. The *Catalogus Coleopterorum* (Junk and Schenkling) has made further progress, parts 71 and 72 having recently appeared. Part 71 includes the Dytiscidæ, Haliplidæ, Hygrobiidæ, and Amphizoidæ, which are comprised in 326 pages. Part 72 deals with the subfamily Cetoninæ, a list of whose species occupies over 400 pages.

Orthoptera.—The two most important contributions to our knowledge of this order of insects are by P. Cappe de Baillon (*La Cellule*, xxi, 1–245) and L. Chopard (*Insecta*, Rennes, 1920). The former writer has an elaborate memoir on the eggs of the Locustidæ, while the work of Chopard deals with the morphology and development of the terminal abdominal segments among Orthoptera. The latter paper is important to students of general morphology.

Applied Entomology.—From among the enormous number of text-books, articles, and bulletins which appear almost weekly and deal with the applied aspects of entomology it is only possible to select a few for special mention. C. F. N. Swynnerton (*Bull. Ent. Res.*, xi, 315–86) contributes an important paper on the Tsetse problem in North Mossurise, Portuguese East Africa. The work in this area is of particular interest, not only on account of the number of species of *Glossina* which occur, but also from the fact that the district, when under Zulu domination, was the scene of an artificially directed scheme of settlement which resulted in the disappearance of Tsetse flies. The latter result suggests the possibility that properly planned settlement is, in itself, capable of clearing infested areas of the two species here concerned, *G. brevipalpis* and *G. pallidipes*.

The report by C. B. Williams on the Froghopper blight of sugar-cane in Trinidad (*Mem. Dept. Agric. Trinidad and Tobago,* **1**, 170 pp., 11 pls.) deserves special mention, and is a valuable study of the relations between outbreaks of the causative insect (*Tomaspis saccharina*) and the conditions under which the affected crops are grown. The economic entomologist is beginning to realise that work of this nature is likely to open up fresh methods of control. If it is possible to grow crops less susceptible to attack either by attention to cultural conditions, or by the breeding of more or less immune varieties, some of the expense and labour devoted to the application of insecticides might be saved. *Entomological Bulletin*, No. 827, of the U.S. Dept. of Agriculture deals with insect control in flour-mills. The most satisfactory method for dealing with all classes of mill-infesting insects is the application of heat (118° to 125° F.). To carry out this process effectually the installation of radiators or radiation surfaces is necessary. It has been estimated that this can be fitted up sufficiently economically in an average-sized mill to pay in five years for the cost of its introduction. L. Moreau and E. Vinet (*Ann. Serv. des Epiphyties*, vi, 299–312) report on a series of experiments dealing with the chemotropic method of controlling the moth *Clysia ambiguella* in French vineyards by means of traps baited with attractive substances. The authors find, as Imms and Husain have found in England, that chemotropic responses are very dependent upon climatic conditions. The maximum numbers of the *Clysia* were caught just after sunset, and there was a gradual decrease after 10 p.m.; 65·1 per cent. of the trapped individuals were females.

J. F. Illingworth (*Journ. Econ. Ent.*, **14**, 238–9) briefly describes some experiments with the use of crude white arsenic (arsenious acid) as a soil insecticide against larvæ of the beetle *Isodon puncticollis*. An application of 80 lb. to the acre gave encouraging results, and it is stated that even such a relatively enormous dose as 200 lb. to the acre had no detrimental effects upon the growing plants. Sugar-cane, for example, grown upon land so treated revealed no trace of arsenic in the sap. Much more extensive experiments, than the author has yet carried out, are needed before the application of a substance of this nature can be advocated. Its effects on the micro-organisms of the soil need to be considered, and we also need to know whether arsenical compounds of sufficient strength to be toxic to insects might not soon render the soil infertile. In *Soil Science* (xi, 305–318), A. Peterson writes on the application of paradichlorobenzine as a soil insecticide with reference to the control of the Pear-tree Borer.

At a joint meeting of the American Association of Economic Entomologists and the American Phytopathological Society

on December 31, 1920, a series of papers were read on insect and fungus control by means of dusting. These, and the discussions which ensued, are reported in *Journ. Econ. Entom.* for April, 1921. The subject was treated from not only the biological, but also the physico-chemical and mechanical aspects. The report on this symposium will interest economic entomologists throughout the world.

In the recently published text-book by Brues (*Insects and Human Welfare*, Cambridge, Mass., pp. xii+104, 42 figs., $2·50), an attempt is made to present some of the principles and practices of economic entomology. Emphasis is laid upon the variety of ways by means of which noxious insects are combated, and the biological method of utilising their natural enemies and parasites is regarded as the most promising. Another American text-book has also appeared : it is edited by W. D. Pierce, and entitled, *Sanitary Entomology, Entomology of Disease, Hygiene and Sanitation* (Boston 1921, pp. xxvi + 518), and is a comprehensive work on the whole of the subjects embraced within its scope. A second edition of E. D. Sanderson's *Insect Pests of the Farm, Garden, and Orchard*, revised by L. M. Peairs, is now available. It contains several new articles, and three additional chapters.

PALÆONTOLOGY. By W. P. Pycraft, F.Z.S., A.L.S., F.R.A.I., British Museum (Natural History), London.

After my survey of 1920 there came to hand a paper of quite exceptional interest on the Anatomy of the Pre-orbital Fossa in the Equidæ and other Ungulates : contributed by Dr. W. K. Gregory to the *Bulletin of the American Museum of Natural History*, vol. xlii, 1920. A few years ago, it may be remembered, Lydekker drew attention to the existence of a pre-orbital fossa (malar fossa) in the skulls of Arab, Thoroughbred, and Shire horses, which, he insisted, represented the last vestige of a once much deeper fossa lodging a face-gland—" larmier." Dr. Gregory, in this Memoir, shows conclusively that, in the first place, the " pre-orbital fossa " of Lydekker is certainly no more than a muscular depression, seating the origin of the *Maxillo-labialis superior* ; and, in the second, that it is the lachrymal fossa which lodges the face-gland, which is so conspicuous a feature in so many ruminants. In existing Equidæ this fossa, as a rule, is barely traceable, but in many extinct species it was of great size. Here, however, it served not for a face-gland, but as the bed of a large nasal diverticulum extending backwards, from the roof of the external nostril, as far as the level of the eye. All that remains of this diverticulum in existing horses is the vestige known as the " false nostril," which never extends backwards beyond the level of the naso-premaxillary

cleft. Pocock was therefore right when he challenged the validity of the association of the pre-orbital or malar fossa with a face-gland. Dr. Gregory's Memoir is illustrated with a number of very beautiful figures.

As touching the broad outlines of the evolution of the horse, or the elephant, we are all in agreement; but it is otherwise where interpretation is concerned with tables of descent. The tendency to-day is to break up the phylogenetic sequences which have been generally accepted, and to multiply the number of hypothetical ancestors belonging to a still more remote past. These, when found, are to link up, once more, the now dismembered ancestral tree.

A case in point is furnished by Prof. Henry Fairfield Osborn in discussing the Evolution, Phylogeny, and Classification of the Proboscidea (*American Museum Novitates*, No. 1, 1921). In the space of a few pages the author has contrived to provide material for argument for a very long time to come: for many of his conclusions run counter to the generally accepted beliefs on this theme. But even those who venture to differ from him will find in this essay a stimulus to further research.

In a second paper, on the First Appearance of the True Mastodon in America (*American Museum Novitates*, No. 10, 1921), Prof. Osborn shows that this animal reached America during the early Pliocene, as a derivative form of *Mastodon tapiroides americanus*, of the lower Pliocene of Hungary. It is represented in America by two species here described for the first time—*M. matthewi*, from Sioux County, Nebraska (*Procamelus Hipparion Zone*), and *M. merriami*, from the Thousand Creek formation, Humboldt County, Nevada.

Dr. W. K. Gregory's fine "Review of the Evolution of the Lachrymal Bone of Vertebrates, with Special Reference to that of Mammals" (*Bull. American Museum of Natural History*, vol. xlii) was not published till December last. It will be read, and eagerly read, by zoologists and palæontologists alike. His survey embraces the whole of the vertebrates from the fishes upwards, but it is too long to be briefly summarised.

A fossil marsupial skull of remarkable interest is described by Dr. H. D. Longman, the Director of the Queensland Museum, in the *Memoirs of the Queensland Museum*, vol. vii, 1921. The surprising feature of this skull lies in the enormous and outstanding jugals, which, it is suggested, formed "cheek-pouches." Whether this interpretation is correct is doubtful. A further study of this skull, and a comparison with that of *Diprotodon*, may enable this point to be settled later. For the moment, at any rate, the skull of *Euryzogoma* forms a notable addition to the list of instances of "Hypertely," so interesting from the evolutionary point of view.

Another mammal paper which must be carefully read is that of Prof. Schlosser (*Beit. z. Kennt. der Saugethiere-reste aus den untersten Eocaen von Reims, Stuttgart,* 1920), since it makes a notable addition to our knowledge of the Cernasien fauna.

Prof. G. V. Arthaber's paper (*Über Entwickl., Ausbilding, & Absterben der Flugsaurier,* 1921) is chiefly remarkable for the number of grotesque figures, masquerading as "restorations" of *Archæopteryx* and the pro-aves which adorn its pages.

Our knowledge of the Hadrosauridæ (Trachodontidæ) has been materially enlarged by the investigations of Dr. Laurance Lambe. He makes a further, and most welcome, addition to this theme, in his able Memoir on the Hadrosaur *Edmontosaurus* from the Upper Cretaceous of Alberta (*Canada Geological Survey Memoir,* 120). The genus *Edmontosaurus,* new to science, is founded on two skeletons, one of which was found in a naturally disarticulated condition, embedded in a clayey sandstone so easily removed that the bones could be cleaned with the greatest ease, thus enabling the elements of the skull to be studied with a completeness till now impossible. The importance of this will be appreciated in full measure not only by those who are engaged in the study of these huge reptiles—a study rendered more difficult by the wealth of forms which have been brought to light during recent years—but also by those who are concerned with the morphology of the vertebrate skull in general. The author concludes his Memoir with a revision of the classification of the Hadrosauridæ, adding a third sub-family—*Stephanosauridæ*—to the two already recognised. A number of extremely well-drawn figures add greatly to the value of his work.

A welcome complement to this Memoir is that of Dr. C. W. Gilmore on the Osteology of the Carnivorous Dinosauria (*Smithsonian Institute Bulletin,* **110**, 1920). This has special reference to the genera *Antrodemus, Allosaurus,* and *Ceratosaurus.*

In a paper on the Structure of the Reptilian Tarsus (*P.Z.S.,* 1921) Dr. R. Broom points out that the most remarkable features of the Amphibian and Reptilian tarsus are the almost constant presence of the fibulare and intermedium, and the great variability of the tibiale. The last named is rarely a large element, and frequently entirely or partly cartilaginous. In many types it is completely absent.

Palæichthyologists will welcome the Memoir on Triassic Fishes from Spitzbergen, 1920, by Dr. Erik Stensiö, not merely because of the relative inaccessibility of that island, but also because he has done his work very thoroughly, drawing some valuable comparisons between these and modern Ganoids and

Teleosts. No less than 35 plates and 90 text figures have been allotted to this work, thereby adding still more to its usefulness.

Of papers on fossil Invertebrates the most important is that on "A Contribution to the Description of the Fauna of the Trenton Group," by P. E. Raymond (*Canada Geol. Survey Museum Bulletin*, No. 31, 1921). This Report is intended to give the results of a study of a number of new or unusual fossils that were found in the Trenton group during a study of the Ordovician formations in Ontario and Quebec. Remains of Cystoidea, Brachiopoda, Gastropoda, and Trilobita constitute the types which are described.

ANTHROPOLOGY. By A. G. THACKER, A.R.C.S., Zoological Laboratory, Cambridge.

THE publications of the Bureau of American Ethnology, which is a section of the Smithsonian Institution, are of an extensive character, and, indeed, some of the bulletins are in themselves small books. These publications are generally detailed studies of the manners and customs of the American Indian tribes, and are therefore chiefly of interest to those who are specialising in this particular branch of social anthropology. Some of the bulletins possess, however, a wider interest, and this is the case with Bulletin No. 66, which was recently issued. This is entitled "Recent Discoveries attributed to Early Man in America," and is by Dr. Ales Hrdlicka. The paper is a thorough and critical examination of the claims of various recent supposed discoveries of Pleistocene human remains in North America. Some years ago Dr. Hrdlicka investigated the alleged discoveries of fossil men in South America, many of which he had no difficulty in showing to be of a surprisingly fantastic character, and all of which rested upon quite insufficient evidence. But the cases with which Bulletin No. 66 deals are of a somewhat more serious character, and were supported and credited by various United States anthropologists of repute. The more important of these recent claims to the discovery of fossil men in the Western Hemisphere are three in number: (1) The Ancient Man of Cuzco, Peru, which was found by the Yale Peruvian Expedition of 1912; (2) the Rancho La Brea Man, of California, which was found in 1914 and was alleged to be of Pleistocene antiquity; and (3) the skeletons from Vero, in Florida, which were found in an undoubtedly Pleistocene stratum, and whose claims to be regarded as truly original inclusions in that stratum were strongly supported by Dr. E. H. Sellards. These last skeletons were found only as recently as 1916, and would doubtless have

attracted far more attention in Europe if the war had not been in progress. Dr. Hrdlicka deals at great length with these various skeletons, and throughout the discussion the English reader is strongly reminded of the controversies which raged around our own Ipswich skeleton and Galley Hill skeleton. Indeed, the circumstances appear to be closely similar, and one is not surprised when Dr. Hrdlicka comes to the conclusion that these various skeletons are burials of a very much later date than the stratum in which they were found. Thus the general result is that Dr. Hrdlicka dismisses all these cases of supposed discoveries of fossil man in America.

The *Journal of the Royal Anthropological Institute* for January–June 1920 (vol. l) contains several articles of first-rate interest. The first paper is by Dr. H. J. Fleure, and is entitled "Some Early Neanthropic Types in Europe and their Modern Representatives." The paper begins by describing certain physical characteristics which are to be found associated with hyperdolichocephaly in the Plynlymmon moorland region. Dr. Fleure gives the characteristics of the people of this district as follows :

"Absolute head length great (typically over 200 on the head) ; this length due partly to strength of glabella and partly to marked and rather low occipital prominence. Cephalic index usually well below 74 on the living head and sometimes below 70. The sagittal line on the skull stands out and has a marked down slope on either side, and the height of the head is a marked feature in spite of the apparently low and receding forehead, the characters of which are accentuated by the great development of the supraciliary arcs and the glabella ; this head form is described as hypsistenocephalic. A sulcus is well marked over the supraciliary arcs, but not over the glabella. The forehead is narrow with well-marked lateral orbito-temporal depressions. The orbit is rather long and low, and this is linked with the rather large bizygomatic breadth. The nose is rather broad, and there is usually some degree of alveolar prognathism ; the palate is horseshoe-shaped rather than V-shaped in cross-section. The chin may be weak. The stature is usually not very great. The arm is rather long in proportion to the leg. The nails rarely have lunulæ. Hair, eyes and skin are often all dark with, frequently, a florid tendency in the complexion. Most have wavy hair, but a very few have been traced with curly hair and marked prognathism and small size, and sometimes the horseshoe palate and very strong white teeth. Hairiness is sometimes very marked, and there may be an unusual amount of body- and neck-hair, even in women."

Now, Dr. Fleure shows that this same group of characters

turns up in patches elsewhere in Europe, particularly in parts of Portugal, Ireland, France, Sardinia, and also in North-East Africa. The skeletal characteristics are also to be found in many of the oldest European skulls of Pleistocene *Homo sapiens*, including those from Brüx, one of the Brünn skulls, Grenelle, one of the Grimaldi skulls and the famous Combe Capelle skull itself, together with several others. Dr. Fleure supposes that this group of characteristics was to be found in the original Aurignacian dolichocephalic population of Europe, of which the best-known example is the Combe Capelle man, and he believes that the type has probably survived without great alterations in these remote districts of Wales, Portugal, etc., and he supposes that from this original dolichocephalic type, the Nordic and Mediterranean races were evolved in different areas. The author gives the following definition of the Nordic characteristics: "Tall, strong-boned, bones often with projections for firmer attachment of the powerful muscles, fair wavy hair, blue eyes, long face, fine profile, strong narrow nose and chin. The skull tends to be long but well filled out and rather finely curved, retaining, however, something of the supraciliary ridges and other marks of some early long-headed types which have contributed to the evolution of the Nordic. Comparison with earlier types brings out especially the better growth of the lateral frontal region." He thinks that in the climatic conditions of the Baltic region there would be an advantage to the possessors of these characteristics. For instance, the lack of pigmentation would mean less radiation from the body in cold weather, and the large nose with narrow nostrils would mean that cold air could not flood into the lungs in undue quantities, and that it would tend to be warmed as it passed down into the chest. Dr. Fleure's argument here is, if sound, of great significance, because it implies that the conditions under which the Nordic type was evolved were so severe, and the action of natural selection was so keen, that trifling differences of pigmentation and small modifications in the form of the nose meant in many cases a difference between life and death.

On the other hand, a different set of conditions in the South-West of Europe brought about the evolution of the Mediterranean race. Owing to the warmth and settled life, maturity was reached earlier, and there was an earlier cessation of growth. There was a lesser development of bone and muscle. "The filling out of the skull and its vaulting are not so marked as in good specimens of the Nordic type. The nose is moderate and variable, but narrower than in the older types, though not so large or, usually, so narrow as in the Nordic peoples." The eyes and hair are, of course, dark.

The chief interest of the paper is perhaps in the indications which it gives that these trifling physical characteristics have been evolved under the stress of different climatic conditions, and, as already mentioned, the facts appear to witness to the surprisingly keen edge possessed by the pruning knife of natural selection. The paper does not deal with any of the brachycephalic types.

In the same number of the *Journal* there is a paper by F. G. Parsons entitled " The Colour Index of the British Isles." In this discussion of nigrescence in the British Isles, Parsons is, of course, following up the foundation work of Beddoe, but he does not adopt the same scheme of estimating pigment as Beddoe used. His scheme allows due weight both for colour of eyes and for colour of hair, and his results show the importance of tabulating males and females separately. He divides the colour of hair into five categories, namely, red, fair, brown, dark brown and black. Parsons says that, in making rapid estimates of the colour of eyes, it is necessary to distinguish between light, dark and neutral ; but that where it is possible to make careful observations, the neutral category becomes unnecessary. Parsons obtains his index of nigrescence by adding in any given case the number of people with dark brown and black hair to the number of people with dark eyes (half the neutral eyes being classed as dark), and then dividing the result by two, and expressing this final result as a percentage. The paper includes a large number of indices of nigrescence obtained in this manner. The figures show, as might be expected, a very wide range of variation, some of the indices being below twenty and others above seventy-five. There is a very marked increase of nigrescence as we pass from Eastern England into Wales ; and a similar, though less marked, increase as we pass from Eastern to Western Ireland. There is a regular and somewhat marked excess of nigrescence in the female sex as compared with men ; and except in very dark areas, the rural population is fairer than the urban population. Parsons finally gives figures to show the distribution of red hair, which is most common in Scotland and the North, and is also commoner in the upper classes than in the lower classes.

The same number of the *Journal* includes a paper by Mr. J. Reid Moir entitled " On the Occurrence of Flint Implements of Man in the Glacial Chalky Boulder Clay of Suffolk." Mr. Reid Moir believes that the flints he describes in this paper are probably of an Early Mousterian type, and, if this be the case, the facts are in perfect accord with the previous discovery by Mr. Moir of Chellean implements in the underlying Middle Glacial Gravel. The reader may remember that Mr. Reid Moir

correlates the Chalky Boulder Clay with the Rissian (third) Glacial Period, and thinks, therefore, that the Middle Glacial Gravel was laid down in the second interglacial period. All this is of great importance in view of the long controversy in regard to the dating of the pre-neolithic periods. And Mr. Moir's results support Penck's chronological scheme, which placed the Chellean in the second interglacial period and the Aurignacian in the third interglacial period, as against the French chronology, which gave later dates to all the pre-neolithic periods.

On April 20th last Mr. C. P. Brooks read a paper to the Royal Meteorological Society dealing with the evolution of climate in North-West Europe (see *Man*, July 1921). The paper dealt with the climatic oscillations that have occurred since the height of the last great glacial period—the Würmian period. The author divides the time which has elapsed since the climax of the Würmian glaciation into seven climatic phases, and he ventures to give dates to these in years. The first phase is that of the Würmian glaciation itself, from 30,000 to 18,000 B.C. The second phase is that which he describes as the retreat of the glaciers, when the climate was still severe, this phase running from 18,000 to 6,000 B.C. The third he describes as the continental phase. This was from 6,000 to 4,000 B.C. The next he calls the Maritime phase, which was warm and moist and lasted till about 3,000 B.C. The fifth was the Later Forest period, which was warm and dry, and endured from 3,000 to 1,800 B.C. The sixth he names the Peat-bog phase, which was, of course, cooler and damper. The author dates the Peat-bog phase from 1,800 B.C. to A.D. 300. The existing phase therefore dates, of course, from A.D. 300.

The third of Mr. Brooks' phases corresponds to that known as the Ancylus Lake period in Scandinavia, when the Baltic Sea was reduced to a lake. Mr. Brooks' fourth period also corresponds well with what the Scandinavian geologists call the period of the Litorina Sea, when the Baltic had a very much wider opening to the North Sea than it has now. It is very interesting to compare Mr. Brooks' scheme with that put forward by Prof. James Geikie some years ago. It will be remembered that Geikie divided post-Würmian time into five phases: a Lower Forestian, a Lower Turbarian, an Upper Forestian, and an Upper Turbarian, the latter immediately preceding the existing (or fifth) period. Brooks' Peat-bog phase clearly corresponds to Geikie's Upper Turbarian; and his later forest phase is clearly the same as Geikie's Upper Forestian. Brooks' third phase is perhaps the same as Geikie's Lower Forestian; but it is somewhat difficult to correlate

Brooks' Maritime phase with Geikie's Lower Turbarian, because the former is described as warm, whereas the latter was certainly cold. These climatic phases have great importance in the dating of the early archæological periods.

Other notable papers in the above number of the *Journal of the Royal Anthropological Institute* are as follows:

By J. H. Hutton, "Leopard-Men in the Naga Hills"; by A. C. Haddon, "The Outriggers of Indonesian Canoes"; by C. G. Seligman, "Bird-Chariots and Socketed Celts in Europe and China"; by L. H. D. Buxton, "The Anthropology of Cyprus." Also, in the *American Journal of Physical Anthropology* (vol. iv, No. 1), "Age Changes in the Pubic Bones," by T. W. Todd.

MEDICINE. By R. M. Wilson, M.B., Ch.B.

Comment was made six months ago on the attention now being devoted to the soil in which disease flourishes—as opposed to the seeds of disease, the bacterial flora. An important announcement bearing on this subject was made in June by Professor Collis who, when addressing an audience of medical men interested in industrial hygiene, described some experiments on stone-mason's phthisis. He said that it had been found that silica dust acted as a chemical on the tissues rather than as an irritant on the mucous membrane of the air-passages.

It had further been shown that stone-masons were more liable than other people to Bright's disease. This latter fact lent strong support to the conception of a chemical action by silica, and thus forced on professional attention the possibility of altering the soil to make it more susceptible to bacterial growth.

Evidently if it is possible, by chemical means, *e.g.* silica dust, to make the human soil more fertile as regards the growth of disease it may also be possible to render it less fertile by the use of other chemical agents. Thus, a new meaning is given to the use of drugs and a new line of thought opened up. We may perhaps have erred in seeking so long and earnestly for substances which have a specifically destructive power on bacteria. It may be that, had we searched as well for chemicals or other bodies possessing the power of rendering the body a sterile soil, we should have achieved more than we have done.

The Annual Report for 1920 of the Chief Medical Officer to the Ministry of Health, Sir George Newman, reveals a steady decline in most disease returns. The birth-rate is rising. The death-rate has again fallen and stands now at 12·21 per 1,000 living. In the decade 1901–1910 it was 15·4. The principal causes of death were organic heart disease, diseases of the ner-

vous system, cancer, bronchitis, pneumonia and tuberculosis. The following table illustrates this :

Disease.	Proportion of deaths per 1,000 deaths from all causes.
Organic heart disease . .	105
Disease of the nervous system and special senses . .	102
Cancer	94
Bronchitis	82
Pneumonia	80
Tuberculosis of lung (phthisis)	72

The point must be noted, however, that the term "organic heart disease" covers many conditions which, primarily, may have no connection with the heart, *e.g.* rheumatic fever and syphilis. The infant death-rate has shown a remarkable decline from 128 per 1,000 births in the decade 1901–1910 to 80 in 1920. On the other hand, the illegitimate infant death-rate is 155—a sad commentary on the position of the "unwanted child."

Women are still dying in large numbers in childbed, the figure for 1920 being 4,144. As many as 1,730 of these died from puerperal fever—a preventable disease. Encephalitis Lethargica seems to be on the increase. But, though there have been sharp outbreaks of diphtheria and scarlet fever, the virulence of these infections seems to be declining.

Tuberculosis, and especially phthisis, is declining fast. Figures of notifications show a steady decline since 1912, and even in 1920 there was a fall of 4,500 on 1919. The difference between the figures for pulmonary tuberculosis in 1920 compared with 1917 shows a reduction by over one-sixth. The total number notified is the lowest by far since notification began. A substantial fall has also taken place in the number of deaths.

ARTICLES

THE SIGNIFICANCE OF SPECTROSCOPY

By HERBERT DINGLE, D.I.C., A.R.C.Sc., B.Sc.
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THE advance of spectroscopy during the last few decades is one of the most striking movements in the vast campaign of physical science. It is not so very long since the spectrum was, if not a disregarded, at any rate an insignificant unit in the battalions of material phenomena. To-day it is hardly an exaggeration to say that it dominates the situation. We no longer look for light to be shed on the structure of matter. We examine the light that matter itself emits, and find there, as well the secrets of the stars as the path to the knowledge of the ultimate constituents of the atom.

Spectroscopy originates in the general subject of radiation, and this in turn arises out of the interaction of matter and ether. For, despite many apparent inconsistencies, we have as yet no better working conception of the physical universe than that of a number of material objects swimming in and interpenetrated by a boundless sea of ether. It is in terms of this conception that we can best approach the question of radiation. It is supposed that every fragment of matter that exists—presumed to be above the absolute zero of temperature—is ceaselessly sending out energy into space. This energy is transmitted by the ether in the form of waves, and is either absorbed by other pieces of matter or goes on travelling to eternity. The higher the temperature of the radiating body, the greater is the rate of radiation. We shall see shortly that not only the rate, but also the kind of radiation emitted, is dependent on the temperature. Now there are three extremely important characteristics of any one kind of etherial radiation. There is the velocity, V, with which it travels; the "wave-length," λ, *i.e.* the distance between two adjacent crests in the wave-train; and the frequency, n, *i.e.* the number of waves emitted per second—or, which is the same thing, the number of waves passing any fixed point in the medium per second.

We might at first be inclined to suppose that these three characteristics are independent of one another, and that one kind of radiation can differ from another in any or all of them. There are two facts, however, which relate them in such a way that a wave is completely known when a single quantity is given. In the first place, we see that the product of the wave-length and the frequency gives the total length of the wave-train passing a given point per second—or, in other words, the velocity of the waves—so that $V = n\lambda$. Secondly, it has been found that, in the free ether, all waves travel with the same velocity, so that $n\lambda$ is a constant—or, in other words, the frequency is inversely proportional to the wave-length. It follows that we have only to state the frequency or the wave-length of any kind of radiation in order to define it completely. We can regard etherial radiation, therefore, as a process of transfer of energy, the energy being stored in a number of wave-trains of different frequency or wave-length, just as a load of furniture can be conveyed from one place to another in vans of different sizes.

It is in the separation of the different kinds of radiation, and their subsequent examination, that spectroscopy finds its *métier*. Spectroscopists now employ more than one means of separation, but it will be sufficient for our purpose to consider the historic method by which Newton first analysed sunlight. We have said that all waves travel with the same velocity in the free ether. The conditions are different, however, when the ether is not free but is in the interior of a transparent material body. In these circumstances, the velocity of all the waves is reduced, and the shorter the wave-length, the greater the reduction. Let us suppose, therefore, that a composite beam of radiation is made to pass through a block of glass. The reduction of velocity will declare itself by a bending, or refraction, of the radiation, the shorter waves being bent more than the longer ones. The directions of the different waves in the glass will therefore depend on the wave-length, and, on emerging from the glass, the beam will have been analysed into its components. It is just as though our furniture vans, which at first travel together along the open country roads, enter the busy street of a town. The larger vans are able to force a passage through, while the smaller ones suffer more delay from the traffic, and fall behind. On reaching the country beyond, they will have been separated from their companions. In practical work, it is customary to cut the analysing substance into the shape of a triangular prism, and to allow the radiation to enter it after passing through a narrow slit parallel to the refracting edge of the prism. In this way, the analysed beam, or "spectrum," consists of a number of images of the slit,

placed parallel and side by side, each corresponding to radiation of a particular wave-length.

It would seem at first that a mere difference of wave-length, or frequency, would be of slight importance, and that radiation of all kinds would have approximately the same properties. As a matter of fact, this is very far from being the truth. The differences between radiation of different kinds are so much more obvious than the resemblances, that more than once a great deal of investigation has been necessary to determine whether observed phenomena have been due to etherial radiation or not. The longest waves that have yet been detected are those employed in wireless telegraphy. Proceeding along the spectrum in the direction of shorter wave-length, we come next to the " infra-red " waves which, in most terrestrial sources of radiation, have the maximum heating effect. Still further down the scale we come to the visible spectrum, beginning with red, and proceeding through orange, yellow, green, and blue to violet—the colours of the rainbow. The visible spectrum is but a very small part of the whole range of known radiation. Beyond the violet we get the " ultra-violet " waves, which make no obvious effect on our senses, but are readily detected by a photographic plate. Beyond these again we get the extremely short X-rays and γ-rays from radioactive substances. The range of wave-lengths now known extends from over one thousand centimetres, for the wireless telegraphy waves, to about one thousand-millionth of a centimetre, for the γ-rays of radium, and it is possible that there is a continuous gradation from one of these extremes to the other.

Let us now see what happens when a body—say an iron poker—is gradually heated. Suppose that at first the poker is at a constant temperature equal to that of its surroundings. Then the radiation it emits will be of the same kinds, and will be discharged at exactly the same rate, as that which it receives from neighbouring bodies. Now let heat be supplied to it. It will commence to radiate more quickly, and with our spectroscope we can analyse the radiation at different stages of the heating. We shall find that, as the temperature rises, two things happen. First, the quantity of radiation of every kind emitted increases continuously ; second, shorter and shorter waves make their appearance as the heating process goes on. The first of these effects accounts for the increasing amount of heat we receive when we place our hands near the poker. The second explains why, after a time, the poker acquires a dull red colour : it has reached a temperature at which it can pour out radiation short enough to stimulate our sense of sight. At a higher temperature, the shorter orange and yellow waves appear and dominate the red, so that the poker successively

assumes those colours. Next, the green and blue waves arrive, but since the red and yellow are still there, the poker does not appear, green and blue, but white. Then, at a still higher temperature, we get violet and ultra-violet waves. The spectrum now appears as a regular, uninterrupted band of radiation, of which a small part in the interior is visible as a strip of brilliant colours. A spectrum of this kind is said to be "continuous." It may be obtained from any solid or liquid body whose temperature is sufficiently high. But now let us heat our poker still further, until it is vaporised. To do this we make it one of the poles of an electric arc. If we now examine the spectrum of the flame surrounding the arc, we shall find that it is no longer continuous. It is broken up, as it were, into a large number of lines, of various grades of intensity, with dark spaces between. This means that, instead of having radiation of all wave-lengths, we are getting only the particular wave-lengths represented by the line images of the slit of our spectroscope. This spectrum is called the "flame" spectrum. If we examine the inner core of our arc, instead of the outer flame, we find now that, corresponding to the higher temperature, we obtain a spectrum similar to that of the flame, but containing more lines. This is the "arc" spectrum. If, finally, we pass an electric spark between two pieces of our poker, the radiation we obtain gives yet another spectrum—the so-called "spark" spectrum. It is similar to that of the arc, but some of the lines in the latter are strengthened while others are weakened, and new lines make their appearance. These new lines, and those which are strengthened in passing from the arc to the spark, are usually called "enhanced" lines.

Now what is the explanation of this variation of the spectrum with the conditions of production? In general terms it is quite simple. Radiation arises from processes taking place inside the atom or molecule. A radiating atom may be compared to a peal of bells, capable of giving certain notes and no others. But if those notes are to be heard separately and distinctly, the bells must have free and uninterrupted play. The atoms of a solid or liquid body are not in a state to allow of this. They are always well within one another's sphere of influence, so that the mechanism responsible for the radiation suffers violent disturbances, and instead of the sharp, clear lines characteristic of the free atom, we get a confused medley of radiation of all kinds, "like sweet bells jangled, out of tune, and harsh." The result is a continuous spectrum. But when we vaporise our substance, the conditions are quite different. The atoms and molecules of a gas have a certain "mean free path," in which they are practically unaffected by their neighbours, and they can therefore emit their characteristic radiations

without let or hindrance. As a consequence, we get a line spectrum. The differences between flame, arc, and spark spectra must be ascribed to changes in the radiating mechanism, arising from the varied treatment it receives. We might say, for example, that certain of our bells need the strong stimulus of the spark to set them in vibration, while others respond readily to the gentler influence of the flame.

We have so far spoken of discontinuous spectra as "line spectra," meaning thereby that they show a number of separate lines instead of a continuous strip of radiation. The term "line spectrum," however, has usually a more limited application than this. There are some spectra containing what appear at first to be a number of short, continuous portions, known as "bands." The bands are sometimes sharply defined at one or both edges, and sometimes gradually fade away. On high magnification they are found to consist of a large number of closely packed lines, arranged with evident regularity, and approaching indefinitely close to one another as a sharp edge or dense central portion is approached. Spectra containing structures of this kind are known as "band spectra," and are thereby distinguished from the "line spectra," which comprise all other discontinuous types. It seems probable that bands are produced by molecules, while lines are due to the workings of separate atoms, so that there is a real difference between them. To summarise, then, we may classify the spectra we have considered as continuous and discontinuous; the latter are subdivided into band and line spectra, the line spectrum of any one element being dependent on the conditions—flame, arc, or spark—under which it is produced.

All these different types are classed generically as "emission spectra," because they are obtained from radiation emitted by the body in which they originate. There is another kind of spectrum, however, which is due, not to emission, but to absorption. If we place a transparent or semi-transparent substance, such as didymium glass or iodine vapour, in the path of a beam of continuous radiation, certain waves are absorbed by it, and the resulting spectrum shows dark lines in the places corresponding to those waves, on a continuous background due to the unabsorbed radiation. Absorption spectra produced in this way are characteristic of the absorbing substance. An important type of absorption is that due to vapours which are themselves incandescent. If such a vapour be traversed by a continuous beam from a hotter substance, the absorption lines in the spectrum will correspond exactly to the emission lines which the absorbing substance would give if it were acting alone. We shall see shortly that this phenomenon has an important bearing on the interpretation of solar and stellar spectra.

We are now in a position to consider what deductions can be made from a spectrum with regard to the nature of the substance producing it. Let us turn first to the continuous spectrum proceeding from a solid body. Inasmuch as precisely similar continuous spectra are yielded by all hot solid or liquid bodies, it is clear that, from the point of view of chemical analysis, they are of no use whatever. Nevertheless, they have one property which is of considerable significance. The spectrum, it must be remembered, is an ordered array of waves of different lengths, carrying energy from the source of radiation to the body on which the spectrum is received. We have said nothing as yet as to the manner in which the total quantity of energy is distributed among the different waves. Investigation shows that this question of distribution is of very great importance. It is not a difficult matter to determine, in any given spectrum, in what waves the greatest amount of energy is stored. When this is done for spectra emanating from bodies at known temperatures, it is found that the length of those particular waves, multiplied by the absolute temperature of the source, is a constant quantity. We have, therefore, only to find the wave-length of maximum energy in any continuous spectrum in order to discover the temperature of the radiating body. To continue our former simile, we might say that, though our furniture vans, being of all possible kinds, give us no clue to the identity of the sender, yet, by the manner in which the furniture is distributed among them, we can estimate his degree of agitation. This method is of great use for the determination of very high temperatures, or of temperatures which cannot be measured directly, such as those of the stars.

The continuous spectrum, therefore, is not without meaning. But the greatest revelations proceed from the line and band spectra. It is there that spectrum analysis has its roots. This exceedingly useful process depends on two fundamental principles. First, the spectrum given by the vapour of any substance is absolutely characteristic of the substance, and can arise from nothing else; second, the spectrum varies with the conditions of excitation, but still remains characteristic of its chemical origin. Furthermore, if any number of substances are mixed together and vaporised, the lines of all of them appear in the spectrum when the appropriate exciting agency is employed. The advantages of this method of analysis are obvious. The whole of the investigation is carried out by a single operation, and, moreover, it is not necessary for the unknown substances to be seen or handled. The presence of hydrogen in a star millions of miles away is demonstrated as clearly as though the gas were captured and imprisoned in a test-tube

in our laboratory. All we require is that it shall send us light. Spectrum analysis is not, it is true, a complete substitute for chemical processes. Quantitatively, it gives, at present at any rate, only a rough approximation to the truth. But, so far as its scope extends, it is of inestimable value. Nor are its uses entirely chemical. The distinction between flame, arc, and spark spectra is a very fundamental one, and leads us to a knowledge, not only of the elements producing a spectrum, but also of their physical state. The most extensive applications of this distinction are astronomical. It forms the basis of a theory of evolution of the stars, and has already given us a great deal of information concerning the physical condition of the sun at different depths below its surface and in the interior of sunspots. The spectrum is, in fact, the most profoundly inspired utterance of the celestial bodies that has yet descended to us, and the secrets it has revealed, incredible as they would have appeared a hundred years ago, impress us to-day even more as an earnest of greater revelations to come than with a sense of their own intrinsic wonder.

It is interesting to note that the great majority of astronomical spectra are due to absorption. The solar spectrum, for instance, consists of a continuous background crossed by a large number of fine lines and bands—the Fraunhofer lines. As a result of our laboratory experiments, there is no difficulty in interpreting this. The sun consists of a central source of continuous radiation, surrounded by a cooler envelope of luminous vapours, which by themselves would give a bright spectrum coincident with the dark lines observed. The constitution of this outer envelope may therefore be inferred by comparing the dark lines with the emission lines of various substances produced terrestrially. As a confirmation of this reading of the spectrum, we can actually obtain the bright lines, with a dark background, by analysing only the light from the sun's edge. A solar eclipse offers very favourable circumstances for the observation.

We have compared a radiating atom to a peal of bells, representing each wave-length by a musical note. It is natural to ask whether the waves emanating from a particular atom are a haphazard, chaotic collection, or whether there is any regularity in them corresponding to melody. To answer this question, we turn to the simplest spectrum known, namely that which is given under certain conditions by hydrogen. As long ago as 1885 Balmer noticed that the wave-lengths (λ) of the lines in this spectrum could be calculated with very great accuracy by giving m the values, 3, 4, 5...... in the formula $\lambda = 3646{\cdot}13 \cdot \frac{m^2}{m^2 - 4}$. The unit of wave-length here is the "angstrom," or "tenth-metre," which is equal to 10^{-10}

metre. The spectrum appears as a series of lines, getting closer and closer together as the wave-length diminishes, and containing no lines of wave-length less than 3646·13. The figure 3646·13 is called the " limit " of the series. The intensity of the lines falls off with decreasing wave-length, so that it is not possible to observe many more than 30 of the infinite number of lines that the formula suggests. It has been found useful to modify Balmer's formula so as to obtain an expression for the " wave-number " of the lines instead of their wave-length. The wave-number (ν) is the number of lines per centimetre, and is obtained by dividing the wave-length (in angstroms) into 10^8, or by dividing the frequency by the velocity of light in free ether. In these terms the formula becomes $\nu = 27418{\cdot}75 - \frac{109675}{m^2}$. There is no other spectrum containing quite so simple a series as this, but patient investigation has shown that the majority of line spectra are largely built up by the superposition of a number of series of the same general form. The essential characteristics of a spectrum series seem to be that it has a maximum wave-number (its limit), from which the various members of the series can be calculated by subtraction of a variable quantity, sometimes called the " term " ; that the lines converge uniformly to this limit ; and that the intensity of the lines decreases as the limit is approached—though this last quality appears to be not without exception. In some spectra, each member of a series consists of two, or even three lines, instead of one—which, by an extension of our illustration, we might liken to musical chords, giving us harmony as well as melody. There are additional regularities attached to the separation of the components of such doublets and triplets, but otherwise the series they form have the same general features as the singlet series. Attempts have been made to represent the wave-numbers of spectral series, other than that of hydrogen, by a generalisation of Balmer's formula, but they cannot be said to have been completely successful. The lines are sometimes represented approximately by adding a constant a to the variable m, and still better representation is obtained by changing a into $\mu + \frac{a}{m}$, where μ and a are constant. The resulting formula, which seems, on the whole, to be the most satisfactory yet suggested, is then $\nu = A - \frac{N}{(m + \mu + \frac{a}{m})^2}$, from which the lines are derived by inserting consecutive values of m. Here A is the limit of the series, and $\frac{N}{(m + \mu + \frac{a}{m})^2}$ is the term. It is

a striking fact that, for all series proceeding from electrically neutral atoms, N has approximately the same value—109675. It appears to be almost a constant quantity for all elements, and is called the "series constant." This number, and the limits of the series, are, however, the only quantities in the general series formula that can be said to be known with any confidence, and the formula is, perhaps, best given as

$$\nu = A - \frac{N}{[f(m)]^2},$$

where $f(m)$ is some function of m as yet undetermined. Another fact of great importance is that the limit of a series is itself a term of another series in the same spectrum. We can therefore generalise our conception of series somewhat by saying that, associated with a given spectrum, there are a number of terms, and that the wave-numbers of the lines in the spectrum are differences of pairs of these terms. When the greater term of the pair is common to a number of lines, the lines form a series, with that term as limit.

The detection of all this regularity, in what appears at first to be a meaningless confusion of lines, has a profound bearing on the problem of the final structure of matter. For the spectrum is an atomic phenomenon, and its order and arrangement are the outward and visible expression of the order and arrangement of the fundamental material units. The spectrum is, in fact, a housetop, on which are proclaimed the workings of the secret chambers of the atom. Unfortunately, the proclamation is made in a language which we have hardly yet begun to understand—which seems, indeed, to express ideas for which the tongue of classical mechanics has no equivalent. The spectrum analyst, with an ear for style, can identify the herald, but he has no faculty for translation. Nevertheless, of late years, a few fitful shreds of meaning have been discovered to us. A word here and a word there, put together and pondered over, have conjured up to the mind's eye the picture of an atom, beautiful in its simplicity, but, withal, so opposed in its workings to the tried and trusted laws of mechanics that, were it not for its almost uncanny harmony with the facts of the spectrum, and suggestions of similar violations of mechanical laws coming from other branches of physics, it would be laid to rest at once among the many fond delusions with which the too speculative have sought to fathom the mysteries of the universe. We are led to conceive of an atom, containing a central nucleus possessed of a net positive electrical charge, round which revolve a number of electrons whose total charge just balances that on the nucleus. The spatial dimensions of the nucleus are negligible, but its

mass is almost equal to that of the whole atom, the mass of the electrons being comparatively of little account. One element is distinguished from another merely by the amount of the charge on the nucleus—and, as a consequence, by the number of electrons and the mass of the atom, since the mass is supposed to be purely electro-magnetic. Hydrogen would have unit positive charge and one electron; helium, two of each, and so on, an increase of one unit marking an advance from one element to the next in the periodic table. Now according to classical mechanics, electrons revolving round a centre should radiate energy, and this is quite in accordance with what atoms are believed to do. But the great difficulty to be overcome is that, as a result, the orbits should gradually become smaller and smaller until at last the electrons fall into the nucleus and the atom ceases to exist. Moreover, as the orbits shrink, the electrons should revolve faster and faster, so that if we suppose—as seems natural—that the frequency of the waves emitted is equal to that of revolution of the electrons, we cannot understand the sharpness of the spectrum lines, which tells of a very high degree of constancy in the frequency. So long as we hold the laws of Newtonian mechanics to be inviolable, there seems to be no possibility of escape from this difficulty. We have, to some extent, therefore, to abandon those laws. The departure is made by two distinct suppositions. In the first place, we suppose that the electrons are compelled to travel in certain defined orbits, separated from one another by finite distances, and that in no circumstances can they possibly revolve in the spaces between. In the second place, we assume that, while an electron is revolving in any one of those orbits, it radiates no energy, but that in other respects the motion is in accordance with classical laws. We can give no explanation of these requirements. The spectrum, if we have understood it aright, issues an imperious demand that they shall be granted, and condescends to no further parley. Starting, then, from these premises, we find that radiation takes place when an electron changes from one of the possible orbits to another. The energy of the electron in the stable state is a function of the radius of its orbit. Consequently, when the orbit is changed, say to one of smaller radius, there is a balance of energy left over. It is supposed that this energy is radiated as a wave whose frequency is proportional to its measure. Here again, if we attempt to picture the details of the process by which energy of revolution becomes transformed into wave energy, we are quite at a loss. Energy of revolution vanishes, energy of wave motion appears, and we simply equate them, making an arbitrary assumption about the frequency.

The remarkable thing is that, with the hydrogen atom containing only one electron, the radiation calculated from these speculations agrees absolutely with the observed spectrum, and gives Balmer's formula completely, with variations not exceeding the probable errors of observation. Moreover, the supposed atom has made more than one remarkable prediction, which experiment has verified up to the hilt. We can understand now how it comes about that a wave-number appears as a difference of two terms. According to the theory, each term is proportional to the energy of an electron in an orbit, so that the difference of two terms, like the wave-number, is proportional to the difference of the corresponding quantities of energy. It is unfortunate that the mathematical difficulties connected with the problem of three bodies prohibit us from investigating the possible radiation from an atom containing more than one electron and a nucleus. We are, therefore, almost entirely restricted to hydrogen for detailed tests of the theory. Almost so restricted, but not quite. The helium atom has two electrons, but if, by any means, we can abstract one of them, the remaining system is amenable to investigation, and is, in fact, just like the atom of hydrogen, except for the double charge and correspondingly greater mass of the nucleus. The radiation to be anticipated from such a system has actually been obtained by passing very strong electric discharges through a tube of rarefied helium, and the complete agreement of prediction and observation, even to the small modification of the series constant for helium, is one of the reasons why it is felt that, despite its apparent incredibility, the theory must lie very close to the truth. It appears that the spectrum of an atom which has lost one electron —the "ionised" atom, as it is termed—is what we have called the spark spectrum, and the enhanced lines are the lines produced by passages from one to another of the new orbits made possible by the resultant positive charge on the atom. Almost daily, fresh evidence pours in, supporting on one side or another this remarkable conception of atomic structure, and though at present it seems difficult to deduce from it the facts of chemical combination, no contradiction is involved, and it may well be that, with fuller understanding of its implications, and perhaps with some modifications of its details, its scope will soon be as striking as its success in the minutiæ of spectrum phenomena. In any case, it has pointed the way to more than one discovery, and in fertility of suggestion and guidance in clear thinking, it leaves the spectroscopist little to desire.

Keats once complained that Newton had destroyed the mystery of the rainbow by reducing it to the prismatic colours.

The spectrum, as Keats knew it, may have been tongueless, and less inspiring as a straight strip than as a celestial arch, though it is difficult to understand how it could ever have been shorn of its mystery. But, with our present knowledge and the endless avenues into the Unknown which it has set before our eyes, who can deny that Newton, by relating the rainbow to the eternal, universal realities of the spectrum, has invested it with a deeper, with a far nobler mystery than any that could reside in the impossible fires of the poet's imagining? For mystery, when all is said, is the latest word of science. As the sea of knowledge extends, its waters go deeper, and farther beyond the soundings of our finite plummets. Spectroscopy is far from the shore, but the boundless ocean is before it.

SOME IMPLICATIONS OF THE CHROMOSOME THEORY OF HEREDITY

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THE history of science shows us that new ideas generally gain acceptance not so much by defeating the old in controversy, but by supplanting them—by competition, not by battle.

The divergences of opinion between Professor MacBride and myself, as expressed in the correspondence section of the last issue of SCIENCE PROGRESS, have reimpressed this truth upon me. Controversy is really of little use in settling such disputes. Far better for each party to put forward their own positive construction, and let the world decide between the two presentations.

Professor MacBride has already set forth his views on the subject (SCIENCE PROGRESS, Jan. 1921); I would crave indulgence to bring forward some of the general implications of that alternative theory which advances furthest in the opposite direction—the chromosome theory of Morgan and his school, which, combining the Mendelian theory of segregation with the mutation hypothesis, grafts both on to a cytological basis.

I shall endeavour to summarise very briefly the chief lines of evidence upon which the hypothesis is based, and then to point out some of the implications of the theory which are usually not dealt with in the textbooks on the subject: Throughout, I shall have to assume acquaintance with the ordinary facts of genetics and cytology as they may be found set forth in standard works such as Punnett's *Mendelism*, Baur's *Einführung in die experimentellen Vererbungswissenshaft*, Doncaster's *Cytology*, etc.

In the first place, then, we have the well-known facts of Mendelian inheritance. Most of these can be explained on the hypothesis introduced by Mendel himself, that visible characters of the adult organism were in some way represented in the germ from which it arises by discrete units, each one independent of the others. These units, usually styled unit-factors, or more briefly genes, are normally present in pairs; but the members of a pair separate from each other at some

period before the formation of the gametes or sexual cells, so that each gamete contains one or other member of each pair.

Later work has extended, or, as some will prefer to think, restricted, this generalisation, by showing that the unit-factors are not necessarily all independent of each other. As a matter of fact, when organisms are thoroughly studied, and a great number of their Mendelian variations genetically investigated, it is found that the factors concerned fall into a series of associations, the linkage-groups. A factor belonging to one linkage-group is completely independent in heredity of factors belonging to any other linkage-group. But the members of one and the same linkage-group are not independent of each other. On the contrary, they tend to stick together in the course of transmission from one generation to the next. When a cross is made involving two or more linked characters in each parent, the combinations carried by the parents tend to recur more frequently in the offspring, instead of all possible recombinations being produced in equal numbers, as will happen when the factors involved are not linked.

Further, linkage may be of differing intensity. Factor A may be strongly linked to B, but weakly to C; but, in the same conditions, the intensity of linkage between any two particular factors remains the same. The number of linkage-groups appears to be constant for any given species.

So far, then, the facts elicited by experimental breeding permit us to make the following generalisations. In the higher animals and plants, many characters are represented in the hereditary constitution of the species by discrete units, or genes, whose segregation from each other and whose arrangement in a series of linkage-groups gives us the various ratios found in genetic experiments.

The next question is whether *all* hereditary characters of organisms are in some way or other represented by these genes. On this point, as on many others throughout this paper, I shall be unable to present the available evidence. I must be content to state my position dogmatically, and let time and the critical judgment of the reader pronounce upon it. Personally, then, it appears to me, in common with a number of others, that certainly the majority, and possibly all, characters are due to Mendelian genes. There is no evidence which at present definitely contradicts this view, and, until there is, "William of Occam's razor" makes it preferable for us to adopt, as covering all cases, the one hypothesis for which there is tangible evidence. One fact should be pointed out—that a great deal of early Mendelian work was done on striking characters which are to be considered abnormal, or even pathological. As the analysis is pushed home, however, it has been found that a great

many normal variations are also inherited in the same way, although their investigation, because of their slight deviation from the normal, is attended with far greater technical difficulty. As a working hypothesis, then, let me propose this—that the hereditary constitution of an organism consists of a large number of genes, or Mendelian unit-factors.

Let us now turn to another field of work altogether, that of cytology. If we examine the cells of organisms microscopically, we find that they contain bodies known as chromosomes. Their number is constant for a given species, and they exist normally in homologous pairs, the members of which separate from each other at some period prior to the formation of gametes. A gamete thus gets one or other member of each pair of chromosomes. Not only this, but, in the (very few) species where the evidence permits a statement, the number of linkage-groups is also the number of pairs of chromosomes. Thus, if Mendelian genes were carried in the chromosomes, we should be given in their behaviour the physical basis for ordinary Mendelian ratios. Further, supposing that each chromosome carries a great number of factors, the phenomena of linkage will be explained if each chromosome represents a linkage-group, and if the intensity of linkage varies inversely with the linear distances of genes along the chromosomal framework. This is Morgan's hypothesis, and the full evidence for it, with certain special points of detail, may be found in his recent book, *The Physical Basis of Heredity*.

The evidence, direct and indirect, for the chromosomes as the bearers of the genes is very varied. The strongest support is to be found in the facts of sex-linkage, where the observation of a different ratio from the normal Mendelian one has been accompanied in many instances by a discovery of a chromosomal difference which will account for the observed divergence of ratio; in the similar facts of non-disjunction, which appear to constitute an *experimentum crucis* designed by nature (see Morgan's summary in his *Physical Basis of Heredity*), and by the very interesting cases in which organisms have been found with double the ordinary number of chromosomes, whereupon the Mendelian ratios obtained are such as might be expected from the random distribution of four instead of two chromosomes to the gametes (Müller, *Amer. Nat.* **48**, 1914; Blakeslee, Belling, and Farnham, SCIENCE, **52**, 1920, p. 388).

There are many other lines of evidence, but in this paper we are deliberately taking on trust the bulk of the facts to be found in standard books.

Granted, then, for the sake of argument, that Mendelian genes are represented by units in the chromosomes, have we

the right to say that the hereditary constitution of an organism is entirely represented by these chromosomal units? To this we must answer that we do not as yet know, but that, again, there is no fact which contradicts that assumption.

It may be immediately objected that in the cytoplasm of the fertilised egg we find at least two other sorts of differentiation, both proved to be of importance for the process of development, both obviously inherited in the simple sense of that word. There is, first, a polarity, or gradient, stretching from the more protoplasmic and active animal pole to the more yolky and less active vegetative pole; and, secondly, there is often an accumulation of definite substances—" organ-forming stuffs "—in definite regions of the egg, which have in many cases been shown to play an important rôle in early development, certain regions or structures of the embryo not being formed in the absence of certain of the substances (Jenkinson, *Experimental Embryology*, p. 223, etc.).

It must, however, be remembered that the ovum is not a simple undifferentiated cell, but highly specialised; its polarity, or axial gradient, appears probably to be due to the position it has occupied during growth in the body of the parent (cf. Child, *Individuality in Organisms*, 1915), and the accumulation of organ-forming substances in definite regions is a process of differentiation comparable to any other process of differentiation in any other cell. This being so, it is clear that the organ-forming substances and their localisation may be properly thought of as formed under the influence of more essential parts of the cell—on our hypothesis, under the influence of the gene-constitution in the chromosomes.

If this is so, then such differentiations, like the subsequent differentiations of the developing fertilised egg, take place as a result of the constitution of the organism—the genes—in reaction with the environment; only they have been produced precociously, in preparation for the rapid changes necessary in early embryonic existence.

It is, however, obvious that in one sense the cytoplasm is part of the " constitution "—it is necessary to life. A nucleus without cytoplasm cannot exist, and, even if it could, it probably could not form the specific cytoplasm out of itself, or develop into a normal cell. What right have I, then, to speak of the genes in the chromosomes, and not the cytoplasm, as representing the essential constitution of the organism? Both appear to be specific, and both appear to be necessary to existence. Much ink has already been spilt over this question; in order to spill as little as possible myself, I shall introduce at once the conception which I believe we shall eventually come to have of the chromosomal factors. Both cytoplasm and

chromatin are essential to life, and in that sense both parts of the "constitution"; but in all organisms with chromosomes, the chromatin has become specialised to discharge special functions in regard to heredity. The factors of heredity, I would say, are those which are segregated in the gametes; they are so segregated by the activities of the chromosomes in which they are lodged, and they constitute a mechanism which has two distinct functions, both of prime importance. The first function is to act as the self-regulating machinery of heredity; the second, in conjunction with sexual reproduction, is to allow the multiplication and more especially the recombination of variations, and so to afford the possibility of evolutionary change. It is noteworthy that the opinion of biologists in the last three-quarters of a century upon the function of sexual reproduction has oscillated between these two extremes; some have maintained that the effect of sexual fusion of germ-plasms would help to preserve specific constancy, others that it would encourage variability. It is the merit of the chromosome-gene theory of heredity that it enables us to reconcile these apparently incompatible ideas.

The chief method of variation revealed by recent breeding experiments is that known as mutation of single genes; in other words, the discontinuous and definite change of single factors. The amount of the change, as revealed in the visible alterations produced, may be very small or very large; it may apparently be due to a subtraction, an addition, or a qualitative alteration which cannot be classified either as addition or as subtraction. It is a curious irony that the occurrences in the Evening Primrose upon which de Vries based his mutation theory of evolution have finally been shown not to be mutations in the true evolutionary sense at all, *i.e.* they are not due to direct effects of changes in the hereditary constitution, but are recombinations of already existing differences brought about by conditions special to the genus (Renner, *Ztschr. Ind. Abst. Vererb.*, **18**, 1917).

I remember discussing the general question with Morgan in 1912, and saying that, if we were to find a number of very minute mutations of single factors, we should for all practical purposes be back again in the old Darwinian position, with the difference that we possessed information which he and his immediate followers did not, as to the nature, extent, and location of variations, and the mechanism of their transmission. Events have moved in the direction which I thought; the wide occurrence of Mendelian behaviour having been demonstrated on the large and striking variations, which, however, do not appear to be the most important for evolution, attention is now being focused upon smaller mutations, and we are coming

round again to the idea that it is by the combination of many small (but definite and discontinuous) constitutional changes that evolution proceeds.

Further, the original very natural theory that, of two contrasted Mendelian characters, one was due to the presence, the other to the absence, of a whole factor is gradually being abandoned. It is becoming necessary to think of the factors as molecules, or groups of molecules, with the power of self-reproduction, and the process of mutation as a change in one, probably small, portion of the factor (see Morgan, *loc. cit.*).

With this in mind, the function of a chromosome-gene constitution, in conjunction with sexual reproduction, in promoting variability is obvious. The independence of the genes (an independence complete for those in different chromosomes, partial for those in the same chromosome) permits of all possible recombinations of any mutations which may crop up. If two varieties of an asexually-reproducing organism exist which differ from each other in respect of two Mendelian differences, only these two varieties are possible until new mutation occurs. But if the same factor-differences exist in a species with sexual reproduction, they may be recombined to give a possible four varieties. In general, in species without sexual reproduction, if n mutations appear, n varieties are possible; but in one reproducing sexually, n^2 varieties are possible for the same number of mutations. If one of these combinations should happen to confer upon its possessor an advantage in the struggle for existence, it will be able to establish itself in a very short space of time. This is shown in the rapid spread of such forms as the dark variety of the Peppered Moth, and its speed can be calculated theoretically (see Bateson on *Problems of Genetics*, 1913, pp. 135–40; and Norton's appendix in Punnett's *Mimicry in Butterflies*, 1915). Where the number of factors is large, the possible recombinations are of enormous number. This was beautifully shown in Baur's cross of two species of Antirrhinum (Baur, *Zeits. Ind. Abst. Vererb.*, **21**, 1919); in the second generation, out of ten thousand plants, it was hardly possible to find two alike; and the process of segregating new (and, be it noted, for the most part healthy and "normal") types continued on the grand scale in later generations. It is important to notice that in this cross the first generation hybrid was uniform. This uniformity of the first generation followed by great diversity in later generations is of course what is to be expected on Mendelian principles, ***and on no others***; this experiment, and others like it, may be regarded as establishing that the differences between species are due, either wholly or in very large part, to Mendelian genes. Further confirmation of this idea as regards local races and

geographical varieties—*i.e.* incipient species—is afforded by most of the recent work on variation in the field. Mention need only be made of most of the instances quoted by Bateson in chap. vii of his *Problems of Genetics*, and further of Crampton's monumental work (*Carnegie Publication*, No. 228, 1916) upon the snails of the genus *Partula* in Tahiti. The varieties of these snails isolated in the various valleys were distinguished by differences which he found impossible to correlate with any difference of environment, but which could be readily and simply conceived as due to mutations in a smaller number of genes. Finally, reference should be made to the important review by Sturtevant on the North American species of Drosophila (*Carnegie Publication* No. 301, 1921), in which he shows that the characters separating species of Drosophila are often similar to the mutations found in the laboratory.

It should finally be remembered that this Mendelian recombination has the consequence of producing, according to the laws of probability, the extreme combinations rarely, but a vast number of combinations which exhibit but few differences from the average.

We now come to the other aspect of the problem. Self-regulating mechanisms are, one might say, the hall-mark of life. Whether, as writers like Haldane appear to believe, they are to be considered as the outcome of an inherent tendency of living matter to organic regulation, or whether, as probably the majority of biologists would prefer to hold, they are the outcome of long selection upon living substance with its fundamental power of self-reproduction, does not concern us here. Enough to realise their importance. The constancy of temperature in mammals and birds is usually quoted as the typical example. But much more striking instances may be adduced. The regulation of the acidity of the blood is so accurate that an increase of one part of hydrogen ion in a hundred million millions of blood (1 : 100,000,000,000,000) is responded to by a change in respiratory rate tending to restore the normal concentration. The regulation of the salt-content of the blood by the mammalian kidney is of the most consummate accuracy. The regulation of organic form, as seen in regeneration among the lower organisms, and in the early stages even of the highest, is another example; in normal environment, the organism restores just that part which is removed, and the form-equilibrium is re-established.

The chromosomes and their contained genes may be looked upon as the regulating mechanism which makes heredity, and indeed any real organic constancy from generation to generation, possible at all—the regulating mechanism of species.

Whatever the constitution of an organism may be, it is clearly in the highest degree complex. Organisms are exposed to very various environments. If the rates of reaction of two processes of life are different in different environments, we shall find that the proportion of their end-products are different in the two cases. In some environments it would be easy to imagine that we should get, not merely different proportions of substances, but different substances produced. What, then, is to prevent the altered proportion or the different substance from taking its place in the " constitution of the organism," and being handed on in heredity ?

The chromosomes, of course, only appear as such during certain short periods, especially during cell-division. From all the evidence at command, particularly from the work of Morgan and his school upon Drosophila, but also from evidence upon other organisms, it appears clear that each gene has a fixed and unaltered position. It is lodged in one particular chromosome, and at a particular place in that chromosome. Our latest information upon the constitution of the proteids indicates that they are built up of a number of amino-acids, not only whose nature, but also their position in the proteid molecule, is constant. In the same way, it appears that the chromosome-constitution of an organism consists *essentially* (*i.e.* omitting consideration of framework, etc.) of a number of genes, whose nature is constant (apart from possible mutations), and not only their nature, but also their proportions and their situations.

Irrespective of any changes which may occur during the so-called resting stage of the nucleus, whether these may lead to different proportions of the substances forming the genes, or to a different cytoplasmic constitution, the gene-materials, it would appear, when the time comes for mitosis, join up once more into the chains which we call chromosomes, in the same proportions, the same order as at the last mitosis. The whole series of genes can in this light be looked upon as a single unit, but on a scale of complexity one or two removes above that of the protein molecule ; however, like the protein molecule, it preserves its identity and its constitution unchanged—or shall we say that it *can* preserve it unchanged ? Mutations may occur, either in single genes, or in whole regions of a chromosome, or even whole chromosomes or sets of chromosomes may be duplicated ; but, once these changes have occurred, the resulting unit-complex tends once more to reproduce itself.

From this point of view, then, the gene-constitution of the organism is a mechanism for preserving the character, number, proportions, and locations of a great number of unit-factors ;

and it accomplishes this by being itself a unit of a higher order, whose parts are linked together in a definite and specific way, each of the parts, and therefore the unit-complex as a whole, being self-perpetuating.

If this is so, then we are presented with a piece of chemical machinery which would appear to be admirably adapted for the task of acting as self-regulator of heredity ; for passing on unchanged from generation to generation that raw material, that constitution which in interaction with the environment gives the adult organism ; and therefore for *resisting* the very type of effect which the Lamarckians consider to be operative in evolution.

A chemical mechanism of this sort will undoubtedly prove in the long run to be susceptible to some external agency or other. No doubt, too, we shall some day find out how to break up the atom and liberate its contained stores of energy ; but so far we have not discovered the way. And we have not yet discovered the way to alter the genes at will. If the recent experiments of Guyer are confirmed (Guyer and Smith, *J. Exp. Zool.*, **31**, 1920, p. 171) it appears that some factors can be directly attacked by the methods of immunology. Otherwise, results have been negative. Selection is of no avail. (The only positive results are those of Jennings and of Hegner, in Protozoa (see *Proc. Nat. Ac. Sci.*, **4**, 1918), and these are contradicted by current researches of German workers). Gradual acclimatisation to changes in the chemical constitution of the medium have an effect, but not a cumulative one, and not persisting after sexual fusion (Jollos, *Zeits. Ind. Abst. Vererb.*, **12**, 1914, p. 14). Attempts to raise the upper level of heat tolerance in insects—a type of experiment in which, if anywhere, one would expect to find evidence of functional adaptation—have proved unavailing ; indeed, the rearing of one brood at a higher temperature makes it more difficult, instead of easier, to bring up the next generation at the same temperature (Northrop, *J. Gen. Physiol.* **2**, 1920, p. 313).

Indeed, the more one considers the nature of any such unit-complex of genes as I have endeavoured to sketch, and the more one discovers as to the apparently accidental and certainly very localised process of mutation in single genes, the more difficult is it to imagine any method (save perhaps one, like Guyer's, connected with protein specificity), which will enable us to alter the genes directly. This is not to say that some such method does not in reality exist ; but the difficulty of imagining how a functional adaptation in the soma could be reproduced, in whole or in part, in the next generation, becomes greater and greater the more we analyse the implications of the gene-constitution hypothesis. The functional

alteration must first produce an effect upon the organism as a whole, presumably upon the composition of the body-fluids; this alteration must act upon the germ-cell so as to change the composition of the cytoplasm; this change in the cytoplasm must then act upon the complex of hundreds or more probably thousands of genes in such a way that their alteration is transformed during development into a somatic alteration similar to the one produced by external agency in the preceding generation. But, owing to the fact that the genes are self-reproducing, and to the further fact that they are only transmitted from cell-generation to cell-generation in the form of an aggregate unit, the chromosome-complex, the proportions and locations of whose parts are fixed like those of a complex molecule—owing to these facts, various possible methods of alteration of the gene-complex seem excluded. Alteration of the proportions of its constituent units, the genes, is excluded. Total destruction of one single gene seems excluded; although its inactivation by some specific lysin or agglutinin is theoretically possible. Change, in fact, seems limited to one of two things. Either some side-chain of a gene can be altered so that the working of the gene, while still similar in essentials, is altered in detail: this process is already known to us in the mutation of single genes, and, so far as at present studied, appears to bear no relation whatever, and certainly not a "Lamarckian" one, to any external stimuli that have been tried. The second is a stimulation of the gene to an over-production of certain substances. In view, however, of the transitory nature of acquired immunity, which must be the prototype of any such changes, and of the fact that it is not transmitted beyond the first generation, and then only through the mother, we must regard this as a speculative possibility only, although one which should by all means be explored.

In concluding this section, I should like to draw attention to the great similarity subsisting between the present conception and that held by the majority of "Neo-Darwinians." Our knowledge of the mechanism of heredity and of the origin of variations has been enlarged, but the central concept remains unchanged—the concept that the adaptation and the progressive evolution of living things, below the human level, has been achieved through a natural selection acting upon variations which are biologically fortuitous. There are some people to whom the idea that chance variations provide the raw material of evolution is unpalatable; they find in it a contradiction of their ideas on the governance of the universe, and cannot reconcile it with the notion of progress or of any beneficent spirit in cosmic affairs. Such minds have not even reached the stage of the New England lady who was reported to

Emerson as having proclaimed, " I accept the Universe. . . ." (" Gad ! She'd better ! " was the sage's comment !)

But the real conflict is between the upholders of chance variation and those who believe in the direct action (not indirect, as in Natural Selection), of the environment; or in an hereditary effect of the organism's own efforts, own functioning.

There can be no doubt but that the gene idea is at present the merest approximation to reality ; if it is true, the labours of analysing a unit-complex consisting of several thousand units, each of these composed of one or many molecules of hundreds or more probably thousands of atoms each, will be prodigious. But, if it is any approximation at all to reality, then nine-tenths of any and every Lamarckian theory falls to the ground. Professor MacBride refers to the " factor-mad pupils of Professor Morgan." But, with such a conception opening up before them, a conception of a material unit of exact composition, but of a complexity undreamt-of even in organic chemistry, a mechanism which resists change sufficiently to allow hundreds of thousands of separate species to co-exist upon this globe, preserving their incredible diversity often in spite of similarity of outer environment—perhaps a little enthusiasm, leading to statements which may appear exaggerated, but have not been proved untrue, is pardonable—pardonable especially when it is the outcome of the most laborious experimental research, and not of speculations which for the present appear to be often unverifiable.

There remains the other essential side of heredity: the translation of the constitution, in certain conditions of environment, into the adult organism.

A formal possibility of such development has already been set forth in the writings of His, Roux, Jenkinson,[1] and others. Briefly, if we have a constitution of some complexity, together with any localisation of material or of structure within the germ, whether pre-existent or caused by outer stimulus, we can theoretically understand the production of a gradually increasing complexity in the developing organism.

It will help us if we take a more tangible example. We have already considered the origin of the two types of gross differentiation to be seen in fertilised ova, the localisation of organ-forming substances, and the pre-existence of a polarity or gradient. If, as we postulate, the constitution proper is lodged in the chromosomes, both pre-localisation of substances and polarity being in the long run determined by the environment in relation with the constitution, we shall have the same chromosomal constitution in each and all of the cells into which

[1] See Jenkinson, *Experimental Embryology*, Oxford, 1909, which contains references to earlier work.

the segmenting ovum divides. But this same gene-complex will be lodged in a different environment according as it is in a cell of the animal or vegetative half, according as that cell contains a particular organ-forming substance or not. The same material in different environments will give different end-products. These can then interact with each other, or with other elements of the constitution; one part may be exposed directly to the surrounding medium, as in the ectoderm, another part, like the mesoderm, may be exposed only to an internally-secreted medium.

Given, in fact, any diversity of different regions of the egg-cytoplasm, whether pre-existing or induced after fertilisation, and a formal explanation of differentiation becomes possible.

This is of prime importance to us, for it answers an objection raised by many critics. If the constitution of an organism, they say, is to be sought in the chromosome-gene complex, and if this is divided into identical halves at each mitosis, then every cell in the body will have the same chromosome constitution. How, therefore, can differentiation be possible? Such objectors, often physiologists, are falling into the very error which they pretend to deplore: they are thinking in unreal symbols. When the geneticist affirms that the constitution of an organism is composed of a number of unit-factors, he merely epitomises the ascertained fact of his own experiments. But, in that assertion, he states nothing as to the physiology of their development. When he states that he knows a pair of Mendelian genes, let us say X^1 and X, which determine yellow and agouti colour respectively in mice, he in reality asserts only that, without X or X^1, the mouse will not be either yellow or agouti; with X and without X^1, it will be agouti and not yellow; with X^1, it will be yellow.

If, further, the result of his experiments indicate that these factors are lodged in the chromosomes, this contributes simply another fact for the physiologist to explain. If the explanation be not immediately forthcoming, this is no reason for denying the facts, but only for further thought and research.

As a matter of fact, it should not surprise us in the least to find the identical gene-mechanism in every cell of the body. The mechanism of a dozen Broadwood pianos may be identical, but the melodies elicited from them may and will differ with the executant. The executant is for us represented by the environment, whether intra-cellular, intra-organismal, or external; the construction of the pianoforte by the gene-complex, and the different melodies by the various types of differentiated cells of the adult body; of the genes in any one cell, some will remain dumb, some never sounded; in one cell a gene will be working at full activity, in another partially inhibited, in a

third neutralised by the products of others. Each environment will strike a different combination of genes. This conception, startling as it appears at first sight, is paralleled in other branches of biology. Thus, the secretin produced by the small intestine circulates through the whole body ; but all the tissues remain " dumb " to it with the exception of the pancreas.

From this standpoint, the unity of the organism as a whole is not only explicable, but necessary. Each cell represents a particular state of equilibrium, and the organism as a whole an equilibrium of all the cells with each other and with environment. Remove one type of cell and cultivate it alone, as in tissue-culture experiment ; its type will be altered. Remove part of the whole ; the working of the rest will be changed.

The development of an organism is a series of states of equilibrium, usually of increasing complexity, none fully balanced, but each resolving itself automatically into the next. Finally, the adult state is reached, in which relative stability is assured, either by the narrow limits of the environment, or by elaborate regulatory mechanisms, of which the endocrine glands are the best example.

A great change has come over our ideas on the subject since Weismann's day. Weismann's merit (*The Evolution Theory*, and other works) lay in destroying the old uncritical attitude towards the inheritance of " acquired characters " or modifications ; in formulating the idea of determinants ; and in locating them in the chromosomes, thus rendering the function of mitosis and the reducing division intelligible.

His demerits were his over-emphasis of the distinction (which is none the less usually a real one) between soma and germ-plasm ; and his total failure to construct a physiological theory of development—a failure which led him into an unreal symbolism.

He never tried to think except in terms of determinants. Accordingly, when a salamander regenerated a limb, or a bisected planarian became two perfect animals, he was forced to postulate a whole battery of reserve-determinants, ready to come into play if the accident of injury should befall the organism.

It was soon seen that this, even as the merest formal explanation, was untenable. On the other hand, the idea of a complete self-reproducing gene-complex in every cell of the body is fully adequate, and indeed is the only conception save that of a mystical vital force or of an unanalysed constitution or tendency, which can account for the facts of regulation and regeneration.

After the Planarian has been bisected, the equilibrium of either half is different from that of the original whole. Changes

may therefore occur, and, since the whole battery of genes is present, the missing organs may be restored under new conditions. If we regard the normal form as a condition of balance, it is no more remarkable that what is restored is just what has been taken away, than it is for the form-equilibrium of a damaged crystal to be restored when placed in an appropriate solution.

Innumerable complications exist. Some cell-differentiations appear to be irreversible; whether regeneration shall occur or not is much influenced by age, temperature, size, and other conditions affecting metabolism; de-differentiation to a primitive condition may be necessary before regeneration can occur; and so forth. But the main concept holds. Some examples may clarify it. As is well known, the lens of the eye in Vertebrates is formed from the ectoderm of the head, at the point where the optic cup approaches the skin. If the developing optic cup of a salamander larva be removed and placed under the skin of the abdomen, no lens forms in the accustomed place in the head, but one is produced (see Morgan, *Regeneration*, p. 204) over the transplanted optic cup from the abdominal ectoderm. Professor MacBride finds this a difficulty for the determinant theory, since it presupposes that "lens-determinants" must exist in every cell of the ectoderm. So, presumably, they do; and in every cell of the body too. But only in the ectoderm have the conditions been such as to bring the cells into the state in which lens-substance is ready to be produced; and it will only be produced under the action of a specific substance secreted by the optic cup. That seems the natural way of interpreting the phenomenon. The "lens-determinants," whether one or many, and in any event acting in co-operation with a vast number of other genes, are dumb notes, except in ectoderm-cells struck by the optic cup's specific substance. Elsewhere they do not act; or, more probably, act in different ways to produce totally other results.

For we must guard ourselves rigorously from the error, elementary, yet none the less insidious, of supposing that each gene "represents" some single adult character in the same way that a word may represent a particular object, or the indentations on a gramophone record represent each a special sound. Every character, such as eye-colour, stature, longevity, needs the co-operation of several genes, and each gene probably affects a multiplicity of characters. This has emerged clearly from all recent Mendelian work, and nowhere more clearly than in Drosophila. To give but two examples: the red eye-colour of Drosophila is affected by a large number of separate genes, each producing a characteristic effect; while the factor for yellowness in mice, if present in single dose, not only extends

the yellow band to cover the whole hair, but brings about obesity, while, if present in duplicate, it kills the embryo at a particular stage of its development.

What are these factors, these genes, in reality? The last example will show us that their most obvious visible effects may often be only the signs or by-products of far more deeply seated changes in metabolism. In one or two cases only have we much definite information upon their actual modes of action. The researches of Riddle,[1] of Onslow,[2] and others upon the hair-pigments of mammals indicate clearly that the presence of some factors leads to the production of certain specific ferments, and that different multiple allelomorphs of a single factor may represent different degrees of oxidation, while other factors may lead to the production of substances which inhibit the action of these ferments. Whether these ferments are produced directly by the individual gene, or as the end-result of a long series of reactions in the cell, we have as yet no means of telling.

The experiments of Guyer and Smith, however, indicate that the material of the gene may in certain cases correspond with the material of the adult organ which it helps to determine. These workers injected fowls with ground-up lens of rabbits. The anti-lens serum thus produced in the fowls was re-injected into pregnant rabbits. Not only did the young show the defects in the lens, but most of them transmitted the defect, although in an irregular manner, for a number of generations. In the present state of our knowledge we must assume that the adult lens and the (or a) gene concerned in lens-production possess the same proteid molecule in their constitution, and the injection of anti-lens serum not only damaged the developing lenses of the embryo, but also attacked this lens-gene in their germ-plasm. If these results should be confirmed, it would appear that the lens-determinants normally present throughout the ectoderm, as indicated by the experiments on salamanders, which we described above, normally come into action only in the lens itself, and remain wholly inactive in the rest of the tissues.

The older discussions of Genetics and Evolution were inevitably in large parts theoretical. It is the aim of experiment to make our formal explanations and theoretical generalisations more precise, by a discovery of the actual mechanisms involved, and so to pave the way for prophecy and control. The hypothesis of a gene-complex located in the chromosomes is the first-fruits of the twenty years' experimental breeding which followed the rediscovery of Mendel's laws. It is obviously the merest

[1] *Biol. Bull.*, **16**, 1909.
[2] *Proc. Roy. Soc., B.*, **89**, 1915.

sketch of reality; in all probability it is erroneous in some respects. But it is at least the only hypothesis which allows us to synthesise so many distinct sets of facts in one conception. The Mendelian doctrines of unit-factors and segregation; the cytology of the chromosomes, their constancy of number, their individuality, their mitosis and reduction; the phenomena of linkage and crossing-over; the continuity of the germ-plasm; mutation; the variability of species on the one hand, their relative constancy on the other; the function of sexual fusion; the epigenetic course of development and the restoration of organic form-equilibrium; the hereditary effects observed by Guyer after injection of anti-lens serum; sex-linkage and the basis of sex-inheritance; the abnormalities of non-disjunction and of duplication,—all these can be blended harmoniously in the hypothesis which sees in the chromosomes the bearers of the hereditary constitution of organisms under the form of a definite structural complex of self-reproducing genes.

SOME BIOLOGICAL EFFECTS OF THE TIDES

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FRINGING every continental land-mass is an area, for the most part very narrow, which belongs properly neither to land nor sea, but is the disputed province of both these realms. Although, on a superficial view, it is by no means favourable to life, the tidal zone turns out, on closer examination, to be one of the richest in variety of animal life on the surface of the globe.

Every distinct haunt of life brings its own special problems to be solved by the animals that live there, but the problems which confront shore animals are particularly urgent. The factor which gives the seashore its peculiar ecological character, or rather, from the ecological point of view, makes this area, is the tides. An examination of the tides from the biological aspect shows that they have had many important consequences, not only for the present life of the sea-shore, but also—it seems not too much to say—for life in general.

Whether life began in the open sea or in the shallow waters of the littoral or in fresh-water pools scientists have not been able to decide. Nevertheless, one thing is certain, namely, that if life did not originate in the tidal zone nor in the area immediately below it, as some still think possible, then life was not very long in reaching there. It is unnecessary to adduce special proof of this statement : the vast number of invertebrate animals that frequent, or have frequented, or have relatives on the shore, from sponges, through cœlenterates, echinoderms, worms, crustaceans, and molluscs up to ascidians, allow of no other conclusion. In its earlier youth, then, *life served an apprenticeship to the tides*, and it is probably not too much to say that life is continuing to show the effects. That is what was meant in speaking just now of demonstrating the consequences of the tides to life in general. That is what biologists intend to express when they speak of the shore as the school wherein many of the most important lessons of life were learnt.

In estimating the importance of the tides to life, whether to life in general or to the present life of the sea-shore in particular, we require to give separate consideration to at least three distinct ways in which the tides act. These are: (1) by causing successive strips of the sea-shore to be pounded more or less violently; in other words, by impact; (2) by subjecting the area exposed to their action to alternate hydration and desiccation; (3) by leaving a rhythmical impress upon organisms subject to their influence. Each of these modes of action may now be taken in turn and examined in its general biological relation. How, for instance, do we explain the origin of a sedentary habit in animals? We can understand the sedentary habit in land-plants, of course, but why in marine plants (where the food substances can be absorbed by the whole surface of the plant directly from the water), and, still more, why in marine animals? The earliest forms of life were almost certainly not fixed forms. The great number of sedentary animals on the sea-shore, sponges, hydroids, polyzoa, barnacles, sea-squirts, and so on, and the obvious value of some form of anchorage in this particular area seem to make it almost certain that the stimulus prompting the sedentary habit in animals is wave-impact, just as no botanist would have any hesitation in ascribing the fixed habit of the larger algæ to the same cause.

Following up this question of wave-impact a little further, we meet another with striking phenomenon which, it seems to us, may not impossibly be correlated with tidal conditions. This phenomenon, which is of very wide-spread occurrence, is known as stereotropism, or stereotaxis (according to whether the animal which exhibits it is fixed or freely moving). The term "stereotropism" expresses the tendency a great many animals have to react in very marked fashion to stimuli of contact, and is well illustrated by a case which the writer himself has studied. On nearly all sandy-muddy shores, where there are stones embedded in the substratum, there occurs, often in considerable numbers, a worm which is known as *Cirratulus*.[1] It is not unlike an earthworm in appearance, but has sprouting from each segment a pair of delicate, rosy processes which look like tentacles, together with a bunch of such processes behind the head. It is always found lying in such a way as to bring every portion of one surface of its body into contact with a stone. If one of these worms is placed in a vessel of sea-water devoid of stones the animal ties itself literally into knots and appears to be very ill at ease. Place a large flat stone in the vessel, and the worm, immediately on coming into contact with it, begins to burrow beneath it. A few minutes

[1] Flattely, F. W., Ecology of *Cirratulus tentaculatus*, *Journ. Mar. Biol. Assoc.*, xi, 1916.

later the worm is found ensconced with its body in contact with the stone, fully extended, its cirri properly expanded, and looking altogether more comfortable. In other words, the animal is markedly stereotaxic, and requires to have the nerve-endings in the skin pressed against a solid surface. This is not an isolated case ; one could illustrate the same phenomenon from a number of different groups belonging to the shore or elsewhere.[1] The wide-spread occurrence of sterotropism has not escaped the attention of biologists. Willey,[2] for instance, regards it as something peculiarly fundamental, as a very primitive property of living matter of which—and this is important—a permanently sedentary habit is merely an extreme manifestation. If this last surmise of Willey's is correct, it follows that if we regard the sedentary habit among animals as being primarily a means of resisting dislodgment by wave-action, then we must also attribute these stereotropic phenomena to the same agency. That is precisely what we mean to suggest, viz. that this habit of maintaining the body in contact with a solid surface may have its origin in the necessity for maintaining a firm hold on the substratum under the influence of wave-action, that it is, in fact, one of the lessons that was early learnt in the hard school of the shore.

Continuing to focus our attention on the wider biological aspect, let us now pass to another aspect of the tides : that of exposing the area covered by their action to alternate hydration and desiccation.

We know that the main route followed by animal life, in its conquest of the land, has been from the sea by ways of rivers ; but it is important to remember that this is not the only route which has been taken by evolving life. The passage from sea to land has also been made via the shore. Everyone knows the common wood-louse, which lives in damp, decaying wood or in humus. This animal closely resembles, and, in fact, is closely related to, the essentially marine forms which we know as "sea-slaters." Bridging the gap between the purely terrestrial wood-louse and entirely marine Isopods are a number of amphibious forms, one of which, *Ligia oceanica*, sometimes arouses the curiosity of summer visitors to the seaside by extending its area of activity to the promenade. Again, there is a common British species of periwinkle, or sea-snail, *Littorina neritoides*, which lives in crevices well above high-water mark of spring-tides, and is satisfied with an occasional spraying by the waves or with the moisture arising from the sea. A

[1] For further examples of stereotropism in animals reference should be made to Loeb's *Forced Movements, Tropisms, and Animal Conduct* (Lippincott, 1918).

[2] Willey, A., *Convergence in Evolution*. (London : Murray, 1911.)

tropical species of the same genus has left its original marine habit yet another step behind, since it occurs among grass on the top of high cliffs, or is found climbing low shrubs. Take the robber crabs which climb coconut-trees in the islands of the Pacific. They present almost an exact analogy to the Amphibia proper. The adults have become essentially terrestrial animals—their gills have become modified and adapted to breathing atmospheric oxygen—and they only return to the water in order to reproduce their kind. Yet another very interesting form in this connection is a little tropical fish called *Periophthalmus*, which has acquired the habit of sunning itself on rocks at low tide, or of creeping about over sand-flats by means of its pectoral fins. In relation with this habit *Periophthalmus* has become exceedingly alert and watchful; the eyes are very prominent, and move about independently as if on swivels. All these examples give one the impression that a second invasion of the land is being attempted directly from the seashore, and would perhaps have met with more success than it has done were the ground not already pre-empted by the descendants of the prior invaders. Certainly, even this partial success would scarcely have been possible were it not for the action of the tides, which provide precisely the conditions necessary. An animal starts a very short distance above the low-water mark of spring-tides, where it has to face desiccation only for an hour or so every fortnight. From there some of its descendants are able to creep to a point where they are uncovered a little more frequently and a little longer each time, and so on by gradual stages to high-water mark of spring-tides or above. The tides, in fact, by their daily movements, and by their increase and decrease following the different phases of the moon, make the shore zone an effective bridge between land and sea; another biological effect of their action.

With regard to the rhythmical effects of the tides, it is not our intention to spend much time on this subject, as we have already dealt with it in another place.[1] To put the matter briefly, the tides impose a periodicity upon the behaviour of shore forms, and this tends to become impressed upon the animal and to persist for a time even when the latter is removed from the tidal influence. This is well illustrated by the tiny flat-worm *Convoluta roscoffensis*, which lives in great colonies on certain flat, sandy beaches. At low tide the worms are at the surface; as the tide reaches them they begin to disappear. The worms are thus constantly moving up and down at intervals

[1] Flattely, F. W., "Rhythm in Nature," SCIENCE PROGRESS, Jan. 1920. Cf. also Reynolds, W. E., "The Cycles and Super-cycles of Nature," *ibid*, Oct. 1920.

determined by the tides. Reproduction in this form is also a definitely periodic phenomenon. Removed to the laboratory and placed in tall test-tubes half filled with sand and half with water, the animals continue to move up and down at intervals strictly corresponding with particular states of the tide outside.

Now, in order to translate the phenomenon of tidal periodicity into terms of life in general, one is compelled to " hedge " a little. It is not altogether intended to suggest, as was done for the phenomenon of stereotropism, that all the many rhythms we see around us (apart from those, of course, obviously referable to external sources) are to be explained as an echo of the times when life was hammered into shape on the seashore (although in some cases this is not impossible). But they are almost certainly merely another form of the same thing. Thus, we find ourselves very frequently employing the analogy of the tides in describing, to give an example, not a few periodic human functions and activities. We will go no further at present than to say that the similarity is perhaps more fundamental than we are aware of.

So far, our discussion has been of a distinctly speculative character ; we may now leave speculation for facts, and give our attention to the biological effects of the tides as they may be observed every day upon the seashore.

The influence of wave-impact on the life of the seashore is at once made apparent to the observer, not only by the large number of fixed forms, of which the rock-barnacle is a prominent example, but also by the tendency even among free animals to keep a grip of the substratum. The adhesion of barnacles is due to a cement substance secreted by special glands in the region of the head, but in the case of sea-anemones, flat-worms, sea-slugs, and sea-snails, adhesion is due merely to an exceedingly close contact of the body with the substratum. The sticking power of the limpet is, of course, proverbial ; it is thought to be due, not to the secretion of a cement substance, but to the exceedingly close contact of the foot with the substratum, the sole of the foot following every little irregularity of the rock surface, and being, so to speak, rolled out on the rock.[1] A very interesting adaptation to life in the area of wave-action is seen in a fish called the lump-sucker, which has the pelvic fins converted into a cup, or sucker, with which it clings to rocks and weeds. Mere adhesive powers are, apparently, not sufficient, since there is a tendency also to adopt a form which offers the least possible amount of resistance to the water, and, in particular, to reduce height. Shore forms are typically flattened forms. It is not difficult to convince

[1] Davis, J. R. A., and Fleure, H. J., *L.M.B.C.* Memoir X. *Patella.* London, 1903.

oneself of this fact by comparing, in imagination, a series of cross-sections through a number of the most familiar shore forms, such as crab, limpet, barnacle, starfish, brittlestar, sand-hopper, sea-slater, butter-fish, flounder, and so on. In some the flattening is lateral, in others dorso-ventral. We should also call to mind the numerous encrusting forms, such as the "crumb-of-bread" sponge, various polyzoa, the bivalve *Anomia*, sea-squirts like *Styleopsis*. It is not meant to suggest that flattened forms do not occur elsewhere, but rather that they are particularly characteristic of the shore. The so-called "stream line" form is conspicuous by its absence. This is because the water is not acting in one determinate direction, as in rivers, but all round. The most typical shape is that of the limpet, and even so there is evidence for believing that limpets which live on exposed rocks have lower spires than those living in sheltered situations.

The danger of being swept away which is ever confronting the adult shore animal menaces still more strongly its eggs and young. No other vital function reflects more clearly the difficulties of the environment than that of reproduction, and it is consequently not surprising to find that reproductive phenomena in the area of wave-action are particularly interesting. In the first place, shore animals are frequently very prolific; the egg-ribbon of a sea-slug may contain sometimes not much less than a million eggs. This is one method. As an alternative, only relatively few eggs may be deposited, but these are made the object of special care on the part of the parent. The parent gunnel (*Centronotus gunnellus*) coils itself round the eggs and protects them with its body. Another rock-fish, the lump-sucker (*Cyclopterus lumpus*) fans the eggs with its tail to ensure their constant aeration. The stickleback builds a nest. Other forms hatch the eggs inside the body, or, in other words, are viviparous. Fertilisation is frequently internal, and the eggs, when deposited, are enclosed in a protective envelope, which adheres to the substratum. This may be gelatinous, as in some Polychæts and most sea-slugs, or chitinous, as in the whelk (*Buccinum undatum*) and dog-whelk (*Purpura lapillus*).

We have seen that there is a sense in which the rise and fall of the tides has provided an opportunity for marine animals to attempt the conquest of the land. Looked at from another view-point, the alternation of aquatic and land conditions presents a very urgent problem, or set of problems. Marine animals require to breathe dissolved oxygen, and are unable to utilise directly the oxygen of the atmosphere. Further, the bodies of many, though by no means of all, marine animals, are soft, contain a great deal of water, and are unable to resist

exposure to the sun's rays. The breathing difficulty is got over by a complete cessation of activity at low tide, a kind of hibernation. Where the animal lives high up the shore its periods of inactivity may far exceed its periods of activity. Thus, barnacles may remain dormant for a period equivalent to nineteen-twentieths of their whole existence. The risk of desiccation is countered in many ways. Burrowing in the sand is one of them, hiding under stones or seaweed another. These are behaviour responses. Sedentary and sluggish animals, incapable of burrowing or hiding, show structural modifications, such as shells and tubes, which aid in the conservation of moisture. One of the most interesting of these is seen in the common rock-barnacle, the shell of which is closed by accurately fitting valves. At low tide, a bubble of air may be retained between the tips of the valves, and its oxygen is, no doubt, an adjunct to respiration. The clearly perceptible clicking sound heard when one is walking over barnacle-covered rocks is due to the complete shutting of the valves as a protective measure, and consequent rupture of the air-bubble.[1] Analogous arrangements are seen in the *opercula* of Gasteropod molluscs, and so on ; to discuss them all would require more space than we have at our disposal.

There is little need to return to the question of rhythm ; the flat-worm, *Convoluta,* has provided us with one example, and there would be no difficulty in finding others. As we have seen, the constant reaction to the tidal rhythm has a profound effect upon the behaviour and other functions, and this effect continues even when the organism is withdrawn from tidal influence. In considering the behaviour of shore forms, therefore, it is particularly necessary to take this factor into account, or our conclusions may be untrustworthy.

A survey of the statements which have been put forward in this article seems to justify the conclusion that the tides have not only been of very great importance in moulding the present day life of the seashore, but have also had far-reaching consequences to life in general. This pulsing, ever-changing strip of the earth's surface has played a part in life out of all proportion to its size.

[1] Darwin, C., *A Monograph of the Balanoidea,* vol. i., 1854.

SYMBIOSIS AND THE BIOLOGY OF FOOD

By H. REINHEIMER

IN the *Origin* Darwin states his view that the productions of nature are far " truer " in character than man's productions, that they are infinitely better adapted to the most complex conditions of life, and that they plainly bear the stamp of far higher workmanship.

This being the case, and time *per se* on Darwin's own admission, not being capable of effecting anything at all, why then base, as he did, the theory of evolution on domestication rather than upon the study of the long-protracted gestation processes of nature—to use an expression of Robert Chambers's—in which processes, according to my view, Symbiosis has played the leading part? Ought we not to pause and to consider in detail the methods of nature's workmanship, the principles which guide it and which differentiate it from those governing domestication, before we conclude that any and every change is good enough to illustrate nature's sublime method of evolution? Over and over again Darwin emphasises (particularly in *Variations*) that domesticated races of animals and cultivated races of plants often exhibit an abnormal character, as compared with natural species, because " they have been modified, not for their own benefit, but for that of man," and he concedes, further, that the higher variability of domestic productions may perhaps in part be due to excess of food—that is, strictly speaking, a pathological cause.

That " fatty degeneration " and precocity are only too frequently induced by domestication is, of course, well known. Recent research has confirmed the view that the usual methods of domestication are pregnant with unwholesome results upon the constitution of the organism, that they retard or inhibit its progressive evolution, and it has also brought to light the fact that they are often fraught with undesirable reactions upon man.

In the *Journal of Economic Biology*, June 1915, Mr. G. Massee pointed out that the leading idea in dealing with cultivated plants is to intensify or to develop to an *abnormal* extent either the flowering, fruiting, or some desirable quality,

and in so doing there is a marked tendency to upset the physiological balance of the plant and also to open the door to the spread of disease.

That domestication of the animal is very similar in its ill effects to slavery in the human world may be gleaned from the following: Dr. W. P. Pycraft tells us, with regard to the "grey," or "grey-lag" goose, the only species indigenous to the British Islands, that in the good old days this species bred in numbers in the Fen country, where the young were caught by the score, and added to the vast flocks of domesticated geese that proved so valuable a property to the dwellers in these rheumatic regions. Five times a year they were plucked alive! Dr. Pycraft goes on to say that some hold that it is largely, at any rate, to this barbarous practice that we owe our breeds of white geese to-day. "The pigmentation of a feather," he says, "is a very variable quantity, and is easily upset. A coloured feather plucked out of a living bird is occasionally replaced by a white one, and, when this plucking is repeated five times a year, and for several years, nature's colour-box becomes exhausted. Thus a white goose is soon 'made'; and it would almost seem that in a few generations the pathological condition became transmitted to the offspring. Whether the white geese so prized by the Romans were produced in this manner, or by the selection of variations in the direction of whiteness till success crowned their efforts, is not known."

We are further told: "The 'grey-lag' goose gained its qualifying 'lag' because it was the one, of all the wild geese which winter with us, which 'lagged' behind, after the rest had betaken themselves to the more northern breeding quarters. This display of confidence was most shockingly abused, for we made of them bond-slaves, and bade them first make feathers from bare and bleeding bodies, and later bad livers for *pâté de foie gras*, and fat, unwieldy bodies for Michaelmas and Christmas feasts."

"Exquisite as are the results of civilisation," says Prof. R. C. Punnett in his *Mendelism*, "it is by the degradation of the wild that they have been brought about." And he goes on to question: "How far are we justified in regarding this as a picture of the manner in which evolution works?"

My answer is that, inasmuch as domestication induces serious degrading effects, it represents the inverse of progressive evolution, which, *per contra*, evinces a slow and gradual upbuilding. There are, of course, different degrees, and also different methods of domestication, and in some cases there is an actual gain of valuable factors, compensating to some extent for the losses in other directions. It is a natural process.

But, whilst domestication is thus justly suspect, we have in Symbiosis a far better principle of progressive modification. Symbiosis is not only free from the blemishes of domestication, but represents also the source of all wholesome accumulation of what I call physiological capital, which is essential to the progress of organic life. Symbiosis implies work and systematic service of organism by organism by the method of "partnership."

Ability to rely upon duly "remunerated" biological partners makes possible a progressive avoidance of waste and of loss of energy. The symbiotic relation proved of such avail in health and in progress that a steady expansion of its range could take place. The partners betook themselves to ever wider fields of action, and, in lieu of the primitive "attached" forms of Symbiosis, wider and more evolved forms of "non-attached" Symbiosis became established, until whole groups of organisms came to be symbiotic with others. Partnership is by no means confined to physical attachment, and it is to the partnership, the sociological aspect of the relation, rather than to the narrow morphological aspect, that I wish to call attention.

To take a concrete, though primitive, case of Symbiosis as discovered and communicated by Mme van Bosse in her work on tropical algæ: In some cases of alga-cum-sponge Symbiosis the alga branches profusely and ramifies through the canal system of the sponge, the alga using for food the carbon dioxide given off by the living sponge tissues, obtaining its salts from the water passing through the canals, and, on the other hand, supplying the sponge with the oxygen given off in photosynthesis.

What emerges is this: the sponge needs a particular substance, which the alga knows how to manufacture, and which the latter can well afford to surrender in exchange for a spare product of the sponge. Given a situation such as this—given, in other words, a pair of opposites to be adequately accommodated, given also sufficient disposition on the part of the opposite parties, the opportunity is provided for a continuous process of mutual stimulation, purporting increased mutual benefits and conducing in the result to increased specialisation, increasing capacity and efficiency, and increasing yields, not only to the "partners," but also to life generally, which is thus the richer for the adoption of the symbiotic mode of life, wherever met with.

The exchange of substances between alga and sponge is, of course, but an example of the complemental relation existing between plant and animal on the grand scale of nature. But, inasmuch as this complemental relation is as indispensable as

it is also useful to life generally, it is clear that every organism, however humble, which is engaged in Symbiosis, is contributing to the general welfare of life. It may justly be said that symbiotic organisms are good citizens in organic civilisation.

The Economy of Nature, as evidenced by the totality of the phenomena of Symbiosis, shows that there is indeed an important fundamental Sociology of Nature, which can on no account be neglected. No one, in dealing with such phenomena as Symbiosis and Parasitism, can help using sociological terms. The use of such terms, however, is generally implied to be but metaphorical, or, at least, only provisional. My contention is that there exists a very actual and very real Sociology of Nature, and this truth is to some extent to be established in this paper.

What is involved in Symbiosis is this : a kind of marriage, a continuous *do ut des*, a mutual industry entailing moderation and restraint in many ways. The symbiotic life may therefore be regarded as providing early physiological, psychological, and even moral, or at least bio-moral, training of the organism—a schooling in fair dealings, in physiological and biological righteousness, in short, in character. The biological is, of course, inseparable from the sociological interpretation. That this is so may be gleaned from a definition of protoplasm as supplied by Geddes and Thomson as of " a successful firm which owes its success to an unusually fortunate combination of partners—of inventive, organising, administrating, pushing, competitive, and other geniuses—a firm unified from within, whether by a common purpose, or by the predominant will of its leading partners, or by something of both " (the internal unity being far from being understood).

This is, of course, only a comparison, calculated to emphasise how largely and fundamentally biology is a science of behaviour. The constitution of protoplasm may well be subject to cosmic laws at present beyond our ken. Meanwhile, we shall probably not be wide of the mark in viewing protoplasm as constituted by a kind of Symbiosis on the part of the " life-elements." Not any and every kind of union of the " life-elements " can constitute protoplasm, any more than any and every biological union can constitute Symbiosis. Qualifications of special serviceability are required in each case. An all-essential legitimacy or " integrity " is required in each case if abiding results are to ensue. The study of Symbiosis clearly brings out the fact that there are ways and methods of life which tend towards progressive modifications of the protoplasm, in contrast to other modes which have the opposite effects. We know that all species in nature have their own protoplasm. But these protoplasms are not equally viable and resisting to disease. We know, further, that the multiple ingestions and (proteid)

intoxications of the organism tend to modify the protoplasm, it remaining to be seen which feeding habits are the most calculated to enrich protoplasm and which to impoverish it. Here the autonomy of the organism plays a prominent part in determining the issue. We also know that there exist a number of physiological and biological antagonisms, which remain to be interpreted as to their full sociological meaning. Again, we are led to infer increasingly every day that health depends pre-eminently upon biological support, such as that of phagocytes and other symbiotic organisms. What it all points to is this: that there is a definite sociological order of the universe which, if obeyed, leads to progressive evolution, or, if disobeyed, to degeneration. My thesis is that Symbiosis furnishes the positive and progressive method of life, whilst one-sided exploitation of organism by organism furnishes the negative, degenerative phase of life.

Let us take another instance of primitive Symbiosis to show that the symbiotic regime tends to produce economic margins much in the same way in which our industrial division of labour produces a constant surplus, rendering possible a maximum of happiness with a maximum of population, i.e. ampler life.

There is the case of the common fresh-water red alga, *Batracho-spermum* (*B. vagans*, var. *epiplanorbis*), which always grows on the shell of the fresh-water mollusc, *Planorbis*. The mollusc gains by being protected from enemies, being densely clad with the alga, and is also able to live in places which would be otherwise unfit for it, owing to poverty of oxygen and excess of carbon dioxide, the former gas being supplied, and the latter removed, by the alga.

In view of these facts, which may again be regarded as typical of the operation of plant-animal Symbiosis, it may be fairly claimed that the symbiotic relation tends to leave the world the better for its presence. In the present example, both the alga and mollusc are decidedly better off for their Symbiosis. The more Symbiosis between them, the less need of predaceous interferences with each other and with other organisms. The point which I wish to stress above all is this, that the symbiotic relation is of more than local significance, that everywhere it makes in an important way for life generally. The presence of symbiotic systems makes, on the whole, for increased economy and increased security of life. In the above case we see, moreover, that the plant, by rendering animal life possible in an otherwise inhospitable region, assists very materially the diversification of life, which is an important factor of evolution. Darwin insisted, particularly in chap. iv. of the *Origin*, on the importance of the principle

of Divergence of Character. He showed that diversification is a useful alternative to close competition, that the greatest amount of life can be supported by great diversification of structure, and he indicated that a carnivorous quadruped, for example, may become more successful by becoming less carnivorous. In his own words: "The advantage of diversification of structure in the inhabitants of the same region is, in fact, the same as that of the physiological division of labour in the organs of the same individual body."

Parenthetically, I would here remark on the fact, observable throughout nature, that occasional abstemiousness from food and an occasional conversion from a carnivorous to a vegetarian nutrition, are usually followed by beneficial effects upon the species. My explanation is that by such means co-operation and general symbiotic integrity are apt to be restored, with the result that plasticity and rejuvenescence are again safeguarded. In other words, the abstemiousness signifies a "good" diversification, one that approximates the general restraint normal to Symbiosis.

The plant can assist symbiotic diversification with increasing reliability the more it is in turn supported by symbiotic contributions from the animal. In the above example, the reliability of the alga is undoubtedly the greater the more the alga can in turn rely upon constancy of supplies of carbon dioxide from the animal. And it is constant and systematic services which count in industry. If, in the natural scheme of things, the plant was to become increasingly a manufacturing specialist, and if the animal was to become increasingly a complemental specialist, this desideratum could have been achieved on condition only that the mutual relation between plant and animal remained in all essentials symbiotic. Which is also saying that the respective appetites had to remain essentially restrained in accordance with the requirements of Symbiosis, and, further, that the healthiness of the joint evolution of plant and animal, and the health of plant and animal individually, depended upon persistent Symbiosis—matters to which I attach the utmost importance.

In my opinion, the life of the flora is so interdependent with that of the fauna that separate study can scarcely conduce to a full understanding. If we cannot have the widest possible view of evolution, it would often be better to have none at all. The plant, then, we may take it, has steadily increased and improved its yields, in proportion as it was able to rely upon an increasing adequacy of animal counter-services. It is recognised that the status of a plant in the evolutionary scale is in accordance with its output of useful substances; but we must likewise recognise that the status of the animal is com-

mensurate with its capacity of service to the plant. It has often been remarked that the evolution of the animal proceeded *pari passu* with that of the plant; but it has been less commonly realised that the plant, too, has had to rely upon symbiotic help, both vegetable and animal. Still less has it been recognised that the symbiotic relation between plant and animal is the indispensable norm of life in all orders, not excluding even the mammalia or man.

More than one writer on evolution has asserted symbiotic adaptation to culminate with insect and bird. But mammalian services to, and alliances with, the plant are numerous and important, whilst special significance attaches itself to the fact of the continuity of the symbiotic relation throughout evolution. In man the plant has a conscious partner—one whose essential intelligence may well be presumed to be subtly supported by numerous plant influences.

As regards primitive Symbiosis once more, that of the lichen has been much investigated, and here the success of the partnership is striking enough. And so is the modification of the stirp commensurate with the intimacy of the partnership between alga and fungus, and indicative of the progressive endowment of the protoplasm consequent upon Symbiosis. The fact of such modification inclines one to the view that sexual partnership constitutes a case of Symbiosis, early hermaphroditic stages representing attached or primitive Symbiosis. Physical attachment, however, is a mere detail; the absence of such attachment need not impair the reality of a symbiotic union. It is not likely that nature could have ever forsaken a method of life as proven as Symbiosis. What other method of evolution is there so ideally conducive to economy and progress?

In the erroneous belief that only attached forms constitute Symbiosis, the case of the lichen is usually looked upon as the stock example of Symbiosis. What I would specially insist upon are the facts of the healthiness, the longevity, and the general usefulness and success of this compound organism, far exceeding anything fungus and alga could achieve singly.

A similar case is represented by the symbiotic bacteria in the soil, which are more resistant to unfavourable influences, to disease, than the predaceous inhabitants of the soil. They, too, are of immense usefulness to organic life generally. Modern scientific agriculture is largely based upon facts of the usefulness of symbiotic bacteria and their powers of resistance to disease. According to Prof. H. F. Osborn, a bacteria-less ocean and a bacteria-less earth would soon be uninhabitable for either plants or animals, and bacteria-like organisms pre-

pared both the ocean and the earth for the further evolution of plants and animals.

What is still more important, however, though generally overlooked, is this: both lichen and symbiotic bacteria are cross-feeders, *i.e.* they draw on the inorganic world for food. Contrary to the widely prevailing prejudice that habitual pain and habitual carnage were indispensable and compulsory at the earlier stages of evolution, before the principle of "live and let live" was introduced by man, as it is thought, we find that the bacteria already knew better methods, had indeed found a highly satisfactory method of solving the economic and sociological problem of existence. Many of them had no need whatever to resort to depredation, as they derived their food from inorganic compounds. Nor would a habit of depredation have allowed them to carry on their indispensable pioneer work as efficiently and as satisfactorily as it has been performed. Numbers of these bacteria do not live on organic food at all—such modes of feeding at their stage of life not being compatible with genuine Symbiosis. It has been shown that the smallest trace of organic carbon or nitrogen compounds is actually injurious to them. There are the Schizomycetes, or "sulphur-bacteria," numbers of which have been found to thrive best in a mineral solution containing ammonium sulphate as a source of nitrogen and with chalk as a neutralising agent. According to Mr. Skene's investigation, as contributed to the *New Phytologist*, 1914, all the organic sources of nitrogen and carbon which were investigated proved to be without favourable influence on the growth of the bacteria; indeed, as a rule, they tended to inhibit development. These bacteria, then, are typical cross-feeders, and as such thrive well. Associated with a proper habit of feeding we find the power of photosynthesis, and—depending on both—the capacity of carrying on genuine Symbiosis with higher (green) plants. The oxygen required for their own industrial purposes these bacteria are able to purchase from the symbiotic plants, and, having got it, they use it to oxidise sulphur or one of its unoxidised compounds, producing in some cases sulphur and in others sulphuric acid. They are thus of agricultural use in a similar way to the nitrogen-fixing bacteria in the soil and in Symbiosis with the plant, and this in so far as they are mainly cross-feeders. Ideal food and ideal work, therefore, go together.

The more, on the other hand, a green cell or plant tends in the direction of parasitism, the more it is apt to lose its chlorophyll. In the case of the lichen, the symbiotic union between alga and fungus depends in great part, of course, upon the photosynthetic activity of the green alga, which could on no account afford to lose its chlorophyll lest the union

with the fungus and the chief purpose of this union should be endangered.

The union depends likewise upon the power, in which the fungus is predominant, of disintegrating, by both mechanical and chemical means, the rocks upon which lichens are often growing. The lichen, as is well known, is a pioneer as a soil-former. According to Dr. O. Darbishire, if bare rocks are found in the neighbourhood of the Poles themselves there is little doubt that lichens will be found growing there. Many of these rock-lichens are distinguished by an astounding longevity. The bare rock, I would add, supplies the ideal food of the lichen. At one time eminent philosophers believed the lichen to be created out of solid rock. The point of importance is that the soil is generally enriched by the work of the strictly cross-feeding, symbiotic lichen.

The same may be said of the clover, a plant which will grow on bare places, provided the right minerals are obtainable, and which is similarly remarkable for its symbiotic alliances. As regards *melilotus*, or sweet clover, an American writer, Mr. E. E. Miller, states: "In the limestone sections it has become the first-aid crop of the most depleted soils. On land so poor in humus and nitrogen that practically no other crop will grow, sweet clover thrives. Being a legume, and an uncommonly vigorous one, it takes it but a few years to store up in the soil sufficient nitrogen, and to furnish by its own growth and decay sufficient organic matter to make the land fertile enough for any of the staple crops."

Of *Lespedeza*, or Japanese clover, he says: "The good it has done by holding and enriching old fields and waste lands which, but for it, would have washed away or grown up with really harmful weeds, no man can accurately estimate. It has literally been the salvation of hundreds of thousands of acres of land all over the Cotton Belt and north of it."

Amongst cross-feeding, symbiotic, and widely useful bacteria, there is the case of *Nitroso Monas*, living on ammonium sulphate, taking its energy from the nitrogen of ammonium, and forming nitrites. Living with it in Symbiosis is *Nitrobacter*, using the nitrites formed by *Nitroso Monas*, and oxidising them into nitrates. These nitrates are of great importance. Without the primal industry of *Nitroso Monas*, the Symbiosis with *Nitrobacter* would be impossible, and without the succession of ever higher but similar forms of life-partnerships based upon it, the evolution of the highest forms of life would have been impossible. All important pioneer work is thus done on cross-feeding. Strenuous work *cum* symbiotic cross-feeding provides the capital on which race after race of higher organisms is reared.

My thesis as regards feeding is this: "Nature abhors perpetual in-feeding." By the term "in-feeding" I mean the indolent appropriation of food manufactured by close relatives, and the correlated shirking of the economic duty of production, or of mutual service in some kind. By "cross-feeding," on the other hand, I mean feeding on material—generally of another "kingdom"—which does not involve the non-symbiotic devouring of organism by organism—the killing of the goose which lays the golden eggs. Once more we may infer that one implication of the underlying truth is that the "self-regarding" activities are to be restrained and regulated in accordance with the contingencies of existence. These contingencies are mainly bio-economic and sociological, *i.e.* they have regard to wide biological usefulness—for instance, to the welfare of biological partners, to accommodate whom must be, and is, a prime concern of biological relation.

The long-standing relation between bee and flower also rests upon the "cross-feeding" principle, which alone allows of perfect "live and let live." If the bees transgress even against "live and let live," by felonious ways of obtaining the nectar, they become "debauched." Just as the parasitic mode of life is generally derived from a previous predatory one, so, I contend, the latter is derived from a previous cross-feeding and symbiotic one. Cross-feeding, in other words, is primitive, representing the norm and integrity of life.

Darwin stated that pollination by insect agency might be a great gain to a plant, though nine-tenths of the pollen be eaten by the insect, so long as at least a cross was effected. The full implication of this observation is that the establishment of sound and progressive symbiotic relations in the world of life reacts favourably on the plant. The plant is thus able to command reliable and systematic counter-services, obviating the far greater waste of pollen in wind-fertilisation. Ability to rely upon biological Symbiosis in this case means great constancy of "crossing" and, hence, a more frequent gain of vigour and health. And it means more: the biological nexus, once satisfactorily established, acts as a stimulant to the plant to perfect its own domestic Symbiosis, conforming more and more to the contingencies of biological Symbiosis, *i.e.* of wide and general usefulness (co-adaptation). It is as though the plant, thus reliably allied, felt a higher call in life than one entertaining only casual biological relations.

It is an indisputable fact, emphasised by Darwin, that entomophilous plants (relying on wind-fertilisation) are higher in the scale than anemophilous (relying on animal agency). They have a higher output of valuable substances—for the reasons already given, namely, that they have earned and enjoy

more systematic and more fruitful stimulation. The non-symbiotic organism, on the other hand, is apt to drift away more and more from the true health—and wealth—giving principle, and it eventually becomes antagonistic to the true Symbiolists, just as wastrels become a moral and positive danger in human society. Pathogenesis ultimately very generally seems to rest upon the contrasting interests represented by symbiotic and non-symbiotic forms of life respectively. The Nematode worms, which play so great a part in disease, do not seem to lead on to anything else. They are mostly parasitic, and, according to Prof. J. A. Thomson, they are almost the only animal types without wandering phagocytes. They have apparently flouted Symbiosis altogether. Their poverty and lowliness are due to the absence of Symbiosis, both domestic and biological. The Lancelets show similar poverty of wandering phagocytes, and regarding them Prof. Thomson says that, though near the base of the vertebrate branch, they are specialised types in a cul-de-sac of their own. True, some of the Nematodes have at least stationary phagocytes ; but there are some important exceptions in their parasitism, for it is known that some Antarctic Nematodes at least are vegetarian. The nemesis of Parasitism, with its ghastly forms of degeneration and its cruel penalties in the shape of hyper-parasitism, is well known. It is scarcely realised, however, to what an extent all predatory organisms, even those not considered as parasitic, have their " hyper-in-feeders " to plague them. Thus, amongst fishes infection is quite common. The " majestic " sunfish, *Orthagoriscus mola*, notorious for its depredations amongst eel larvæ, the leptocephalids, is an example. According to Geddes and Thomson, there are " the tuft of barnacles upon his back, the biting isopods like enormous fleas upon his skin, the trematodes sucking like leeches upon his eyes ; and within, not only is his alimentary canal crammed with worms more than with food, but his liver is changed from its natural brown almost into the likeness of a tangle of white worsted, of which each thread is a tape-worm."

It is also a notorious fact that there obtains a high " infant mortality " amongst carnivora, which points to a serious decline of viability amongst them. " In-feeding," in fact, constitutes a considerable divorce from Symbiosis, and it is *pro tanto* negative in its results. Contrast a case of an in-feeding species with that of a cross-feeding, from instances taken at random : In *Nature*, Sept. 4, 1913, Mr. F. Balfour Browne tells us the following concerning a swimming, carnivorous water-beetle of the group *Hydradephaga* : " I found that tadpoles and pieces of chopped worm were suitable food, but under natural conditions small newts, water-shrimps, and insect

larvæ—including brothers and sisters—constitute the normal diet. It is impossible to keep two larvæ together in one small vessel, as one attacks and kills the other within a few hours. Even when I gave a tub to four specimens only one survived after a few weeks, so that in a small loch, where at least some thousands of these larvæ hatch out, the death-rate must be enormous."

Now take the case of the honey-ants. "These ants," says Mr. P. Leonard, in the *Scientific American*, Supp., Dec. 9, 1916, "do not display such a wolfish eagerness to acquire any chance scraps of food as is shown by other species, who live from hand to mouth." Mr. Leonard goes on to say that, whilst among the solitary insects, such as the flies, the moths, and beetles, only a very small percentage of their numerous offspring ever reaches maturity, owing to paternal neglect, amongst ants, under favourable conditions, the infant mortality is practically nil. We are further told: "The ants have shown the possibility of a perfect communal life, and have proved that individuals can be incited to the maximum of effort with the minimum of personal advantage, and that the little states based upon unselfish sisterhood are supremely fitted to survive in the struggle for existence."

Mr. Balfour Browne's comparison of a carnivorous with a herbivorous, or at least omnivorous, species of water-beetle almost literally bears out the truth that you cannot make a silk purse out of a sow's ear, *i.e.* by the method of perpetual in-feeding. Whilst the carnivorous beetle lays its eggs singly in holes pierced by it in the living vegetation, the herbivorous beetle builds an elaborate silken cocoon, which floats in the water and contains the eggs.

We have seen that one result of the operation of Symbiosis is to make the conditions of existence on our globe increasingly favourable for those which are inclined to reciprocity. Moreover, the presence of industrious organisms tends to raise the level of organic life much in the same way as the presence of a thrifty class of citizens tends to raise the level of the socio-political life. I employ the term symbiotic momenta in order to indicate the power for good and for an ampler life engendered by Symbiosis. These momenta in their entirety constitute, I believe, an important evolutionary principle, namely, that of "Symbiogenesis," by which I mean the direction given to evolution by the long-continued operation of Symbiosis in the production of higher forms of life, and in the more complete development of beneficial relations between them. I consider that the terrestrial conditions of life, for instance, are more favourable than aquatic to the advance of Symbiosis, owing to greater security and better opportunities for mutuality.

Upon the land a far greater number of symbiotic momenta could, therefore, arise and push each other on unceasingly; and the result is that it is upon the land that we find the most developed, the most advanced, and the most intelligent animals. I would point out that such notorious cross-feeders as man, the apes, and the parrots, for instance, rank high in intelligence and status. I believe they could never have attained to their position by perpetual in-feeding.

It is worth recalling, in this connection, that food-borne infection is very common amongst animals and men, and, in particular, that it is always animal food (in-feeding) which is responsible for such infection. Our food-plants are not attacked by any micro-organisms pathogenic to man or other animals, but our animal slaves generally suffer from bacterial infection, which is communicable to man.

Again, there is the fact, established by Prof. Richet and other physiologists, that fruits and vegetables—with the exception of a few over-cultivated varieties—never induce "alimentary anaphylaxis" (the dietary equivalent of serum disease); whilst flesh foods often produce the same distressing symptoms upon body and mind as are known frequently to result from a direct introduction of unsuitable proteins into the blood.

Facts such as these I believe to be of almost inconceivable importance in evolutionary physiology. They go a long way in proof of the contention that nature abhors promiscuous feeding, and, further, that the required specificity of the food depends on the delicate and complicated relation of organisms in the web of life. That is to say, the food is precisely suitable only where there exists an adequately reciprocal relation between supplier and supplied, and this involves cross- rather than in-feeding.

Prof. Richet interprets his results to mean that nature desiderates the stability of the species. But it is also necessary to recognise that, if stability is to be maintained, there must exist a satisfactory bio-social basis of life, and this basis is apt to vanish with any prolonged unilateral exploitation of organism by organism, as in domestication, in parasitism, and in merely "seasonal" Symbiosis, for instance.

We might add the discovery that the pollen of certain plants is apt to cause anaphylactic symptoms in man and beast, as, for instance, in hay-fever. It appears that the deeper cause of the disease is inherent in the fact of a biological relation the opposite of Symbiosis between the respective animals and plants. The chief culprits in the causation of hay-fever have been found to bé wind-pollinated (anemophilous, rather than entomophilous) plants. Prominent amongst

these are some highly noxious weeds, such as the rag-weeds, cockle-bur, yellow dock, etc., against which the farmer has to wage incessant warfare, since they are inimical to his crops. Here we have plants, which—evidently in the absence of due symbiotic productiveness—show highly redundant rates of multiplication, simultaneously with mischievous, " toxic " effects, which in this case extend to man.

No satisfactory explanation has hitherto been forthcoming for the fact that weeds are generally detrimental to crops. We can now see that the antagonism is that between two contrasting principles of life: the industrious and the improvident; the same antagonism, in fact, which is universally observable throughout life. The inferior species labour not for reciprocal or common, but for fundamentally anti-social, purposes; hence their productions do not support but rather poison the strenuous community of life.

The Nematodes furnish a good illustration of what I call a divorce from Symbiosis, with its direful physiological and biological effects. In the desert some Nematodes are actually symbiotic and beneficial to the plants, but on cultivated lands they very soon exhibit a striking weakness of symbiotic disposition. Consider the following data communicated by Dr. N. A. Cobb, of the United States Dep. of Agriculture (*Nature*, April 4, 1918). The genus Mononchus, which is of world-wide distribution, has some species which are cosmopolitan. Mononchus are regularly present in arable land of a sandy or loamy nature, and sometimes occur in great numbers. Dr. Cobb estimates that there were at least thirty million per acre in the top six inches of a field of maize in New Jersey. Most Mononchus are carnivorous. They have been found to feed on protozoa, on rotifers, and on other Nematodes. One cosmopolitan species was found in Florida feeding on larvæ of *Heterodera radicicola*, a serious root-pest, and it is suggested that further investigation may reveal the possibility of utilising Mononchus to reduce the enormous losses in crops due to plant-infecting Nematodes.

As our cultivated plants are generally over-fed and correspondingly lacking in resistance, this provides the temptation for predaceous excursions on the part of the Nematodes. First they begin, as plant-murderers, to batten on the plants alone, the lapse from Symbiosis becoming evident in redundant multiplication and in abnormal evolution, which in turn offers the temptation to other Nematodes to still more intense depredation, in fact, to in-feeding. The result is so-called obligatory carnivorism, or obligatory parasitism. The latter, at least, avail towards keeping down the numbers of plant-assassins, a check favourable to the plant and likewise to the

maintenance of Symbiosis generally. Such checks are usual with depredation.

As is well known, man himself is liable to serious disease as the result of Nematode infection. Seeing that infection is in accordance with general principles, I am of opinion that prevention should in its turn rely upon a general principle, such as symbiotic integrity, implying that man should shape his habits, and especially his feeding habits, so as to conform as nearly as possible to plant-animal Symbiosis. Apart from Symbiosis, and the incumbent duties and restraints, I do not know of a single principle capable of providing the physiological and psychological funds and the direction necessary for the purposes of normal evolution.

According to competent writers, most of the peculiar features of man and of his kindred are derived from advantages and adaptations gained during a period of arboreal life. This, surely, was a period when the ape-like ancestors of man were content to live mainly on the "kindly fruits of the earth." The physiological and anatomical patrimony of the human race, so far as this evidence goes, was securely founded upon a tolerably non-predaceous feeding basis—the usual requisite and concomitant of Symbiosis, the alternative of parasitic nurture and decline. I would also instance the case of the Vitamines, which the plant alone knows how to manufacture, as indicating that symbiotic organisms enjoy an immense advantage over non-symbiotic in that they receive the most directly effective pabulum for body and mind, which not only sustains but positively directs evolution. I would point out, further, that in our climate, for instance, the chances of survival are infinitely better for those animals that rely upon the surplus stores of the plants than those that seek their provender predaceously among living organisms. In the winter time, as Mr. G. G. Desmond has pointed out, insect fare being "off," the animals that feed on insects are palpably worse off than those that feed upon hard fruits and grain. The latter have made their winter store, and may remain awake in its midst and active enough to go out and about on fine days.

Surely the chances of fruitful, social, and mental life are therefore higher amongst cross-feeders than in-feeders. From the storage of food supplies for the winter it is not a long step to the formation of intellectual habits, which in their turn aid the increase of facility in acquiring and reproducing new knowledge. The cross-feeders, therefore, other things equal, must excel in "plastic power of the brain." Their brain is healthily occupied, and is fed in accordance with the requirements of wholesome and widely useful efficiency, whilst that of carnivores is occupied with theft and murder, and fed in

accordance with the requirements of selfish efficiency, which is productive of lop-sided developments—such, for instance, as disproportionately long fangs, which may require such extravagant supplies of blood for their maintenance as to inhibit valuable supplies from reaching the brain, as they otherwise might have done. The very limitation of symbiotic organisms, and especially their indispensable discrimination as regards food, in the end make for psychical progress. The evolution of the moral sentiments required sympathy, and this in turn implied the concomitant of gregariousness. Sympathy and gregariousness have all along increased by " mutual aid," which may be considered as a form and extension of symbiotic aid. Gregariousness, moreover, as is well known, requires kinds of food and supplies of food that permit association. We can trust to no other regime in the world but the symbiotic for adequate supplies of food. By maintaining a tolerably symbiotic relation to the " environment," the organism maintains a partner as food supplier and obviates recourse to depredation—the dire necessity of non-symbiotic organisms. Symbiotic suppliers are workers *par excellence*, who know by long and adequate training, and also, we may suppose, by " sympathy " the exact needs of their biological partners. Food supplied under Symbiosis, therefore, is eminently sanctioned by nature, being concordant in character with the most fundamental and the most far-reaching principle of organic association, namely, " live and let live." Such food represents exchange, or surplus capital—to be duly redeemed in one way or another by the organism supplied, who is thus stimulated to work, the exercise of which enriches both himself and the supplier. Such food represents spare capital, the parting with which does not leave the supplier poorer in the end.

A general survey of food habits amongst animals which live exclusively on plant products shows that, as the norm of life, the animal is content with special parts of the plant, and that there is no inherent need for the animal to be actually destructive *vis-à-vis* the plant, the prevailing relation, on the contrary, being one of " live and let live."

The symbiotic relation with its needs of industrious habits rivets the attention of the mind upon reciprocal activities, and thereby tends to fix a corresponding state of mind—a socialised mind, as it were, which is the *sine qua non* of psychic progress. It may justly be claimed that the symbiotic relation provides the best physiological and sociological groundwork for psychic progress, for refinement and elevation of psychical powers.

Darwin anticipated some such principle as Symbiogenesis, as is evident from his constant emphasis of " division of labour "

and of "mutual relations" ("the relation of organism to organism, the most important of all relations").

As he states in chap. iv. of the *Origin*: "No one ought to feel surprised at much remaining as yet unexplained on the origin of species, if we make due allowance for our profound ignorance of the mutual relations of the inhabitants of the world at the present time, and still more so during past ages."

I cannot but think that if Darwin's great works on "Variation" and "Fertilisation" had been more widely and more thoroughly studied, this would have greatly facilitated the acceptance of "Symbiosis."

Huxley likewise foreshadowed a new synthesis when he stated: "When we know that living things are formed of the same elements as the inorganic world, that they act and react upon it, bound by a thousand ties of natural piety, is it probable, nay, is it possible, that they, and they alone, should have no order in their seeming disorder, no unity in their seeming multiplicity, should suffer no explanation by the discovery of some central and sublime law of mutual connection?"

There seems to be, indeed, a central or cosmic law of life, a constitutional law of the universe, which may be stated thus: a body should possess all that is necessary, but no more. Any superfluity acts as an impediment, apt to cause disease, inasmuch as it militates against usefulness in Symbiosis.

And this would apply in a similar way in the physical world. A body needs to be pure and austerely constituted, lest it lose resistance *pari passu* with (cosmic) usefulness.

Some fifteen years ago Prof. Patrick Geddes remarked in a preface to a book on *Parasitism, Organic and Social*: "May we not therefore hope some day to see an antithetical title to the present one—Symbiosis, Social and Organic? Neither economist nor naturalist is ready to write such a book."

Most readers, I think, will agree with me that the time has arrived to attempt a new generalisation.

POPULAR SCIENCE

SOME OTHER BEES

By HERBERT MACE

I

The classification of insects is based primarily upon the structure of the wings, but certain other features are found in each order, the development of which is particularly marked and on which survival or prevalence seems to depend. Thus, the Diptera are remarkable for their astounding powers of reproduction, and the Lepidoptera for diversity of colour and marking, enabling the species to achieve perfect concealment under a great variety of conditions.

Hymenoptera are particularly notable for the extreme care they take to provide for future generations, special structures being prepared for the habitation of the helpless larvæ and quantities of food collected to sustain them during the infant stages. Between the gall-flies, which, by merely puncturing a stem or leaf, induce an abnormal growth which automatically brings an ample supply of food within reach of the sedentary larva, and the social bees and wasps, which actually provide the young with food as required, every conceivable modification of this principle is to be found.

This care is rendered necessary by the fact that the number of young produced by the individual is in all cases relatively small. Few moths, flies, or beetles produce less than fifty to a hundred eggs, while in many cases the number runs into several hundreds, a large margin thus being allowed for inevitable losses when the larva is more or less left to its own devices. Hymenopterous insects, however, produce very few young. In most cases, ten to twenty in a brood is the limit, and particular care of this small number is necessary if the race is to be continued.

At first sight, seeing the enormous number of eggs laid by a queen bee during the summer, this may seem a mis-statement, but it should be remembered that the neuters which form the bulk of the hive population—as of the vespiary or formicarium—are not, strictly speaking, individuals at all, for they are

incapable of reproduction and cannot exist except as parts of the original colony, any more than the leaves of a tree can survive detachment. Indeed, a tree and a colony of bees may equally be regarded as one individual organism and the co-ordination and harmony which exists in the hive and has so often been remarked upon and quoted as a text by theologians and philosophers, is but little more remarkable than in the case of the tree. The fact that neuter bees can detach themselves temporarily from the cluster makes no difference. They cannot live long apart from it or become the progenitors of new colonies or of individuals, their functions being limited, as the functions of leaves are limited, to the collection of food, its assimilation and conveyance to the growing point of the organism. In a tree, the transport is effected by means of the branches, which maintain physical contact, while in the bee it is by elaborate locomotive appliances and instinct. So long as the tree is growing it continues to produce more leaves, and the bee-hive grows by the addition of neuters. When full development is attained, however, this growth is modified, in the one case by formation of flowers and fruit, and in the other by production of individuals with perfect sex organs. Even to minute details, the resemblance is singularly exact, for, while a tree produces an enormous preponderance of the male element, so the bee-hive produces an excessive number of drones, or male bees. Seldom does a hive raise more than twenty female bees, and of these very few are permitted to come to maturity, two or three new colonies being the usual limit of reproduction during the season.

II

Many volumes have been written about the hive-bee, and the wonderful details of its life and growth are readily accessible to all who are not familiar with them. Indeed, so much literature has been devoted to this important insect, mainly on account of its direct service to humanity, that attention has been drawn away from other members of the family *Apidæ*, in which there are many species with habits at least as interesting as those of the typical species.

Comparatively little is known, for instance, of the nearest relatives of *Apis mellifica*. Yet the genus *Apis* contains several other species with interesting features, showing the possibilities of variation, even when a race has developed such a highly specialised state as this which stands at the head of the *Apidæ*.

Three species, for example, are found in India, the most widespread being *Apis Indica*, which very closely resembles *mellifica*, and is by some regarded merely as a pronounced local

variety of the latter. Of this, again, two distinct varieties are found, one inhabiting the plains and the other the hills. Like *mellifica,* it builds its combs in hollow tree-trunks, caves, and similar places, but seldom stores very large quantities of honey, six or seven pounds being the usual amount taken by the natives, either from wild colonies or from those which they keep in hollowed logs and other artificial receptacles. *Indica* is distinctly smaller than *mellifica,* and the comb-cells are correspondingly diminutive.

Another species is the *Apis dorsata,* or Rock-bee, which is larger than *mellifica,* the workers equalling the queen of the latter in size and the comb-cells being as large as those occupied by drones. This insect builds only one comb, but this is of immense size, often being three or four feet in diameter. It seldom builds under cover, but fastens the comb in quite an exposed position, generally to the face of a rock. As a set-off against this accessibility, this species is extremely pugnacious, and the honey, of which as much as sixty pounds is often obtained from a single comb, can only be taken with great difficulty. For this reason, as well as on account of the habit of building in the open, all attempts to domesticate this species have so far failed; otherwise it seems probable that tremendous improvement in the direction of honey-storing would be possible with it.

The third species is *Apis florella,* which is even smaller than *Indica,* the comb-cells being 100 to the square inch as against 25 in the case of *mellifica.* This species builds a single comb about the size of a man's hand, generally in a bush or among the branches of a tree, and this seldom contains more than a few ounces of honey.

The outstanding features of these Indian species are extremely interesting. In the first place, all three migrate seasonally and deliberately, removing the stores from the comb to a locality where other flowers are blooming and returning to the old home in due course. It is not difficult to understand why this habit has been adopted. *Apis mellifica,* of course, migrates at swarming time, the first swarm thrown off consisting of the old queen and the older workers. Generally, this swarm settles comparatively close to the old hive, but there are plenty of instances on record of swarms travelling five or seven miles to a suitable location. But the honey-flow in temperate regions is more or less uniform in all districts, and, when it ceases, a period of cold weather supervenes during which the bees are in a semi-dormant condition. In India, however, although the honey-producing flowers dry up, the conditions which induce hibernation do not exist, and the bees would consequently be actively consuming their stores and expending their energies

in defending them from enemies. Migration to a district entirely distinct in character, where other species of plants are blooming, is therefore of immense advantage, and it seems almost impossible to imagine an insect constituted on the lines of *Apis* surviving in India without this habit.

The second feature is the fact that two of these species confine their structure to a single comb. This is, of course, the simpler and more natural arrangement and it is rather a striking advance on the part of *mellifica* that has brought about the eight or more parallel combs, instead of the one very greatly extended comb of *dorsata*. The immense advantage of this compact form, from the point of defence, is obvious, as is also the habit of building in hollow places.

III

In *Apis* and one or two minor tropical genera closely allied to it, specialisation has reached the stage in which the queen has entirely lost the foraging instinct, together with the necessary structural appliances. She does not build comb, nor undertake the work of hatching and rearing the brood. The advantage of this is, that her energies are concentrated on egg-production; she can turn out an enormous quantity of foragers and nursery workers, and, as she remains always within the hive, her safety is very much more assured. The benefit of this specialisation is very evident, for queens frequently live four or five years, and, although only two or three new colonies are started annually, the race is able to take care of itself and increase under reasonably favourable conditions. The honey-storing habit, which in principle is precisely the same as that of many plants, which are able to survive long periods of drought, in a leafless condition, by living on the stores collected by the leaves and accumulated in the roots, enables sufficient workers to survive the winter, so that the hive, when once substantially founded, is a permanent institution.

In the Bumble-bees, *Bombus*, however, specialisation is only partial and the insect is strictly annual in its duration. Workers are produced, in numbers varying from ten to thirty in the case of the small Carder-bees, to as many as eighty or a hundred in the Great Earth Bumble-bee. But there is not the constant succession of workers which enables a number to survive and assist the queen through the winter and early spring. Worker Bumble-bees do not appear before June, and are all dead by October. Rather more young queens are produced, at the end of the summer, than in the case of *Apis*, and, on the other hand, the males are not so over-predominant. After pairing, the queens seek sheltered quarters in banks of moss or

similar places and come out in the spring to found new colonies. They are fully equipped for the work of nest-building and foraging, though they do not build comb, but, having found a suitable hole and lined it with moss or grass, place a mass of pollen and honey in the centre, deposit some eggs and incubate them. When the larvæ are growing, more food is collected by the mother, but when the workers, which this first batch of eggs produces, have come to maturity, the work of foraging is taken up by them and the queen remains within the nest, keeping up the production of eggs.

Bombus, therefore, forms a very marked half-way house between the solitary bees and the perfectly social species. The habits of all members of the genus are generally similar, though each species varies a little in details. Thus *B. terrestris* invariably builds in a hole in a dry bank, while *B. lapidarius* chooses a stone under which it excavates a hollow for its nest. The Carder-bees build their nests of moss on the surface, generally under the protection of a bush. An interesting habit of several species is that of making a back entrance to the nectaries of certain flowers whose tubular corollas are too narrow and deep for them to enter. This habit can be witnessed by anyone who cares to watch Bumble-bees at work in a field of beans. Proceeding to the base of the flower, the insect tears a hole with its jaws and extracts the nectar. I have noticed that honey-bees and ants will often make use of this back entrance, though I have never seen them make it themselves.

A genus closely allied to *Bombus* is remarkable for being parasitic on the latter. This genus, *Apathus*, is structurally but little different, having even the dilated hind tibiæ and tarsi, which are the foundation of the corbicula, or pollen basket, but without the necessary bristles which hold the load in position. They produce no workers, and are, indeed, properly speaking, solitary species, their simple plan being to enter the nests of *Bombus* and deposit their eggs in the food, leaving the larvæ to be reared by the host.

IV

The details of cell-making, lining of the nest, collection of honey and pollen and its conveyance by special contrivances, which, in such complexity, make up the circle of labour distributed with exquisite nicety in *Apis*, are found in their primitive form in one or other of the numerous species of solitary bee, and in very many cases the survival of the species manifestly depends on the high development of one of these details, instead of all-round advancement.

Thus, the Mason-bees have remarkable facility in the art

of cementing together grains of earth, using the salivary secretion for the purpose, as the honey-bee does for manipulating its wax. With such grains a roughly circular cell is formed upon a wall or fence, stored with honey and pollen, in which the egg is deposited, and sealed up. To this other cells are added, in succession, to the number of eight or ten in all, and although the walls of the cells which are contiguous to each other are smoothly finished, the outer wall is left in a rough condition, so that the effect of the bunch when completed is exactly that of a mere accidental splash of mud.

Anthophora is another genus of workers in earth and contains some noteworthy species, one of which carries out its work in such a remarkably efficient and economical manner that a detailed description of the procedure, which I was fortunate enough to witness in a Balkan ravine, may be of interest, especially as the species appears to be very local and but little known.

About the face of a sunny cliff large numbers of these bees were flying. At first sight I took them to be of the ordinary British species, which commonly makes its burrows in old walls and sand-banks, as there were numerous little tunnels into the cliff about three-eighths of an inch in diameter. On looking closer, however, I was astonished to see, sticking out from the cliff face, a number of tubular structures, resembling a small water-tap. Inspection of these tubes showed them to be built up of small grains of earth roughly attached to each other, forming a kind of filigree pattern. Instead of opening direct into the cliff face, the entrance of the burrow was, therefore, at right angles, and, looking directly at the cliff face, it was impossible to see the opening, while the irregular construction of the tubes caused them to blend very closely with the general surface.

Bees were very busy entering and leaving the tubes, and numbers were at work on the construction, which was in all stages from a single course round the burrow to complete tubes. Watching one of those which had only a few courses laid, I saw the bee emerge backwards and pass a pellet of earth, glistening with moisture from salivation, to her hind feet, with which she pressed it in position on the wall, where it dried instantly in the hot sun. In a few moments she appeared with another pellet, and so rapidly did she work that, within two or three hours, the tube was projecting over an inch and being gradually curved downwards. I noticed that, while some of the tubes were being lengthened, others were getting shorter, bees being at work taking the walls down and carrying the earth inside.

The purpose of these tubes seems quite obvious when one knows that these bees are much pestered by a species of parasitic

insect of the same order, many of which were, as a matter of fact, busy searching the face of the cliff and entering the burrows. These parasitic bees, scorning the work of constructing burrows, deposit their eggs in the nests of the *Anthophora* as the *Apathus* does in that of *Bombus*. It was quite obvious that the projecting tubes, with the entrance beneath, considerably baffled these intruders, who rarely entered a completed tube, but made their way readily into the obvious holes in the cliff. In these, of course, the *Anthophora* was working, and they contained no provision. Moreover, the attachment of the earth in this manner is a great economy, because, while serving the purpose of a screen, the earth is in the handiest possible place for use when it is needed for making the cell partitions and finally sealing the burrow.

Visiting the place two days later, I found the bees had disappeared completely, and the cliff bore practically no trace of their operations.

The procedure of these bees and of the allied genus *Osmia*, of which several species are found in Britain, is much the same, except for the construction of the tube, which is apparently confined to this species. Otherwise, all make a tunnel into the cliff or wall, which does not, however, run directly at right angles, but, after entering a little way, turns sharply, so that the cubicles of the larvæ lie parallel with the cliff face. A mass of pollen and honey having been deposited at the end of the burrow, grains of earth are formed into a partition to divide the cells, in each of which one egg is deposited with an adequate supply of food. Finally, the burrow is filled up and the larvæ left to undergo their transformations in safety. One species of *Osmia*, which in sandy districts builds in this manner, selects old posts or soft-wooded trees such as willow when living in an argillaceous neighbourhood, and thus forms an interesting link between the Mason-bees and Carpenter-bees, *Xylocopa*, whose work is of an exceedingly interesting character.

V

Most of the Carpenter-bees are tropical species of considerable size and beauty, but one or two are found in Europe, quite plentifully, in fact, in the South and East. The Violet Carpenter-bee is a very handsome insect, as large as a Bumble-bee and of somewhat similar build, but only slightly hairy, its body and wings being of rich metallic blue, with a violet tinge. The jaws of this bee are exceptionally well developed, and it frequently makes its tunnels into hard new wood, the timbers of house-roofs being favourite places. Like the other solitary bees, the female makes her burrow angular, the bulk of it lying

parallel with the outer surface. This burrow is fully half an inch in diameter and a foot in length, taking several days to construct and several more to provision. The consequence of this is that the cells at the farther end are completed and provided with their occupants several days before the last, and, therefore, hatch earlier. It being manifestly impossible for the creature to leave its cell through those occupied by yet undeveloped pupæ, the parent bee, in constructing the burrow, makes a second opening at the extreme end, which she seals with grains of sawdust, in a similar fashion to that in which she makes the partitions between the cells. As the bees leave their cocoons, therefore, the way is clear for their exit, except for the sealed end, which is no serious obstacle, even to their unhardened jaws.

The construction of the sawdust partitions is worthy of notice, because it is almost precisely the same as that which the honey-bee adopts to form cappings over the cells, a ring of material being first fastened to the side-walls and other grains added on the inside of this, so that, working spirally inwards, the work is finished at the centre.

Between these two extremes of the Mason-bees building in the simple soil, and the Carpenter-bees tunnelling into solid wood, there are many variations of procedure, and some species save themselves a good deal of labour by utilising materials singularly well adapted for the form which, in most cases, the nests take. One species of *Osmia* builds in the interior of bramble-stems, removing the pith to form the cells. Unlike those which work in earth or wood, she does not remove the whole of the pith, but carves out separate cells, leaving *in situ*, where the partitions will come, a ring of pith, to which she afterwards adds sufficient material to fill the centre. Others make use of old shells of various species of land-snail, using each whorl as a separate cell and making partitions of wood-pulp between them.

A very different procedure, which is a specialised form of the same instinct that induces the Bumble-bee to line its nest with moss, or the honey-bee its hive with gum from trees, is that of the leaf-cutting bees of the genus *Megachile* and its allies. The common English species is pretty familiar to most people, on account of the neatness with which she cuts semicircular pieces from the leaves of roses. These pieces are used for lining the burrow, which, like that of *Anthophora*, is made in the earth. A double thickness of leaves is laid round the walls and the divisions between the cells are formed of a circular disc of the same material. Probably the advantage of these linings is that they form an obstacle to the entrance of parasitic animals which live in the soil. Without some such protection worms, for

example, would probably often destroy the nests merely by their passage through the earth, and the species which are content to deposit on the bare earth usually select cliff faces, where, as a rule, owing to lack of moisture, worms are rarely found. Some continental species make use of the petals of flowers, such as the poppy or convolvulus, instead of leaves.

It is notable that, throughout the whole family, it is a well-nigh invariable rule that each egg is segregated from its fellows. At first sight, it seems almost an unnecessary proceeding, entailing much extra labour, but doubtless it ensures a larger percentage of the brood reaching maturity. It is well known to all beekeepers that the larvæ of the honey-bee are carefully rationed with their supplies, and many could probably consume more food than is provided. If the eggs were deposited together, the earlier hatched larvæ would, in all probability, feed more heavily, at the expense of the later ones, which would receive insufficient, while the excess consumed by the others would not be of sufficiently compensating benefit in the way of producing larger size and vigour in the adult bee.

The part played by bees in the development of flowers is so tremendous that there can be no question that the family has exercised profound influence on the form, colour, and habits of the vast majority of flowering plants. These, in their turn, effect corresponding changes in form and colour of insects of different orders. The tracing of these changes and the intricate connection between one species and another is certainly a most fascinating aspect of the study of nature.

NOTES

Nobel Prizes

Twenty years have now elapsed since the great Nobel prizes were instituted in Stockholm by the wise legacy of Dr. Alfred Nobel. The prizes amount to something approaching eight thousand pounds each, and are given every year for the following subjects. The Peace Prize is allotted at Christiania by the Norwegians. It will interest our readers to have a list of the recipients of the literary and scientific prizes.

Year.	Physics.	Chemistry.	Physiology and Medicine.	Literature.
1901	Wilhelm Conrad Röntgen.	Jacobus Henricus van't Hoff.	Emil Adolf von Behring.	René François Armand Sully Prudhomme.
1902	Hendrik Antoon, Lorentz and Pieter Zeeman.	Emil Fischer.	Ronald Ross.	Theodor Mommsen.
1903	Henri Antoine Becquerel, and Pierre and Marie Sklodowska Curie.	Svante August Arrhenius.	Niels Ryberg Finsen.	Björnstjerne Björnson.
1904	Lord Rayleigh (John William Strutt).	Sir William Ramsay.	Ivan Petrovič Pawlow.	Frederi Mistral and José Echegaray.
1905	Philipp Lenard.	Adolph von Baeyer.	Robert Koch.	Henryk Sienkiewicz.
1906	Joseph John Thomson.	Henri Moissan.	Camillo Golgi and Santiago Ramon y Cajal.	Giosuè Carducci.
1907	Albert Abraham Michelson.	Eduoard Buchner.	Charles Louis Alphonse Laveran.	Rudyard Kipling.
1908	Gabriel Lippmann.	Ernest Rutherford.	Paul Ehrlich and Elie Metchnikoff.	Rudolph Eucken.
1909	Guglielmo Marconi and Ferdinand Braun.	Wilhelm Ostwald.	Theodor Kocher.	Selma Lagerlöf.
1910	Johanns Diederik van der Waals.	Otto Wallach.	Albrecht Kossel.	Paul Heyse.
1911	Wilhelm Wien.	Marie Sklodowska Curie.	Allvar Gullstrand.	Maurice Maeterlinck.
1912	Gustaf Dalén.	Victor Grignard and Paul Sabatier.	Alexis Carrel.	Gerhart Hauptmann.
1913	Heike Kamerlingh Onnes.	Alfred Werner.	Charles Richet.	Rabindranath Tagore.
1914	Max von Laue.	William Theodore Richards.	Robert Bárány.	—
1915	W. H. and W. L. Bragg.	Richard Willstätter.	—	Romain Rolland.
1916	—	—	—	Werner von Heidenstam.
1917	Charles Glover Barkla.	—	—	Karl Gjellerup and Henrik Pontoppidan.
1918	Max Planck.	Fritz Haber.	—	—
1919	Johannes Stark.	—	Jules Bordat.	Carl Spittelar.
1920	Charles Edouard Guillaume.	—	August Krogh.	Knut Kamsun.

A Shakespeare Theatre (Sir R. Ross)

The proposal to institute a national theatre for the performance of Shakespeare's plays and other great dramatic works has been before the British public for many years, but always with no results. Everyone is familiar with Sir Frank Benson's fine efforts in this direction; and at a lecture on Shakespeare delivered before the British Academy by Mr. John Masefield on the 27th of April, he reiterated the hope that this proposal would mature some day. He said that we take great interest in all the dead matters concerning Shakespeare—where he lived, his business affairs, etc.—but that we did nothing to honour him in the proper way, namely, by providing a place where the public may be educated by seeing his works performed in their entirety. British apathy towards all intellectual effort is an extraordinary and disquieting phenomenon. Some years ago I suggested to Mr. Fisher, the Minister of Education, the advisability of instituting, solely in the interests of education, a House of Poetry where readings of our great poetical literature could be given for all to hear. The cost would have amounted, let us say, to one thousand pounds a year; but Mr. Fisher turned down the proposal, while the sum which we are now spending for the alleged education of the British public has, I understand, doubled or trebled in the last few years. The fact is, that our education is based upon wrong principles: we teach our public to be little men, and not great men; we hold no real ideal before them; we teach them little more than what any young people can teach themselves if they take the trouble; and the fact remains that the vast mass of the public are uneducated. The real great educators are not the schoolmasters but the poets, the men of science, and the historians; and nine-tenths of the vast sums expended upon our education-fallacies are really more or less wasted, except as regards the primary education of small children. Frenchmen and Germans are better educated, because more rationally educated. English people are becoming exceedingly dull people, interested only in games and party politics; and they will not be able to hold their own in the world much longer unless they look at things from a higher point of view.

Mount Everest

We mentioned in SCIENCE PROGRESS of January 1921 that proposals had been made to ascend Mount Everest in the Himalayas, the highest mountain in the world. Reports in the *Times* as to the progress made are now frequently appearing. Though Mount Everest is just visible from Darjeeling behind the mighty Kinchinjunga, the enterprising mountaineers have to make a long round before they are able to approach it with any hope of success, because they must ascend it from the northern, that is, the Tibetan, side. By June last they had reached as far as a place called Tingri Dzong. From this part of the country Mount Everest stands out all by itself, "a wonderfully shaped peak towering several thousand feet above its neighbours, entirely without rival." That is not what had been expected. Apparently it is very inaccessible even from this direction. Colonel Howard-Bury gives some interesting details of the Buddhist convent at Shekai Dzong. The altitudes may be imagined when it is understood that the Tibetan plains here are themselves as high as the summit of Mont Blanc, and that Everest soars the same distance still higher above them.

A Growing Cancer

Those who possess the laudable desire to see our own country always as great and as prosperous as possible view with disquiet what seems to be a growing tendency amongst our countrymen to abuse their fatherland on every occasion, to find fault with everything that our Government does, and to accuse our nationals of crimes which they excuse in our enemies. Criticism

of one's own country for the purpose of remedying abuses is absolutely justifiable *except* in one case—when that criticism helps the hand of our own country's enemies. In this case the critic, however strong his convictions may be, is bound by duty to keep silence; and when numbers of persons have so lost their sense of patriotism as to break this rule we cannot help feeling that there is danger ahead for the whole country. The world exists to-day, and *advances*, in a state of conflict. That is a law of nature, of evolution, of human society. The great virtues which we commend—courage, truth, loyalty—were built up by ages of conflict; and the nation that does not possess them to-day will inevitably sink.

Nothing disgusted the masses of the British public so much during the war as the efforts of pro-Germans and pretended pacificists to help our enemies in that great conflict; and now that a similar conflict is being waged by this country against the most monstrous criminal conspiracy against it ever devised, the same class of people come forward to abuse their countrymen and to attempt to justify their countrymen's enemies. Is it going to be the quality of Britons that we shall always be our enemies' friends and our friends' enemies? The most recent case was that of a vile letter written by a number of clerical people to the Prime Minister, insinuating that the Crown forces in Ireland had been guilty of crimes similar to those which they are trying to suppress. But, in our opinion, this insinuation is quite eclipsed by a general proposition which these same ministers of so-called religion advance in their letter, namely, that: "We cannot regard the cruel and detestable outrages which have given rise to the whole reprisals' policy, authorised and unauthorised alike, as a mere outbreak of wanton criminality in the ordinary sense." In other words, these ministers of religion would have us believe that anyone who thinks he has got a political grievance is entitled to murder other people. The whole country abhors this attitude, and would like to see those who advocate it soundly punished, and Mr. Lloyd George punished these no-gentlemen, at least in words. "What amazes me," he says, "is that a body of responsible men, eminent leaders of the Church, should state publicly that Sinn Fein has some kind of justification for murdering innocent men in cold blood because its novel and extravagant ideals have been denied."

So far from a political object being an excuse for any crime, it is, in fact, just the opposite. We may perhaps excuse a wronged man murdering him who has wronged him, or even a starving tramp murdering somebody in order to obtain a little money; but when some rascal assassinates another person merely for a difference of opinion surely his crime is the worst that can ever be committed.

Persons who commit such crimes have become the curse of the whole world. We cannot forget how the great war was precipitated by the similar crime at Sarajevo. That persons who are really little higher than lunatics should have the power to push all humanity into turmoil is amazing, and is due, as the vast majority of people now believe, to the laxity with which such fools or villains are dealt with under the pretext that their crimes are political. We should like to see legislation introduced making it a criminal action even to suggest force for the settlement of political questions; and, indeed, even to suggest that the use of such force is in any way excusable. Every civilised nation should adopt similar legislation, and begin to control the minute proportion of the population, consisting of madmen, who do such actions. We quite agree with the Prime Minister that this letter of "religionists" is "subversive alike of order and government, morality, and the Christian religion."

How to Encourage Science (R.R.)

On July 20 Mr. Frank Briant, J.P., Member for North Lambeth, asked Mr. Arthur Balfour, Lord President of the Council, the following question:

"Whether, in view of the vital importance of medical research to the health of the nation, the prevention or cure of disease and the alleviation of pain, there can be provided a fund from which can be paid awards for discoveries or inventions which contribute to the general health of the community and which are placed gratuitously at the service of the public, pensions to those who have become totally or partially incapacitated in the course of research, and pensions to dependents of those who have directly or indirectly [suffered] as a result of their devotion to research?"

Mr. Balfour replied:

"I see no ground for differentiating medical from other forms of scientific research which may be of equal value to the community, and I doubt whether any system of pecuniary rewards would, in the long run, be beneficial to science or medicine. The difficulty of apportioning merit for even the greatest of discoveries is often overwhelming; monetary rewards would lead to jealousy instead of co-operation among research workers, and might prove to be an incentive to work for results which are sensational rather than for the advancement of scientific knowledge. The question was fully discussed at a deputation which I received on March 7, 1920, at the Privy Council Office."

On this Mr. Briant asked:

"Does the right hon. gentleman recognise that in one branch alone, the research into the value of X-rays, a great number of medical men have lost their lives, and their dependents get no recognition from the nation?"

To which Mr. Balfour replied:

"I believe there were in the early days of X-ray investigation very serious results to some medical men, who lost their lives. Of course, their dependents are eligible for pensions. The Royal Bounty Fund is available for this purpose. Whether the Royal Bounty Fund is large enough is a question on which I am not competent to give an answer."

Obviously these arguments will not bear analysis. There are good grounds for differentiating medical from other forms of scientific research, at least in many cases. Medical discoveries are the work of professional men who are often not in receipt of any kind of subsidy or professorial salary, as frequently the case with other kinds of scientific discoveries. The difficulty of apportioning merit may sometimes, and in very rare cases, be overwhelming, but in the majority of cases does not exist. The medical men who really add materially to our knowledge may be counted on the fingers, and their claims cannot be contested, or can easily be established by those who take a little trouble to inquire into the history; and prizes can always be divided between two or three claimants if necessary. If the difficulty of apportioning merit for pecuniary awards is so overwhelming, how is it that any awards at all can ever be given, such as university honours, fellowships of the Royal and other societies, state honours, and even prizes such as the Nobel prizes? If monetary awards would lead to jealousy instead of co-operation among research-workers, why do not other awards such as those just mentioned have the same effect? The same argument would apply against the granting of all state honours, such even as the Victoria Cross. Why particularly should monetary awards have such a bad effect, and why should any awards be incentives to work for results which are sensational rather than sound? Surely such an argument merely throws doubts on the competency of the persons who select candidates for the awards, rather than on the value of the general principle for rewarding service. When Mr. Balfour says that the question was fully discussed at a deputation which he received on March 7 (really on March 2), 1920, he seems to have forgotten the circumstances. The alleged full dis-

cussion amounted to little more than an *ex-cathedra* statement of his own views; and it appeared that he had probably never been allowed to see the previous report of the Conjoint Committee, which approached him on that occasion. There is a singular cruelty in the concluding part of Mr. Balfour's reply. The Royal Bounty Fund is, it seems, available for the beggared descendants of the nation's benefactors; but the Lord President of the Council has not gone into the question sufficiently to enable him to state whether the Royal Bounty Fund is large enough for this purpose!

The whole question is really one of national morality. Does the country wish to place itself in the position of dishonest members of the public who escape paying their fees for professional services rendered to them? At the present moment a large proportion of the public get their medical benefit for nothing on one pretext or another; is the British Empire to place itself finally amongst these people?

We would invite the comments of our readers on Mr. Balfour's reply quoted above.

On July 23 the following letter from Mr. W. Hampson, M.A., appeared in *The Times*.

"On the subject of Sir Ronald Ross's letter to *The Times*, you published a letter from Mr. Tindal Robertson, secretary of the Commission for Awards to Inventors of Devices useful during the war, in which he states that the scale for use with my invention of an improved and quicker method of localisation of foreign bodies in wounds was copyrighted by the publishers and sold at a fixed price, a suggestion calculated to lead the public to suppose that I, the inventor, have already been suitably rewarded in the sale, and to weaken my complaint that I have not been rewarded by the Commission. It is, therefore, due to me to publish the following statement: This copyright was arranged without my knowledge; I had no contract to receive any pecuniary benefit, and I did actually receive no consideration of any kind for my invention, which was freely open to the public and the soldiers to use, and was very widely used in the great military hospitals. While, therefore, an invention in a technical and recondite branch of science which was declared by the highest authorities to be extremely ingenious and of great utility and widely employed met with rejection, the Royal Commission gave a considerable award to the inventor of an improvement in nosebags for horses!"

Burning Mosquitoes (R. R.)

The Chief Sanitary Inspector of Cyprus, Mr. Mehmet Aziz, has sent me the following interesting note in connection with his long-continued efforts to reduce malaria in the island:

"The anti-malarial campaign which Sir Ronald Ross recommended after his visit to Cyprus in 1913 was followed by an annual decrease in malarial fever until last year, when a serious outbreak occurred at Famagusta. Nearly all the surface waters in Cyprus dry up towards the end of May, but last year there were exceptionally late rains and the large fresh-water lake near Famagusta did not dry up as soon as usual. About the end of May 1920 I was informed that the neighbourhood was infested with mosquitoes and that a search for Anopheline breeding-places had been made without success. I visited Famagusta, and found a great number of Anopheline mosquitoes sheltering in caves and in the underground cisterns of the old fortress, but I could not at first discover where they were breeding. The lake seemed to be the only possible place, and, after a long search, I found, towards the centre of the lake, Anopheline larvæ in enormous numbers.

"The lake is, unfortunately, a difficult breeding-place to deal with, and I decided to try to destroy the adult mosquitoes by burning them in the caves and cisterns, which are very numerous in this locality.

"I sprayed benzine on dry grass in the caves and at the entrance of the caves and set it on fire. Sometimes lighted torches were moved quickly close to the resting mosquitoes. Next day the caves had cooled and seemed to be just as full of mosquitoes, and they were again burnt. This was repeated daily for about a fortnight in some thirty caves and cisterns, when no more mosquitoes were found. In the meantime, pumps to empty the lake were worked day and night, larvicide was used, and weeds were cleared.

"In Cyprus the principal breeding-season of mosquitoes is usually limited to April and May, and the destruction of adult Anopheline mosquitoes, if they are found in large numbers, as they were at Famagusta last year, seems to be a valuable anti-malarial measure. The caves formed natural traps, and it was an easy matter to burn the mosquitoes in them."

Research Defence Society

The Research Defence Society has made a change in the distribution of its quarterly reports. They have hitherto been sent only to Members and Associates, but are now being forwarded to the chief public libraries, and may be obtained through any bookseller or newsagent. The Society hopes to be able to publish them bi-monthly instead of quarterly. The Report of April of this year contains an account of the hospital stalls in the Efficiency Exhibition at Olympia in February, which, the Society says, pleased the public greatly. Six short lectures were given there with success, three by Dr. W. B. Tuck, D.Sc., on chemical research as a foundation-stone of general medicine, on industrial health, and on pharmacology; and three by Professor Lazarus-Barlow, on fluorescence and ultra-violet light, X-rays and their uses, and radium.

Income-tax Abatement (A. G. Church)

One of the first matters to receive the attention of the Economic Aims Sub-Committee of the National Union of Scientific Workers was the position of salaried professional members relative to the Income-tax Laws. It was found that the salaried professional worker was at a disadvantage, relative to the Income-tax Surveyors, in comparison with scientific workers in private consultative practice, and manual workers and other wage-earners. Before the war, the matter was not given any prominence because the average amount realisable from abatement on account of expenses was not worth the expenditure of time and energy in pressing the claim, scientific bodies were apathetic, and individual scientific workers were unaware of the concessions which had been made to other classes of the community.

The post-war position of salaried workers in research institutions and universities, the continual depreciation of salaries, with the rise in the cost of living, the ever-increasing expense of materials, renewals of apparatus, and books, and the alarming increase of the income-tax rates, caused scientific workers to regard income-tax assessment in a new light and to seek for relief from undue burdens. Certain members of the National Union of Scientific Workers forwarded claims for abatement on account of expenses incidental to their occupation to their local surveyors, and in some cases they were successful in obtaining relief. But there was no uniformity of treatment of the claimants by the assessors.

Accordingly, in January 1920 the National Union approached the Institute of Chemistry and the British Association of Chemists, with the object of securing joint action by the three bodies in pressing the claim of all scientific workers that certain expenses should be allowed as a charge against income in arriving at the assessment for income-tax. A joint committee was appointed, and a draft of a memorial to the Lords of the Treasury was submitted to the three bodies for their approval, and subsequently sent to practically

all representative scientific bodies in the kingdom and the heads of science faculties of the universities. A large number of replies were received, with requests to be associated with the memorial; in some cases information was sent which proved of the greatest assistance to the joint committee in presenting the case to the Treasury.

In June 1920 a letter was addressed to the Chancellor of the Exchequer, enclosing a copy of the memorial with a list of signatories, requesting that a deputation be received by him at an early date. Mr. Chamberlain finally arranged that a deputation should be received by the Inland Revenue Commissioners on Friday, December 10, 1920. The deputation, introduced by Professor Leonard Bairstow, O.B.E., F.R.S., President of the National Union of Scientific Workers, were received by Sir Richard V. N. Hopkins, K.C.B., Commissioner of Inland Revenue, Mr. C. C. Gallagher, and Mr. C. G. Spry.

Sir Richard Hopkins asked the deputation to state their case, after which the Commissioners were prepared to enter into a discussion on the points raised, without prejudice to the decision of the Treasury. Professor Bairstow made an introductory statement, in which he explained the scope of the Union and the economic position of the majority of its members. He explained that the National Union was the most typical body, because it represented all grades of qualified scientific workers in all branches of science, the standard of qualification being a university degree. The salaries of its members ranged from the lowest taxable limits to about £3,000 per annum, and had not been increased in proportion to the cost of living. Present conditions of the law and the great differences of practice in regard to the treatment of claims for abatement both pressed hardly on scientific workers, and now the income-tax was so high it had become a matter of urgency to secure uniformity of treatment for all classes.

The six points of the memorial were then dealt with, viz.:

I. Subscriptions to Scientific and Technical Societies and Libraries, and to Scientific and Technical Periodicals.

II. Purchase and Renewal of Scientific and Technical Books, Instruments, Apparatus, Chemicals, and other Materials.

III. Rent and Expenses of Study and/or Laboratory.

IV. Travelling and other Expenses incurred in attending Scientific Meetings.

V. Provision of Special Clothing for Work and Renewal of Clothes damaged in course of employment.

VI. Other Expenses incurred in the Course of Research and the Preparation of Scientific Memoirs.

These were considered from two points of view:

1. The necessity of the expenses incurred from the point of view of—
   (a) Personal efficiency.
   (b) Demands of employers and terms of contracts.

2. Inequality of treatment—
   (a) Of salaried scientific workers as compared with manufacturers, manual workers, and consultants in their own and other professions.
   (b) In different districts, by local assessors.

Replying to the members of the deputation, Sir Richard Hopkins spoke most sympathetically of the value of the work of those engaged in scientific research to the community. He was not convinced, however, that amelioration of the conditions of the scientific workers should take the form of concessions with regard to income-tax. It was the obvious duty of the Commissioners to deal impartially with all classes of the community under the existing laws; but in principle there was no distinction drawn between

manufacturers, wage-earners, and salaried workers as regards the expenses allowable as a charge against income. It was, however, open to the societies represented to produce a formula for the amendment of the existing law, which would receive the attention of the Commissioners. (This course is now being pursued.)

It appears, therefore, that the following expenses of salaried workers may, under certain circumstances, be considered as coming within the scope of the existing income-tax laws as regards claims for abatement, provided that they can be proved to be " wholly, exclusively, and necessarily connected with employment ":

1. Subscriptions to scientific bodies, such as the Institutes of Chemistry and of Civil, Mechanical, or Electrical Engineers, since membership of these institutions constitutes a professional qualification, and is often a condition of employment.
2. Renewals of books and apparatus and purchase of materials.
3. Travelling expenses to fulfil a single professional engagement, such as an examination.
4. Provision of special clothing.
5. Other expenses involved in research, where such research is essential to the holding of a post, and explicitly stated to be so in the terms of agreement or letter of appointment of an employee.

From reports received, it is evident that such claims are now receiving more favourable consideration from the local assessors.

The Income-tax Payers' Society

A Society with this name has been formed under the Presidency of Lord Inchcape, and with a large number of leading men as members of the General Committee. The object is to look after tax-payers' interests and to advise them when necessary. The subscription is only five shillings. All inquiries should be addressed to the Secretary of the Society at Iddesleigh House, Westminster, S.W.1. Men of science may be interested in this matter if their incomes are large enough to pay any income-tax at all.

The Burton Memorial Fund

The Burton Memorial Fund Committee are soliciting subscriptions, and have requested that their appeal shall be made known through the medium of SCIENCE PROGRESS. We publish it, therefore, in its entirety.

" At a meeting recently held at the Royal Asiatic Society it was decided to celebrate the birth-centenary of the late Sir Richard F. Burton, by the institution of an Annual Memorial Lecture, by a medal bearing his effigy, and in other suitable ways. Recent correspondence in the Press has proved, if proof were needed, that deep and wide-spread interest is still taken in one who was among the foremost men of his generation. And what were Burton's claims to fame ? Above all, he was a great pioneer. He led the way as an explorer of the first rank. He also studied his fellow-men profoundly, and, by his marvellous interpretation of the inner life and literature of the Arabs and other races, and his unsurpassed linguistic powers, helped to bridge the gulf between East and West for those who would cross it. He was the moving spirit in founding the first Society for the study of anthropology in this country. But perhaps he appealed most to the world by the daring of his journeys to Mecca and to Harar, the Unknown ; by his intense sympathy for the weak ; by his contempt for cant and sham ; by his romantic character, and by the many indefinable qualities that constitute genius. It is the privilege of the present generation to raise a memorial to this great pioneer, and thereby to secure that Burton's spirit and Burton's vision shall inspire

generations that are yet unborn to emulate his splendid deeds, and thereby to guard a priceless possession of our race."

Those wishing to contribute should send to the Manager, The National Provincial Union Bank of England, Union Bank Branch, Oxford, England. Cheques should be made payable to R. Campbell Thompson, Esq., and crossed "Burton Memorial Fund."

The American Journal of Tropical Medicine

The growth of tropical medicine during the last twenty years has been one of the most remarkable developments in the history of science. Possibly this is one of the very few medical advances in which British medical men have led the way, though even here the names of Laveran and Blanchard challenge comparison with those of British leaders. Innumerable societies of tropical medicine have been established throughout the world, and that means many publications. The admirable *Tropical Diseases Bulletin* published by the British Colonial Office, summarises the outflow in an excellent manner. There are many British, French, Italian, and German periodicals; and now we must welcome the *American Journal of Tropical Medicine*, of which the first number appeared last January. It is published bi-monthly by Messrs. Williams & Wilkins & Co., Baltimore, U.S.A., and is the official organ of the American Society of Tropical Medicine. We wish it all success.

Notes and News

The Honours List published on the occasion of the King's birthday did not contain any names which need be mentioned here.

At the meeting of the Royal Society of Edinburgh, held on July 4, the following honorary Fellows were elected:

British—William Henry Perkin, F.R.S., Waynflete Professor of Chemistry in the University of Oxford; Sir Ronald Ross; Sir Ernest Rutherford, Kt., F.R.S., Cavendish Professor of Experimental Physics in the University of Cambridge; Sir Jethro J. H. Teall, Kt., F.R.S., lately Director of the Geological Survey of Great Britain and of the Museum of Practical Geology.

Foreign—Reginald Aldworth Daly, Professor of Geology, Harvard University, Cambridge, Mass.; Johan Hjort, Director of Norwegian Fisheries, Bergen; Charles Louis Alphonse Laveran, Nobel Laureate, Medicine 1907, Paris; Heike Kamerlingh Onnes, Nobel Laureate, Physics, 1913, Leiden, Holland; Salvatore Pincherle, Professor of Mathematics in the University of Bologna.

Dr. Alexander Carrel of the Rockefeller Institute, has been elected a national associate of the Paris Academy of Medicine.

The Franklin Institute has conferred its medal and certificate of honorary membership on Prof. Charles Fabry for his studies on light radiation.

It is announced in the daily press that Prof. E. Branly is the selected candidate for the Nobel prize for physics 1921.

Dr. W. M. Hicks has been awarded the Adams prize of the University of Cambridge.

It is proposed to appoint Prof. H. Lamb to an honorary university lectureship at Cambridge. It will be known as the Rayleigh lectureship in mathematics.

Dr. E. K. Mees, now research chemist to the Eastman laboratory, has been awarded the D.Sc. degree by the University of Rochester, U.S.A.

Prof. J. W. Nicholson has been elected President of the Röntgen Society for next session, and Dr. A. Smith Woodward President of the Linnean Society.

The following is a list of some of the well-known scientific men whose decease has been announced during the last quarter: J. Elster, physicist;

William Warde Fowler, naturalist; Dr. A. M. Kellas, chemist and mountaineer; Viktor von Lang, formerly professor of physics at the University of Vienna; Prof. G. Lippman, For. Mem. R.S. physicist; J. A. Menzies, professor of physiology at the School of Medicine, Durham University; Dr. Edward Bennett Rosa, chief physicist of the Bureau of Standards, U.S.A.; W. Shackleton, astro-physicist; H. R. Le Sueur, chemist; Abbott H. Thayer, artist, well known for his studies on the protective colouring of animals; Prof. H. W. G. von Waldeyer, anatomist.

Nature (August 11) gives a list of some well-known German scientists who have died since 1914. Included in it are: 1914, W. Hittorf, physicist: 1915, A. von Könen, geologist; E. Riecke, physicist; P. Ehrlich, physician: 1916, R. Wedekind, mathematician; E. Mach; K. Schwarzschild, astronomer; R. Helmert, mathematician and physicist: 1918, G. Cantor, mathematician; E. Hering, physiologist.

The Council of the Society of Chemical Industry has decided to use part of the income from the Messel bequest to institute a memorial lecture in memory of the donor. The lecturer will receive a gold medal and an honorarium. For the present the remainder of the income from the bequest will be allowed to accumulate.

The committee organised in 1911 by the late Prof. MacGregor to found a second chair of Natural Philosophy in the University of Edinburgh as a memorial to Prof. Tait has now almost finished its work, and the Tait chair will be established in the near future.

Among the Civil List pensions granted in the year ending March 31 last were the following: Mrs. J. A. McClelland, in recognition of her husband's distinguished services as an investigator in physical science, £100; Mr. J. N. Fitch, for his work in botany, horticulture and natural history, £75; Mr. Herbert Tomlinson, in recognition of his services as a teacher and of his valuable and distinguished contributions to physical science, £100.

The British Empire Cotton Growing Corporation will receive from the Government a lump sum of £1,000,000 to assist the development of the cotton-growing industry within the Empire, instead of the £50,000 per ann. for five years, as originally arranged. The money now allotted represents one quarter of the profit made by the British and Egyptian Governments from their joint control of the cotton supply during the war.

In spite of the fact that Physics is the most English of all the sciences, we have not until now had an opportunity of recording a donation to this branch of science in these *Notes*. It is therefore with special pleasure that we announce the gift of £200,000 from Mr. H. H. Wills to Bristol University for the building and equipment of a new Physics laboratory. It is an unfortunate fact that present-day research in Physics generally needs expensive apparatus and a well-equipped workshop. As a consequence, much of the more precise and elaborate work in Physics is being carried on in the United States, where wealth is coupled with a generosity and appreciation of the benefits of science which is almost entirely lacking in this country. A vivid conception of the difference in the resources of the physicist in England and the United States can be obtained by reading the lecture delivered before the Physical Society by Prof. A. A. Michelson last spring. In it he described the three researches he has carried out since the armistice, namely, the measurement of the earth tides, of the velocity of light, and of the size of certain stars by his interference method. This work could not possibly have been done at any English University for lack of staff and funds. It seems to have formed only a part of the activities of the Ryerson Laboratory in Chicago.

It is stated that the Birmingham University appeal brought in £285,062, together with increased grants from the education committees in the surrounding counties. Yale received $1,859,154 in gifts and bequests last

year, including \$545,729 from the alumni fund, which has 8,000 subscribers.

M. E. Deutsch de la Meurthe has given ten million francs to the University of Paris to provide for a University quarter where students may live at a moderate cost. Columbia University has received a donation of one million dollars for a similar purpose, namely to provide living accommodation for *five hundred foreign* students.

Medical education in the States continues to receive vast donations. Thus in 1919 the General Education Board provided four million dollars to the Vanderbilt University, out of which sum a medical department is to be built and equipped. The Carnegie Corporation and the General Education Board have now provided another three million dollars for the endowment of this most fortunate department. By comparison, the \$500,000, donated to the Western Reserve University by Mr. Samuel Mather for building a Medical College, seems insignificant.

The fourth Report of the Conjoint Board of Scientific Societies shows that the Board has received evidence that scientific investigation is being seriously hampered by the heavy cost involved in the publication of results. An exceptional number of papers are being communicated to the Scientific Societies, including many held up during the war, while the resources of the Societies, which have not increased, are insufficient at present prices to publish even the normal pre-war number. The country is thus in danger of being seriously handicapped at a time when re-habilitation of industry is in most serious need of scientific assistance.

Much of the Report is occupied with a short abstract of the third Report of the Committee on the Water Power of the Empire. It is shown that too little is being done to ascertain the total resources, or to secure uniformity in investigation and record. It is urged that steps should be taken to convene an Imperial Water Power Conference in London at which the various Dominions and Dependencies of the Empire should be represented. The outcome of such a Conference might well be the creation of an Imperial Water Power Board with extensive powers to carry out a comprehensive policy for stimulating, co-ordinating, and, where necessary, assisting development throughout the Empire.

The Board has also dealt with questions relating to the formation of National Research Committees, in connection with the International Research Council formed in 1919, with the collection of scientific data in the former German Colonies, and with instruction in Technical Optics. The research on glues and other adhesives, initiated by the Board as a war measure, at the instance of the Air Ministry, has now been taken over by the Department of Scientific and Industrial Research.

On April 11 last the Japanese Parliament passed a law making the use of a metric system compulsory in that country—its use has been legal since 1893.

Messrs. J. N. Brönsted and G. Hevesey, of the Polytechnic Institute of Copenhagen, who last year claimed to have effected a partial separation of the isotopes of mercury, now state (*Nature*, July 14) that they have been able to obtain a partial separation of the isotopes of chlorine by the same method. They evaporated a strong solution of hydrochloric acid at $-50°$ C. in a high vacuum, condensing the vapour formed on a surface cooled with liquid air. The condensed part of the hydrochloric acid was found to contain a larger proportion of the lighter chlorine isotope than that which remained unevaporated. This conclusion was reached by transforming the two specimens of the acid into sodium chloride and determining the density of a saturated solution of the salts so formed.

Sir Ernest Shackleton has just started from Southampton on a new expedition to the Antarctic in a 200-ton Norwegian wooden ship known as

the *Quest*. He is accompanied by six members of his former expeditions, including Capts. F. Wild, F. Worsley and J. R. Stenhouse, Dr. A. H. Macklin and Mr. L. Hussey, and will carry on oceanographical and meteorological work throughout the voyage. To this end he is provided with a full equipment for deep-sea soundings, pilot balloons, and a specially-constructed Avro seaplane. The main object of the expedition is the exploration of the Antarctic coastline between Drygalski's Wilhelm Land and Bruce's Coats Land, the only portion within these limits known with any certainty being Enderby Land, which was discovered in 1831, but which has never yet been visited. It is also hoped to visit a number of isolated islands (*e.g.*, Bouvet Island, which is quite unknown and said to be inaccessible) and to search for others whose existence is still doubtful, such as Keates Island and Tuanaki in the South Pacific. The expedition is financed by Mr. J. Q. Rowett and has received considerable support from Mr. F. Becker. It will be known as the Shackleton–Rowett Oceanographical and Antarctic Expedition.

The Annual Report for 1919 of the New South Wales Technological Museum, which has just reached us, shows that those in charge of the Museum do their best to make it of real assistance to the industrial life of the continent. A notable instance is provided by the building trade, which seems to have been quite ignorant of the beauty and durability of the Australian marbles until attention was drawn to them by the publication by the Museum authorities of a volume on Building and Ornamental Stones. The result has been that the bulk of the marble used in the continent is now obtained locally, while before it was all imported. The policy of the Museum staff has been to carry out experiments and tests in anticipation of commercial demand and not merely when such demand has arisen. Then, when inquiries are made, full economic data are furnished, and no time is lost in the placing of the goods on the market. As soon, for instance, as a new marble is located, specimens are at once procured, tested, labelled, and placed in the collection; again, when a scarcity begins to develop in a building or cabinet timber, this Museum has ready efficient substitutes; and this policy obtains right through the whole of the economic resources of the State in particular and Australia in general. The report contains some interesting photographs of many of the original models of monoplanes made by Laurence Hargrave about 1890–91. These were driven by compressed air and bear a remarkable resemblance to the early types of the modern aeroplane—even to the revolving cylinder engine. A Hargrave box-kite is preserved in the Museum; but his classic collection of box-kite models was secured by Germany, although it had been offered to the N.S.W. Government and accepted by them.

The Ministry of Health reports that, as a result of the Burns Exemption Act, the number of unvaccinated children has increased from 39,925 in 1901 to 277,558 in 1919. Thus only 40·6 per cent. of the children born in the country are now being treated. That this will ultimately lead to a serious epidemic is shown by the present figures for the Nottingham outbreak: 50 cases, 45 of them in unvaccinated persons. Dr. Drury in the July number of *The Fight against Disease* refers to an even more remarkable outbreak at Ossett in 1902. Smallpox was introduced into the school by one of the children, and as a result 37 out of 77 unvaccinated children caught the disease, and only 5 out of the 92 vaccinated ones. Of these 5, not one had been treated within the previous ten years. In the class into which the disease was first introduced all the unvaccinated children fell ill, and not one of those who had been vaccinated!

We have received from the Director of the Science Museum, South Kensington, a copy of the second edition of the classification of works on Pure and Applied Science in the Science Library. The sub-division of the various subjects by means of the familiar decimal indexing has been carried out thoroughly and adds considerably to the value of the book, though, to

be quite candid, we doubt whether it would be worth 18*s.* to very many people! (Paper covers, H.M. Stationery Office, 1921.)

Bulletins Nos. 19, 20 *and* 21 of the *Institute of Science and Industry, Australia*, deal respectively with Wood Waste, the White Ant Pest in Northern Australia, and with Power Alcohol. The first of these, by Mr. I. H. Boas, M.Sc., contains a most exhaustive account of the possible ways of utilising the waste product of the saw mills, which amounts to about one-and-a-half million tons per year in Australia. In the United States the Forest Service has organised a highly-successful Wood Waste Exchange to bring producers and buyers into contact with one another. The purposes to which this waste —either in the form of trimmings, edgings, etc., or of sawdust—are manifold; Mr. Boas gives a list of forty-one for sawdust alone, including such diverse usages as circus rings, soap-making and the manufacture of alcohol and viscose. This last industry had grown to enormous dimensions in the States; for example, in 1919 there were exported fifteen million pairs of silk stockings made from wood pulp. The report embodies the results of experiments made to determine the suitability of typical Australian woods (*e.g.*, Jarrah and Karri) for various purposes, and should result in the establishment of a number of new and remunerative industries in the continent. *Bulletin No.* 20 is a reprint of *No.* 6, with an addendum summarising the work which has been done since January 1918, when the report was first published. It is clear that the petrol position is rapidly becoming critical; the consumption in the U.S. in September 1920 was about fourteen million gallons *per day* or two millions in excess of the production in that country. Experiments have shown that power alcohol is satisfactory as a motor spirit and that, with suitable priming, any ordinary motor-car engine can be started from the cold. Unfortunately, however, the ideal source of this alcohol has yet to be discovered and the only progress made in Australia has been distinctly negative for the yield obtained from sorghum (the source favoured in the first report), by the methods so far employed, has been disappointingly low. The third *Bulletin* contains some preliminary observations on the *Mastotermes Darwiniensis* and other Termites in Northern Australia. These insects are enormously destructive (it is stated that they have even penetrated a leaden water pipe!) and at present there seems no way of eradicating them, hence the chief recommendations in the report are concerned with methods of rendering the materials most frequently attacked termite proof.

We have received from the Oxford University Press a copy of *No.* 7 *of the Harvard Bulletins of Education*, which contains an account of the methods employed in the "sight-saving" classes in the schools of certain American cities. These classes are intended for children who, while not being blind, have eyesight either so defective as to seriously handicap them in ordinary class-work or such as is likely to be still further impaired by continual use of ordinary school-books. It is found that one in every 1,000 children in the large towns and one in every 500 in the smaller ones fall within this category. Previous to 1913 these children either had to do their best in the normal classes or were sent to schools for the blind. In either case the child suffered, for amongst ordinary children he appeared a dunce, and at a school for the blind endeavoured to read Braille by sight in class-rooms where no attention at all had been paid to lighting, either natural or artificial. A sight-saving class is a class held in a properly lighted room by a special teacher, whose duty it is to help the children to keep pace with those enjoying normal eyesight. Special text-books are used, printed in twenty-four point heavy type, and most of the written work is done on blackboards, or, with pupils above the fourth grade, on typewriters, using the touch method. Whenever possible (*e.g.*, for all oral work) these weak-sighted children work in the same class as their fellows; this guards against the danger of a lower standard being set for them than for the others. Finally, it may be noted that precautions are taken

that those who receive this rather expensive treatment at school do not, at home, defeat its purpose by reading ordinary print or by attending the cinema!

The *Reprint and Circular Series of the National Research Council, U.S.A., No.* 11, contains a survey of the outstanding research problems in geophysics, prepared by the chairman of the several sections of the American Geophysical Union. The chapter on Geodesy, by William Bowie, deals first with the errors of geodetic surveys which start from different astronomic stations. These are due to errors in the position of the plumb line caused by inequalities of the earth's surface, and make it most desirable to base the survey of any given area (*e.g.*, of Europe) on a single datum. It also reviews the present position of the Theory of Isostasy, which was first put forward by Archdeacon John H. Pratt in the late 'fifties of last century. This theory supposes that under mountain masses there are deficiencies of matter approximately equal in quantity to the masses above sea level, while, under the bed of the ocean, there is a corresponding excess, which approximately balances the deficiency of matter in the mass of water above it. Investigations by John F. Hayford, Chief of the Division of Geodesy of the U.S. Coast and Geodetic Survey, published in 1909, showed that the depth affected by this excess or deficiency of matter was roughly 122 kilometres. Further investigations, published by the Coast and Geodetic Survey in 1917 and based on gravity determinations, showed this depth of compensation (*i.e.*, the depth at and below which the materials of the earth are supposed to be in a state of equilibrium) to be 96 kilometres. These observations also showed a decided relation between the value of gravity and the underlying geologic formation. This follows from the fact that the Cenozoic formation is approximately 10 per cent. lighter than the older geological formations, while Pre-Cambrian rocks are decidedly heavier than normal. Thus pendulum observations may be a means of indicating the nature of the strata close to the surface of the ground—information which would have a considerable economic value, *e.g.*, in oil-boring operations.

The review of the problems of Seismology by Harry F. Reid contains much information of general interest. One of the outstanding problems of this branch of science is the accurate determination of the time taken by earthquake waves to pass from their point of origin to points on the earth's surface at different distances from it. When this is known, it is possible to trace out the paths and velocity of the waves through the earth and so to obtain some knowledge of the physical condition of the earth's interior. Records of earthquakes obtained at points more than about 110° from the origin have, so far, proved very unsatisfactory, and it is still uncertain whether this is due to the imperfections of the observations or, as Oldham thinks, to the existence of a central core in the earth having different physical properties from the material surrounding it. Professor Wiechert and his collaborators claim that the transmission curves indicate the existence of several shells in the earth where the velocity of propagation of the waves suddenly changes.

Among other chapters is a very important one by Louis A. Bauer on Terrestrial Magnetism and Electricity. In this branch of geophysics one of the great problems is the determination of the various systems of magnetic and electrical forces which together make up the total terrestrial field as observed at the earth's surface. It is known that the major portion of the earth's magnetic field is due to an internal system of forces; superimposed on this is an external system probably due to electric currents circulating in the earth's atmosphere and possibly also a system of vertical electric currents which pass from the atmosphere into the earth and *vice versa*. The question of the existence of these vertical currents is regarded by Sir Arthur Schuster as one of the chief outstanding problems in terrestrial magnetism. The ocean work of Prof. Bauer's department has been arranged specially for its solution

and, but for the war, the results would have been available before now. The phenomena which accompany magnetic storms provide several interesting problems. There is first their effect on the intensity of the earth's magnetic field. After the very severe storm of September 26, 1909, it was about three months before this field rose to its approximately normal value. Again, the storm on March 22–23 this year was associated with marked sun-spot activity, a remarkable display of the Aurora Borealis and with abnormal fluctuations of the solar constant. This quantity is observed every day by the Smithsonian Institution party at Calama, Chile. From January 1 to March 22 it varied from 1·93 to 2·00; on March 23 it reached the low value of 1·866, on March 24 it was 1·905, and thereafter it went up again to its normal value. The general result of magnetic observations carried on during almost every solar eclipse since 1900 has been to show that there is an appreciable magnetic effect which, though minute, is similar in character to the variation experienced by the earth during a solar day. It is believed that the detection and study of an eclipse magnetic effect will be of great importance to theories of the earth's magnetic field and its variations.

It has been said of Mr. Arthur Balfour that he is a philosopher among politicians and a politician among philosophers. We should have preferred him to be a man of science among statesmen and a statesman among men of science; but his reply to Mr. Briant on the subject of rewards for medical discovery, quoted on page 287, shows how far he is from this ideal. Can a weaker argument be conceived? On reading it everyone will ask at once, "Why then give any rewards to anyone for anything?" Why exactly should Mr. Balfour discriminate against men of science and doctors? The whole theory of rewards is that they discharge an obligation towards those who have rendered good service. Rewards honour not only the recipients but the givers. Mr. Balfour argues that because he fancies there may be some difficulty in selecting the proper recipients, therefore he is entitled to escape from all obligation in the matter!—surely an indescribable evasion. Yet the Government of which he is a part is now pouring forth millions upon millions of the public money in doles to the undeserving, the lazy, and the inefficient. When asked to pay honourably for benefits received from those who have laboured hard for the State, this Government refuses even to consider the matter!

CORRESPONDENCE

To the Editor of "Science Progress"

THE INHERITANCE OF ACQUIRED CHARACTERS

I. From Julian S. Huxley, M.A.

Sir,—May I be allowed to comment, with the utmost possible brevity, upon Professor MacBride's reply to my letter in your last issue?

1. Prof. MacBride, in the passage to which I took exception, in reality made *two* statements: first, that the mutations employed by Mendelians are pathological; and, secondly, that they are not of evolutionary importance. In his reply he has dealt only with the second; the more sweeping and unwarranted first assertion he has not attempted to defend.

2. *Eye-colour.* Various species of animals and birds, as well as different races of man, are distinguished by differences of eye-colour, which appear to be or are most easily interpreted as, Mendelian. For the question of degeneration Professor MacBride should consult the discussion by Müller.

3. *Segregation of quantitative factors.* Whenever a cross gives an F.1, which is intermediate and has a small range of variability, followed by an F.2, with a large range of variability, segregation is the only known hypothesis which will cover the facts See discussion in Morgan's *Mechanism of Mendelian Heredity*, or Baur's *Einführung in die experimentellan Vererbungslehre.*

4. *Colours of Mammals.* I accept Professor MacBride's challenge. If he will consult R. E. Lloyd's *The Growth of Groups in the Animal Kingdom,* he will find evidence of colour-variations in wild rodents which are (*a*) discontinuous and presumably segregable, (*b*) resemble the patterns of other wild species. Again, the difference between white-bellied and grey-bellied types (a common distinction in nature) is in rodents due to two members of a multiple allelomorph series; and a specific difference in coat-colour between two species of Cavia has been shown to be Mendelian, and the factor for it has actually been transferred from one species to the other.

When yellow ground-colour (as in many desert forms) and sharply defined patterns (as in Skunks) occur, the onus of proof rests on those who deny that these appearances are due to factors similar to those which produce similar effects in domesticated rodents.

5. *Modifying factors.* In certain cases these have been definitely shown to exist (Morgan, *Carnegie Publication,* No. 278, 1919, pt. iv; Attenburg and Müller, *Genetics,* Jan. 1920.)

Let me ask Professor MacBride to look at Crampton's monumental work upon the snails of the genus Partula in Tahiti (*Carnegie Publication* No. 228, 1916), and say whether his facts can be explained except by the assumption of variations which are (1) inherited; (2) accidental, *i.e.*, in no observable relation with the observed differences of environment; (3) discontinuous; and (4) of small amount—in other words, small mutations, and often modifying factors.

6. *Organic equilibrium.* When a chemist is dealing with a problem of

mass-action, he must think in terms of equilibrium; but he does not therefore deny the existence of molecules, ions, atoms, or other units. When an organism is in process of development, it presents the biologist with a problem of equilibrium—hormonic and other interrelation of parts, "Kampf der Teile," and so forth. This does not—or should not— cause us to deny the existence of unit-factors which have, as I believe, provided the basis from which the dynamic process of development starts. Professor MacBride writes almost as if he would like to prevent the geneticist from trying to think physiologically; but I hope that specialisation has not gone quite so far as yet.

7. *Guyer's work.* If there are factors in the germ-plasm concerned with lens-production; if one or all of these are of the same (or similar) composition as the proteins of the lens; then we should expect an anti-lens serum to attack these factors; and further, if the damage caused by this attack is permanent, expect that the lenses of future generations will also be affected. This appears to me to be the natural hypothesis; but I fail to see how it or any other interpretation can be used either for or against Lamarckism.

8. *Abnormalities of division.* All Morgan's recent work goes to show that irregularities of division have nothing to do with the origin of Mendelian mutations, and that the alteration is an extremely localised one, occurring in one very small area (a single factor) of a single linkage-group (on Morgan's view, a chromosome.)

9. *Weismann.* My remarks about Weismann were simply intended to relieve Professor MacBride from any necessity of slaying the slain. The continuity of the germ-plasm and the idea of determinants are important broad generalisations. But that there is no interaction whatever between soma and germ-plasm, or that regeneration is "due to" packets of reserve-determinants—such assertions are to-day only of historical interest, whether to Professor MacBride or to Professor Morgan.

With apologies for taking so much of your space,

I remain,

Yours faithfully,

JULIAN S. HUXLEY.

NEW COLLEGE, OXFORD,
May 8, 1921.

II. FROM PROF. E. W. MACBRIDE, D.Sc., F.R.S.

SIR,—Before I enter on the discussion of the points raised in his last letter, on which Mr. Huxley differs from me, I should like to set forth briefly the points on which we agree. Thus, we both believe that the chromatin of the nucleus is the bearer of the hereditary qualities, and that, therefore, if the hereditary potencies of any animal are altered, this chromatin must somehow have been affected.

I believe that the chromatin has been altered in two ways, viz. (*a*) by the slow influence on it of a changed cytoplasm, the cytoplasm itself having become modified by its reaction to changed conditions of life, and (*b*) by irregularities of division when two sister chromosomes having paired in the prophases of the meiotic or reducing division, again separate from one another. Mr. Huxley appears to think that his mutations arise like Melchisedek, causeless and unconditioned, without father or mother. I should like to call his attention to the fact that the master whom he so slavishly follows, *i.e.* Prof. Morgan, when confronted by this dilemma, is driven to admit that in the last-named resort the cause for mutations must be sought for in alterations in the cytoplasm.

The principal point in the paper which I contributed to SCIENCE PROGRESS

was this, that the slow, continuous change in the chromatin has been the dominating factor in evolution, and that the sudden mutations with which the Mendelians have chiefly experimented have not been the kind of changes which have led to the production of new species of animals, and in this opinion the best systematists whom I have consulted agree with me. An eminent icthyologist thus phrased it: " Mendelism is all right, but it has got nothing to do with evolution."

Taking now the points which Mr. Huxley raises in order:

1. He complains that I asserted that the mutations experimented with by Mendelians are pathological, and that I had not attempted to defend this statement.

I am certainly not prepared to assert categorically and dogmatically that every mutation which has been the subject of Mendelian experiment is pathological. Such an assertion could only be justified by a detailed examination of each case, but I reassert that the overwhelming majority of them are pathological, and that the language in which Mr. Huxley brings forward his so-called exceptions is so chosen as to obscure this fact. To give one instance, he mentions " size " as a quality which is certainly not pathological, but which is inherited in a Mendelian manner. Now allied species certainly do differ in size, and a slow, continuous increase in size is often the accompaniment of the evolutionary series which we find in fossils; but there is all the world of difference between cases like these and the real Mendelian mutants like the dwarf Œnothera, which are as genuinely pathological as are human dwarfs.

2. Mr. Huxley is good enough to refer me to a discussion by Müller on differences of eye-colour between allied species, which appear to be most easily interpreted on the factorial hypothesis. I need no instruction from either Mr. Huxley or Müller as to the interpretation to be placed on these differences, nor am I, nor, as I believe, are the majority of zoologists to be influenced by the arguments of Mendelians who are determined to crush everything into a Mendelian mould. Certainly the attempts to deal with the differences between human races in respect of eye-colour on the Mendelian basis have not been conspicuously successful. The so-called duplex eyes which contain brown as well as blue pigment show every gradation from the heavily pigmented eyes of the Negroes and of the Mediterranean races to cases—equally ranked as duplex—where traces of brown pigment are only to be discovered by a magnifying lens. If two blue-eyed parents give rise to blue-eyed children, then Mendelians assert they are simplex—if they happen to have a brown-eyed child then they had concealed brown pigment. This is the kind of arguing in a circle to which I referred in my last letter, and to which I strongly object.

3. Mr. Huxley reasserts that when two allied strains differing in size are crossed there is segregation in F.2 Against this I can only reiterate that the result of both Bateson's and Punnet's experiments is, that when a large strain and a small strain are crossed, though individuals of varying size are produced in F.2, the large strain is never recovered.

4. With regard to coat-colour in Mammals, I asserted that the colour differences between allied species were totally dissimilar to those exhibited by Mendelian mutants. In pity for my ignorance Mr. Huxley is good enough to refer me to a book entitled, *The Growth of Groups*, by Dr. Lloyd, for proof to the contrary. It is indeed kind of Mr. Huxley to call my attention to a work which has been on my book-shelves for eight years, and which it was my duty to review when it first appeared. This work contains the attempt of an amateur biologist, a member of the Indian Medical Service, to solve the problem of species on the basis of a large collection of rats made in India. No experiments were attempted and no new principle emerged. That amongst a large collection sporadic appearances of mutants occurred is cer-

tainly not new, nor is the occurrence of a "group" of these mutants numbering fifty collected from two or three adjoining houses a startling or new phenomenon. That such mutants when paired breed true was known long before the days of Dr. Lloyd, and that if a pair of these mutants escaped to a desert island they might give rise to what would be described as a new species must also be admitted; but they have not done so. May I refer Mr. Huxley to a still older book than Lloyd's, viz. Wallace's *Island Life*, for proof that the degree of distinctness of island forms from their nearest continental allies is a function of the time of isolation, and points unmistakably to progressive modification and not to mutation?

5. In support of his contention that specific differences are due to the accidental action of "modifying factors," Mr. Huxley refers me to Crampton's work on a Tahitian genus of land-snails. Again I must refer Mr. Huxley to Wallace. In oceanic islands, which consist essentially of volcanic cones trenched by radial valleys, it has long been known that endemic genera of land-snails exhibit an extraordinary multiplicity of species with exceedingly limited ranges. To show in each case the functional significance of specific marks is of course impossible, but let me remiud Mr. Huxley of what that Nestor of British zoologists, Sir Ray Lankester, has well said: "A character may either be directly useful or it may be the outward and visible sign of an inner physiological grace." It is infinitely more likely that the specific marks of the Tahitian species are symptoms of the constitutions that fit them to their surroundings, rather than that these differences are due to "accidental mutations," for it is not easy in the latter case to explain why one species does not spread into the area of another, and by crossing with it produce a mixed population.

6. Mr. Huxley accuses me of objecting to "geneticists" thinking physiologically. My objection is that "geneticists" as a rule do not think physiologically, but treat the bodies of animals as picture puzzles in which the parts can be shoved about and exchanged at will; but I have a further and radical objection to the tacit monopolisation of the term "geneticist" by a limited group of Mendelian experimenters. Every zoologist worth his salt is a geneticist; for the great root problem of his science is the discovery of the causes and conditions of alterations in Heredity. Mr. Huxley compares his "unit-factors" to the differences between chemical molecules, and the reactions between organs—in a word, the action of the internal environment—to chemical "mass-action." I have tried to point out that this conception is radically false, that the divergences of the Mendelian mutants from the type give no indication of the steps by which that type was built up. They are no more units than are the fragments into which the mechanism of a watch would be pounded by blows of a hammer. Such fragments would give no indication of the means by which the wheel-trains of the watch had been put together.

7. Mr. Huxley can still see no reason for using Guyer's work as a support to Lamarckism. I must say that he appears to me to be singularly dense in this respect. The essence of Lamarckism, as I have repeatedly pointed out, is the theory that the soma can influence the germ, and this theory certainly involves the assumption that the essential constituents of the organs of the body are somehow represented in the germ. This is precisely what Mr. Huxley assumes; he says: "If one or all of these [factors concerned in lens production] are of the same composition as the proteins of the lens," then the cytolysins which attack the one will attack the other. Incidentally, Mendelians always seek in this way to escape the force of evidence that acquired characters are sometimes transmitted: they maintain that the original cause which modified the body modified the germ-cells at the same time. The shallowness and far-fetched character of such assumptions has already been alluded to by me in my original article. Guyer points out that, if

we imagine that the cytolysins act only by action on the germ-cells, it is incomprehensible why these lysins, pumped into the veins of the mother rabbit, are powerless to affect any of her germ-cells ripened after the pregnancy at which the injection takes place; and he suggests the possibility that the diminished and half atrophied lenses of the daughter rabbits are producing lysins which affect their own germ-cells—and that is in essence Lamarckism.

8. Mr. Huxley states that Prof. Morgan has disproved my assumption that the production of Mendelian mutants is due to irregularities of nuclear division. This theory is, of course, not mine, but that of the leading British Mendelian, Bateson; it is the most plausible theory that has yet been put forward, and it receives support from the cytological observations of Prof. Gates on the mutants of Œnothera. It may be a mistaken hypothesis, but to say that Prof. Morgan has disproved it is a pure piece of unmitigated bluff. Neither Morgan nor any of his pupils has observed the genesis of a mutation, nor is any of them in a position to say what kind of interchange may go on between two chromosomes which meet in the meiotic pairing. May I, in return for Mr. Huxley's kindness, direct his attention to some startling and little-known experiments by Tornier on the development of the races of gold-fish, in which he strives to show that the most gorgeous Mendelian mutations, such as extravagantly elongated fins, forked tails, protruding eyes, and colour variations including in their extreme range albinism, are one and all due to the action in varying intensity of one pathological factor, *plasma weakness*, itself due to the action of evil conditions on the earliest stages of development. Many of Morgan's "mutants" will acquire a new significance when viewed in the light of these facts.

9. Lastly, Mr. Huxley graciously intimates that he merely wished to relieve me from the trouble of overthrowing Weismann, since both Prof. Morgan and he regard him as already overthrown, and Prof. Morgan can do without him. Weismann has earned the gratitude of all biologists by endeavouring to follow out to its bitter logical end the implication of a theory which denies the inheritability of acquired characters—a task which Morgan has not attempted. Morgan, who lives surrounded by experimental embryologists happily unhampered by the provisions of an antiquated Anti-vivisection Act, is well aware that the assumptions of Weismann's theory are untenable; but he is unable to put anything in their place. This is markedly so in regard to regeneration; it is all very well to dismiss in an airy fashion, as Mr. Huxley does, the assumption that regeneration is due to "packets of reserve determinants," and at the same time to affirm the continuity of the germ-plasm (a conception hopelessly at variance with recent results of experimental embryology). Such an attitude on Mr. Huxley's part merely proves that he is one of those "to whom to solve difficulties is easy, but to feel difficulties is hard."

Yours faithfully,

E. W. MacBride.

July 21, 1921.

To the Editor of "Science Progress"

THE GYROSCOPIC COMPASS

From G. T. Bennett, Sc.D., F.R.S.

Dear Sir,—A recent work with the above title has received unqualified praise from your reviewer (see Science Progress, April 1921). Being professedly non-mathematical, it may rightly escape any censure for such shortcomings as may be due to the exclusion of mathematical methods in dealing

with an essentially mathematical topic. But freedom from sheer dynamical fallacies should still be retained as a minimal standard. I submit, therefore, a short selection of errors, from my own marginal notes, solely with the hope of saving non-specialist readers from attempting an impossibly thorough comprehension of the text.

The author contemplates early (p. 5) the common horizontal precessional movement of a gyroscope with its axis level and loaded at one end. He says (truly enough) that " there is here no question of perpetual motion " ; and goes on to say that " the work expended in overcoming the friction at the vertical journals is derived from the energy of the spinning-wheel." This is entirely wrong. The friction spoken of would inevitably cause the axis to tilt and the weight to drop, regardless of maintenance of the spin. The rate of precession is then stated to be proportional to the speed of rotation of the wheel. The word " inversely " needs inserting to make the statement true.

In the description of the mounting of a gyroscope with three degrees of freedom it appears (pp. 12–14) that the author definitely supposes that the three axes of rotation must be mutually perpendicular. When one of the angles is changed it " tends to destroy one of the three degrees of freedom." This is a kinematic fallacy. The three degrees of freedom exist in all completeness so long as the axes are not coplanar. The lapse to two degrees of freedom is no gradual matter.

The eighty-five-minute period prescribed for dead-beat compasses is described (p. 37) as " the period which a simple pendulum would have if its length were equal to the radius of the earth." This error is a common one. It results probably from assuming the usual formula to hold for the period of a pendulum even when its length is comparable with the earth's radius. The length of the eighty-five-minute pendulum is infinite. The bob moves in a straight line, and the curvature of the earth alone accounts for the period.

As illustrating one type of disturbance of the compass, " the ballistic deflection of a pendulum hung from the roof of a railway carriage is dependent upon the length of the pendulum " (p. 87). The deflection depends, of course, only on the ratio of the acceleration of the train to that of gravity, and not on the length of the pendulum. The ballistic deflection of the compass is wrongly made out (pp. 88–89) to depend on the time occupied by the change of the ship's speed. The point is altogether confused. The simple fact is that the rate of westerly deflection of the compass is proportional to the ship's northerly acceleration ; and so the deflection produced is proportional to the change of northerly speed.

The explanation (pp. 122–128) of the action of the three gyroscopes mounted on the float of the Anschütz compass is entirely wrong. The East and West gyroscopes are credited with but one degree of freedom relatively to the float, and yet are made out as stiffening the float against tilting about its north-and-south diameter. The fallacy is in defiance of the simple fact that for a stiff configuration of their axes any number of gyroscopes are merely equivalent to a single one possessing their resultant angular momentum. Each of the two gyroscopes has two degrees of freedom relatively to the float, and the stiffening depends fundamentally on the extra degree of freedom.

The compass is said (p. 92) to be placed usually " above the vessel's metacentres—transverse and longitudinal—about which rolling and pitching take place." The metacentre for pitching being usually located somewhere high up the mast, it ought to be clear that the last statement is certainly wrong ; and if so then equally the notion (though a common one) that the metacentre for rolling remains stationary is (qualitatively) baseless.

I suppress reference to many other like instances of error. But, as regards general method, the author claims to have appealed only to " the most familiar physical principles " ; yet the gyroscope on various occasions " does its best "

(p. 7) or "manifests a desire" (p. 25) or even suffers from "its unsatisfied desire" (p. 66). The psychology of motive and emotion makes sad confusion among mechanical facts.

Tastes differ even among scientists, and it is possible that other critics may be prepared to disregard as venial slips, in a plausible aggregate, what I have instanced as fundamental misconceptions. And it must be admitted that your reviewer is not alone in his opinion. Another reviewer in a contemporary journal (*Nature*, May 12, 1921, p. 328) gives equally unqualified commendation to the same work. By a happy accident of the press your own reviewer is made to say that "the methods of eliminating fiction are described." The statement is premature.

G. T. BENNETT.

EMMANUEL COLLEGE, CAMBRIDGE,
May 23, 1921.

To the Editor of "Science Progress"

RUSSIAN SCIENTISTS

DEAR SIR,—Russian scientists are at present in extremely necessitous circumstances. Reference has frequently been made to the matter in the newspaper press and elsewhere. Maxim Gorki in particular has made urgent appeals to our men of science and artists to come quickly to the aid of these unfortunate people, and since the frontier between Finland and Russia was reopened, for diplomatic intercourse at least, certain Finns have been able to visit Russia and to verify the accounts received from there. At Petrograd, for instance, there are more than five thousand scientists, with their families, who are fighting for their lives against hunger and other privations. More than one scientist of European reputation has already perished.

We, the undersigned, who have been appointed by our colleagues of the University of Helsingfors as a Committee to assist the Russian scientists in their extremity, deemed it to be our duty to look for help in the first instance in our own country.

We recently supervised the dispatch of several wagon-loads of foodstuffs to Petrograd for distribution by trustworthy persons. But this help, to be effectual, must be continued for months, especially as it is foreseen with certitude that the appalling food-crisis which exists in the Russian cities will become more acute in the immediate future. It would also be desirable that help should also be extended to other cities, particularly Moscow.

It is to be feared, however, that the resources of our country will not suffice the purpose we have in view. We, the undersigned, therefore, with the approval of our colleagues, have decided to approach a certain number of persons of eminence in the scientific world in order to obtain, through their benevolent mediation, publicity for the facts of the case. It may perhaps only be necessary to bring the precarious situation of the Russian scientists to the notice of the world in order to secure for them the assistance of which they stand in need if they are to be saved in the terrible crisis with which they are confronted. With this object in view we venture to count upon your sympathetic co-operation.

We add: Should our project, thanks to your enlightened efforts, be efficaciously supported by others, and should our Committee, by reason of the vicinityof our country to Russia and of the knowledge we already possess of the means of transport in Russia, be able to serve the cause by accepting the rôle of intermediaries, we shall be happy to place our services at your disposal. If this proposal is accepted we request you to be kind enough to

address to the undersigned Professor *Mikkola, Helsingfors,* both parcels of food and sums of money for the purchase of food for dispatch to Russia.

Our delegates (the undersigned Igelström and Mansikka) who have just returned from Petrograd, inform us that the Russian scientists whom they met in conference greeted them with enthusiasm as the first bearers of a message from their colleagues in the civilised western countries. The Russian scientists accept with the deepest gratitude help from abroad in the form of food, clothing, and books, and are of opinion that our Committee is the most suitable organisation for the rôle of intermediary.

In virtue of the fact that in all that appertains to this work of mercy we have succeeded in arriving at a complete understanding with the Soviet Government, which has shown our delegates the utmost consideration, we believe that we can guarantee that all parcels dispatched will reach their destination.

WALD. RUIN, *Professor of Pedagogy.*
ROBERT TIGERSTEDT, *Late Professor of Physiology.*
R. FALTIN, *Professor of Surgery.*
J. MIKKOLA, *Professor of Slavonic Philology.*
A. IGELSTRÖM, *Librarian.*
V. J. MANSIKKA, *Reader.*
O. V. SCHOULTZ, *Lecturer in Russian.*

HELSINGFORS,
June 8, 1921.

ESSAYS

TYPEWRITER REFORMS—THE COMBINATIONAL KEYBOARD (William Wilson Nelson).[1]

INTRODUCTORY

Amongst the many mechanical inventions of the nineteenth century there has been none more ingenious and useful than the Typewriter. Although in point of size it cannot compare with the larger mechanical constructions of last century, and it must, therefore, as an exponent of mechanical powers, be reckoned amongst the minor inventions, it may nevertheless, as an exponent of *human faculty*, be assigned a high and important rank. It is no part of our purpose to speak of the skill and ingenuity that have been expended on the many fine mechanical adjustments for the various kinds of operation to be found in the best class of Typewriters. Viewed merely as a machine, and for the work it has to do, the Typewriter at its best seems to have already become mechanically perfect.

What we are proposing to do is to deal with it from a linguistic and psychological point of view, in respect of which an improvement is yet imperatively necessary and may be expected in due time. Considering that the Typewriter was invented to exploit the powers of the mind, it is a curious and surprising fact that, though thousands of pounds sterling and all the ingenuity of genius have been devoted to the perfecting of its mechanism, so little thought should have been bestowed on what is the *very pulse and soul* of the machine, namely, an arrangement of the keyboard suited to the genius of our Language. The importance of the question as to what should be the *most perfect* arrangement of the letters of the English alphabet for yielding the *greatest ease* in the typing of a writer's thought *cannot be overestimated*. It is not too much to say that a keyboard perfect for its purpose is more than the equivalent of adding new mechanical powers to the machine. Why the present " Standard " keyboard, as it is called, should be so defective in its literary scheme it is impossible, in the absence of precise information, to say; but it may be assumed that the original inventors being, as mechanicians, more anxiously concerned about the conception, adjustment, and proper working of its mechanical parts than about the study of the habits of our Language, failed to see the possibilities that lay hidden in the keyboard problem, and arranged as best they could the keyboard which, through purely commercial considerations, has since become the standard keyboard of all Typewriters. It is certain that the real elements involved in a right solution of the problem were not taken into account—with the unfortunate result that those things have been done in its arrangement which ought not to have been done, and those things left undone which ought to have been done.

There is an almost infinite number of ways in which the letters of our Alphabet can be grouped,[2] but for typewriting purposes there can be only

[2] Leibniz, in his *De Arte Combinatoria*, states that an Alphabet of 24 letters can be arranged in the following number of ways: 620,448,401,733,239,439,360,000. (Prof. F. Max Mueller, *Lectures on the Science of Language*, Ed. 1888, vol. ii, p. 81.)

one grouping which shall surpass all others, and which cannot be attained except through a careful analysis of the structure of our Language as exhibited in its ordinary written usage. The Keyboard herewith submitted is the result of researches extending over the leisure hours of three years, and involving no less than seven million separate scrutinies of 165,000 words, embodied in the prose writings of representative living authors in various departments of literature and science. It is, as will shortly appear, appropriately named the "Combinational" keyboard.

Those who have not looked closely into the structural habits of our Language would be surprised at the very large number of words in which certain combinations of letters are always found together. It might almost be said that not a single sentence, however short, can be written without including one or several of them. On each page of every English book they are to be seen in hundreds. It is, therefore, self-evident that if such combinations be suitably grouped on the keyboard for easy word-making, it will be found that those words in which they are embodied—the greater number of them being the most commonly used of all words—will "come together" under the fingers almost *en bloc*, and approximate most closely to the habits of the mind and pen that have been fostered by centuries of uniform usage. Many hundreds of the commonest words are, from start to finish, wholly constructed of these combinations, and many thousands of other words are largely combinational in structure.

Combinations may be classified as either *fixed* or *variable*, the former class being chiefly associated with the consonants, and the latter with the vowels and semi-vowels. For the new keyboard a selection of thirty-eight of the most representative fixed combinations in the English Language has been made, and their places and connexions assigned to the most efficient positions on the keyboard in accordance with the order of their frequency in various styles of prose. With a view to arrive at reliable statistics as to frequency of occurrence, each combination has been separately *combed out* page by page from a mass of prose which would fill a crown octavo volume embracing 550 pages of brevier print. This research, though extremely taxing to the eyes, brain, and patience, has been of the utmost importance for the formulation of a literary and scientific Keyboard—scientific, that is to say, in the sense of being based on a precise induction of the facts involved in the structure of our Language.

PRACTICAL

The important points to be considered in the arrangement of a perfect keyboard are three in number:

1. The habits of the hands.
2. The habits of the Language.
3. The associational habits of the mind.

1. In typing the really capable or effective fingers are the fore and middle fingers of each hand. The third finger can be used for letters lying on the outskirts of the keyboard, but, unless specially trained, it is naturally a somewhat unreliable finger in quick typing (as also similarly in pianoforte playing).[1] The thumb and little finger are not used on the keyboard. In brief, the first and second fingers are those which are most instinctively used by typists because their touch is always quite firm and certain.

[1] Sir C. H. H. Parry, D.Mus., calls the third finger the *clumsy* finger. (J. Seb. Bach. 1909, p. 114.)

Some expert typists use only the forefinger of each hand, and for this type of fingering the new keyboard is an ideal one; but it must be regarded as a fussy and undesirable type of fingering.

Now it is to be observed, that when the hands are brought together to begin typing, these really effective fingers are all found to be directly *at the centre* of the keyboard, and the weaker fingers toward its outskirts. This is the most important fact in our problem as regards the habits of the hands; and it at once delimits the centre of the keyboard as the *most efficient area* for doing work. This being so, the question arises how best to utilise the central area of the keyboard, and a consideration of this question carries us on to the next two aspects of our problem, which may be taken together as being mutually dependent on each other.

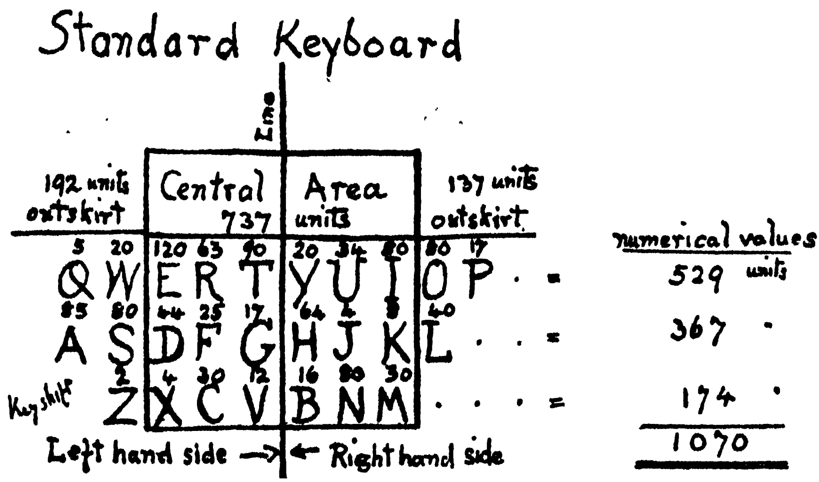
2 and 3. *The Habits of the Language and the Habits of the Mind.*—It must be regarded as the primary axiom in keyboard arranging that the most *efficient letters* of our Language, that is to say, those letters which enter most largely into the structure of English words, shall be brought to the *centre* of the keyboard to meet the *direct action* of the *most efficient fingers*. The relative values of the letters of the English Alphabet as *word-makers* are as follows—(Chambers's *Encyclopedia*, ed. *circa* 1888, article—Printing): E 120, T 90, A 85, S 80, I 80, O 80, N 80, H 64, R 63, D 44, L 40, U 34, C 30, M 30, F 25, W 20, Y 20, P 17, G 17, B 16, V 12, K 8, Q 5, J 4, X 4, and Z 2.[1]

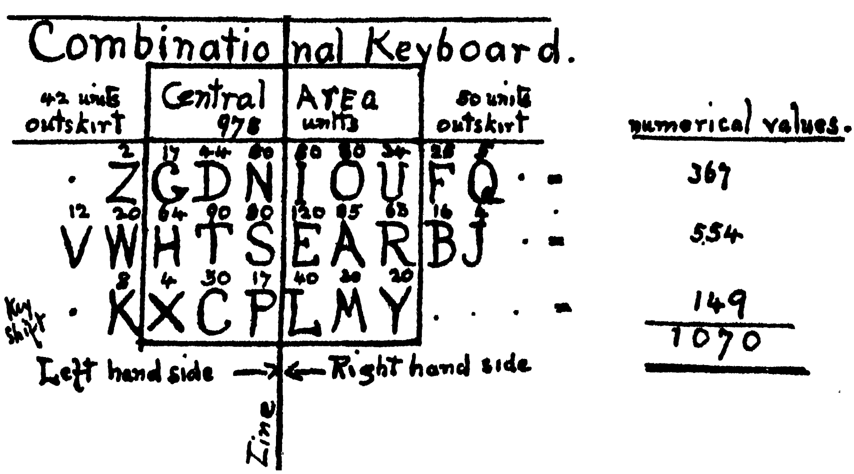
These figures, set out in the order of their importance, are deserving of a most careful scrutiny. Added together they represent a sum-total of 1070, which number, for the purpose of our exposition, may be regarded as the *full numerical power* of the twenty-six letters of our Alphabet. On proceeding, however, to subject this sum-total to analysis, the remarkable fact is revealed that little more than one-half of the twenty-six letters includes within itself nearly the whole value of the numerical sum-total. By a simple sum in addition it will be seen that only fourteen letters bear the burden of forming the structure not merely of one-half, nor two-thirds, nor even of three-fourths, but actually of more than four-fifths of all the words in our Language; or, to state it more concisely in another form, 86 out of every 100 words are built up from the following letters: E 120, T 90, A 85, S 80, I 80, O 80, N 80, H 64, R 63, D 44, L 40, U 34, C 30, M 30, or equal to a summed value of 920, the remaining twelve letters of the alphabet reserving a value of 150 only! Hence the paramount importance of these fourteen most efficient letters being placed on the keyboard in the most efficient positions to meet the *direct action* of the most efficient fingers. They must, without the shadow of a doubt, be found *within its central area*. From the logic of this fact there is no escape. But if the first axiom of keyboard-arranging be that the effective letters must be centralised to meet the effective fingers, the second axiom, and only second in importance, is that those combinations of letters which are most frequently found together in the structure of words shall also be found together on the keyboard, and the most valuable of them within its central area. This problem of combinations is really a complex one, and comparable with trying to solve a difficult chess problem, for although it is easy to place E, as the most efficient letter for word-making, at the centre of the board, and to group the other letters about it in the order of their relative values, from the centre outwards to the outskirts of the keyboard, it is not easy to do this and at the same time suitably group together all those combinations that are of the utmost importance to the fluent and pleasant writing of our Language. Many hundreds of keyboards have been drawn with different groupings of letters for testing and adjusting the various combinations into the best positions in order to arrive at a satis-

[1] The Encyclopedia gives the figures as E 12,000, T 9,000, A 8,500, and so on to Z 200; but, for the more convenient handling of these values, which play a large part in our exposition, the hundred units have been struck off each letter without, of course, affecting their relative values, since the ratio of E 120 to Z 2 is the same as E 12,000 to Z 200.

factory final result. No letter has been located on the keyboard whose position has not been considered many times in all its connexions.

The following are those combinations of the greatest use, marshalled in the order of their utility. The figures within brackets indicate the number of times each occurred in 165,000 words of various prose writings.





GENERAL TABLE OF COMBINATIONS

First Series: FIXED.—chiefly *Consonantal Combinations*.

TH (23,874), ND (9,131), ST (6,038), NG (5,900), ED past participle ending (5,815), NT (5,183), CH (3,481), ES plural ending (3,417). WH (2,984), NC (2,513), LY adverbial ending (2,512), CT (2,430), SH (2,176), GH-T (1,859),

NS (1,619), MP (1,389), SP (1,066), XP (959), QU (907), SC (897), DIS (713), CK (588), NESS (462), MB (386), SM (248), SW (178), BJ (172). Total occurrences, 86,897.

The suffixes "tion" (2,900) and "sion" (445), not included in the above, were separately combed out; but, as their common element is the diphthong IO, their numbers are included in the 4,348 occurrences of the latter combination given in the Third (or diphthongal) Series. The terminals -ED, -ES, -NESS, and the initials QU-, DIS-, are, like "tion" and "sion," partly vowel and partly consonantal in structure; but as, like the purely consonantal combinations, they never vary from a fixed order in words, we have classified them with the consonantal Series as being of a fixed character.

Second Series: VARIABLE—consisting of the letters L and R in close union with the five vowels. L + vowels (23,202), and R + vowels (41,320). Total occurrences, 64,522. These combinations are so varied, interesting, characteristic and universal as to form one of the most beautiful and convincing proofs of the superiority of the new keyboard.

Third Series: VARIABLE—*Diphthongal Combinations.*

OU (5,414), EA (4,406), IO-OI (4,348), AI-IA (3,083), EI-IE (3,020), AU-UA (1,066), OA-AO (315) = 21,742 occurrences.

Other diphthongal combinations of less frequency, *e.g.* EO, EU, UI, etc., have not been combed out, but, occur as often as they may, are taken close together on our keyboard.

For ease of fingering and writing, it is desirable that the respective letters of which each combination is composed should be placed, as regards the most powerful combinations, on *the same line* of the keyboard next together, or, as regards the less powerful combinations, with only one intervening letter between their two components. It was impossible to get all the 38 combinations equated on the same lines, but, subject to the inevitable compromises which keyboard arranging begets, it will be found that, with few exceptions, the most frequently used combinations are equated, the greater number of them being on the middle and upper lines of the keyboard, *e.g.* Middle line: EA, ES, ST, TH, SH, WH, SW, and BJ (in all 43,335 occurrences). Upper line: ND, NG, OU, IO-OI, and QU (25,700). Bottom line: LY, MP, XP, CK (5,488)—a total number of 74,483 occurrences from these seventeen equated combinations.

An examination of the *GENERAL TABLE OF COMBINATIONS* shows the letter H to be one of the root letters of our Language in its combinational aptitudes, entering as it does into fixed union with five other letters—namely, TH, SH, WH, CH, GH-T; and special care has been taken to locate it on the keyboard in accordance with their requirements. It constantly follows directly after these letters, and its numerical force is chiefly used up in the writing of these combinations. Hence, its place on the keyboard is not so close to the central vowel E and other vowels as its numerical value might seem to demand; nor could it be placed closer without displacing letters of greater numerical value than itself. One of its combinations—TH—is by much the most powerful of all the *consonantal* combinations, its only rival in frequencies being the L and R + vowel combinations. The H group of combinations yields a total number of 34,374 occurrences.

The letter N is another of the root combinational letters of our Language, and, regarded from the point of view of *all* its relationships, much more powerful than H, though less potent in its purely consonantal combinations. Like H, it also unites with five other consonants—ND, NG, NT, NS, NC. The ND combination in particular is second only to TH in frequency of use, and all its other combinations are of common use, especially NG as forming the present participle ending. The total number of its *consonantal* combina-

tions is 24,346. Apart, however, from its consonantal usages, it must be taken into consideration that N is classified by grammarians as a liquid, or semi-vowel; and this has a real bearing on its utility as a word-maker in general. Though a letter of high numerical value, it is not the *initial* letter of many words. In this respect it agrees with the five vowels, which are barren in generative force but rich in nutritive qualities. For instance, the joint numerical values of P 17 + B 16 = 33 are less than one-half of the value of N = 80, yet in the number of words of which P and B form the initial letters they surpass N by nearly sevenfold. Ogilvie[1] gives 153 columns of words beginning with P and B, but only 23 columns under N. It therefore necessarily follows that N is most largely used up *in union with the five vowels* in the *medial* and *terminal* positions of word-building, for which reason it will be found on our keyboard so closely associated with the vowels as to form a series of *variable* combinations (uncombed out) that enter into thousands of words. Its semi-vowel character is clearly evident in them, being similar herein to L and R, which are also by philological classification liquids or semi-vowels. Too much space would be needed to illustrate the richness of all the variations of N with all the vowels. A dictionary or page of any book may be scrutinised for its varied medial and terminal combinations with the vowels, but words like, *e.g.*, union, none, inane, onion, noun, nine, neon, æon, etc., are wholly composed of N + vowels; other very numerous words, *e.g.* anneal, alone, annul, sane, anoint, announce, opinion, minion, annual, main, pain, rain, etc., being, with the exception of one other letter, likewise wholly so composed. But this is not all. The letters L R M S in several of the words just given are themselves of the same nature as N in being liquids or semi-vowels,—the full list of the semi-vowels being L M N R S and Y.[2] Until the habits of our Language are closely looked into, it is impossible to realise the predominant part that these six semi-vowels play in union with the five pure vowels in building up the structure of words. They are ubiquitous. These eleven letters, constituting the vowels and semi-vowels, are all grouped together on the new keyboard and form one of its most indispensable endowments. Taken as a group, they represent a word-making power of 712 units out of 1,070 units for the whole Language, which fact alone is in itself enough to confirm their wide-spread usage; and, we may add, they contribute the chief elements of euphony to our tongue. The considerations just stated make it quite clear that the position of N must be next the leading vowels, while at the same time it must not be dissevered from its important fixed consonantal combinations.

There is yet a third letter that demands special notice in regard to its combinational relationships. The letter S combines with six other consonants, ST, SH, SP, SC, SM, SW (and in a number of words with L, N, K, Q), but, like N, does not draw the whole of its significance from its purely consonantal

[1] Our dictionary statistics are drawn from Ogilvie's *Smaller English Dictionary*, 468 small quarto pages, three columns of closely printed matter to each page—a handy, yet full volume. The Editor's Preface says: "All English words in general use are given; at least none are omitted which are likely to occur in the common run of literature."

Ogilvie displays for the whole Language 1,404 columns of words, of which only 278 columns represent words beginning with the five vowels; and even these are not all root-words, since about 75 columns out of the 278 are used up in repeating words from other letters of the dictionary merely prefixed by the negatives in- and un-.

[2] "L M N R are liquids which, with the sibilant S, are also termed semi-vowels." (*Edinburgh Academy Greek Grammar*. Ed. 1878, p. 10.)

"The liquids L M N NG and R, and the sibilants S SH Z ZH, are all continuous sounds approaching in this respect to the vowels." (Prof. Alex. Bain, *English Composition and Rhetoric*. Ed. 1880, vol. ii, p. 280.)

combinations, though these are of the greatest value in many of the most commonly used words, ST, for instance, being the most powerful combination there is after TH and ND. Unlike N, however, the letter S is in initial generative force by far the most powerful of all the letters of the Alphabet. If we regard the monosyllables only, which are the foundation-stones of our Language, numbering altogether according to Ogilvie,[1] 3,660, then S begins 664 of them, the next greatest numbers for other letters falling to the credit of C with 317, T 293, and B 278 each, whilst of the three root letters now under discussion H has 171 and N but 76; and what is here said of the monosyllables applies equally to the polysyllables. But, apart from its strong initiative force and its consonantal combinations, S forms with E the frequent plural ending -ES; yet this extended form of the plural, which alone has been combed out, does not exhaust the plural functions of S, since it is also used up in forming the plural of all those nouns that add only S itself to their stems (*e.g.* hands). Another of its functions is to form the oft-recurring 3rd person singular of the present indicative mood. And, as we have already seen, S is also a semi-vowel. Taking into account all the connexions of this important letter, it has been given precedence over T on the keyboard and placed next to the central E, while care has been taken to see that its consonantal combinations come together fluently under the fingers. Moreover, the placing of the T one point backwards from the centre makes the fingering of the TH combination quite perfect, as on account of its importance it ought to be. The total number of occurrences of the S group of combinations is 8,427 exclusive of SH, which has already been included under the H group, and also exclusive of its extensive semi-vowel combinations.

Summing up results so far attained, these three root letters, H, N, and S, with their sixteen "fixed" combinations, mostly equated on the same lines of the keyboard, represent no less than 70,564 occurrences.

GENERAL SUMMARY

H, N, S Combinations (GENERAL TABLE—1st Series)	70,564	occurrences.
L and R + vowel Combinations (GENERAL TABLE —2nd Series)	64,522	,,
Seven diphthongal Combinations (GENERAL TABLE —3rd Series)	21,742	,,
Miscellaneous Combinations unclassified—1st Series .	16,333	,,
	173,161	

The significance of this result for the fluent writing of our Language does not need to be emphasised: it speaks for itself.

THE FIVE VOWELS

We have now to consider one of the most distinctive features of the "Combinational" Keyboard, namely, the grouping of all the vowels together, and, as regards the *diphthongs*, in their natural order as if written by the pen from left to right, *i.e.* OU, EA, IO, IA, AU, EU, EO. The mind expects the diphthongs to occur in this serial order in the majority of words and the fingers find pleasure in so writing them.

Collectively the five most important letters in our Language are the

[1] The number of monosyllables is as follows:—under A 49, B 278, C 317, D 177, E 38, F 227, G 195, H 171, I 19, J 46, K 46, L 164, M 158, N 76, O 39, P 251, Q 33, R 134, S 664, T 293, U 5, V 44, W 205, Y 26, and Z 5 = a total of 3,660, out of which number no less than 3,210 contain either consonantal or diphthongal or semi-vowel combinations.

five vowels. No word can be written without a vowel. If the consonants form the body of a word the vowel is its soul. Numerically the sum of their letters exceeds the sum of T S N H and R, which are the next five letters having the highest powers. Therefore, the vowels require especially careful grouping. There is no greater fallacy than to suppose that any advantage is gained by scattering them all over the keyboard. Such an arrangement is the worst possible. On our keyboard they are all taken by the two efficient fingers of the right hand. This is as it ought to be. In ordinary writing it is the universal habit to use the right hand, and in all the usual avocations of life the right hand is *par excellence* the capable hand. The left is the steadying or supporting hand, the right is the active, cunning, and efficient hand for doing essential work; and we believe that in the division of work between the two hands in typing this superior efficiency of the right hand can and must be taken into account.

In order to facilitate the process of fingering, keyboards are divided, *memoriter*, into two equal areas, one of which is assigned to the right, the other to the left hand. On our Diagram the dividing line is shown on both the "Standard" and "Combinational" keyboards; but, on a comparison being made, it will be seen that, whereas on the "Standard" the right hand area has only a numerical word-making power of 457, the "Combinational" has a power of 602. Conversely, on the "Standard" the left hand is burdened to the extent of 613, but only to the extent of 468 on the "Combinational"; so that on the "Standard" the less efficient hand is given the stress of work to do, while on the "Combinational" the more efficient hand is so taxed. The full significance of these figures will only be understood when we come to deal with them in connexion with the middle line of the keyboard—the point on which it is now desired to focus attention being that, on the "Combinational" keyboard, the really vital part of the work assigned to the right hand is a *complete control of all the vowels*, and of all the semi-vowels except two. It soon learns that the duty of providing the vowels falls solely to its care, and their arrangement is such that it can type them rapidly in any order of combination in which they may be required. On the left hand is thrown the duty of providing the leading consonants and consonantal combinations; and in this way the two hands *conspire together* to turn out words in the most workmanlike fashion. In typing words on the "Standard" containing several vowels, the mind, eyes, and fingers are sent fleeting all over the keyboard to pick them up at relatively wide intervals, mostly in inefficient positions, and the words refuse to "come together" naturally and easily. If an English dictionary be taken and examined from A to Z, it will be found (see footnotes, *ante*) that the great majority of words begin with a consonant followed by a vowel or diphthong, and the mind is attuned by the law of association to this requirement of the Language. Now on the "Combinational" keyboard the left hand first picks up the consonant or consonantal combination, the right hand following on with the vowels, and the *general flow* of producing words, wholly sometimes, largely at all times, is serially from left to right, as it would be if written by the pen according to customary habit. This feature, in conjunction with all the other good points of the new keyboard, gives a great psychological value to its writing powers. In other words, there is much less *reverse* action or travel of the mind and eyes, when typing, than on the "Standard" keyboard.

It must not be overlooked, *and too much stress cannot be laid on this fact*, that the eyes are continually following the fingers, and that the mind is always precedent in its action to the movement of both eyes and fingers: for it is certain that not a movement of the eyes or fingers can take place without the prior consent of the mind, even though it may seem to act with unconscious instantaneity. Nor can the mind think of two things at the same moment.

One letter must follow the other by a separate consent, however swiftly and unconsciously such steps may seem to be taken. Even for a blind typist, the interior travel of the mind from one part of the keyboard to another is just as active as for a seeing typist; and it is precisely this incessant mental action that demands as much relief as it is possible to secure. The conclusion to be drawn from these psychological considerations is, that when the efficient letters of our Language are largely or in part scattered on the outskirts of a keyboard, and, besides being so scattered, are lacking in those combinational relations so largely involved in word-structure, the fingers and eyes not only require to be *ex*tensively in incessant and active operation all over the keyboard, but the mind is to the same degree *in*tensively active and, to a considerable extent, distracted by the merely mechanical process of typing from its intellectual action of thinking about what is to be written.

Now, it is well known to medical men and oculists that this kind of incessant action is alike physically trying to the eyes and nervously fatiguing to the brain, and, ultimately, to the mind, which is dependent on the freshness of the physical brain for the exercise of its full powers.

The bearing of these observations is not far to seek. It must be concluded therefrom that a badly arranged keyboard is one that makes the maximum demand on fatigue of brain and effort of mind, whereas a well-arranged keyboard is one which, by *centralising* the fourteen all-important word-making letters thereby limits, and so minimises, the action of the eyes and mind, and at the same time arranges these letters, together with the remaining twelve letters of the alphabet, in combinations structural to the Language and expected by the laws of mental association.

The "Combinational" Keyboard has taken the whole body of these manual, linguistic, and psychological considerations into account.

CRITICAL

Having now laid bare the linguistic and psychological principles underlying the formulation of a truly scientific and literary keyboard, we may proceed to discuss wherein consist the defects of the keyboard at present in common use. There is a great variety of Typewriters on the market, but in respect of the *mechanical* disposition of their keyboards there is no dissimilarity. In the majority of machines the keyboard is disposed in four rows or lines of keys, ten or eleven keys on each line, or altogether about 43 keys. The lines rise stepwise one above the other, the uppermost line being reserved for figures and other signs useful chiefly for commercial purposes. The three lower lines are appropriated to the Alphabet and some punctuation signs; but it is the upper two lines of these three that receive by far the largest complement of letters. Of these three lines, which alone concern our discussion of the keyboard problem, the fingers have a preference for writing on the middle line, the reason of which is clear, its position being such that they can stretch from it with least movement to the next line above or drop to the next line below. It may, therefore, be regarded as the *master line* of the keyboard, and must be treated with special care on account of its supreme importance in the arrangement of a perfect keyboard. On it the most efficient letters must be centralised, and it must, as a whole, receive the fullest distribution of alphabetic power. Consequently, it gets on the "Combinational" a total numerical value of 554, representing a word-making power of more than one-half of the whole Language, as compared with a numerical value of 367 on the "Standard." On the other hand, the upper line of the "Standard"—a line second only in order of importance—has a value of 529 and its lower line of 174; so that between the interaction of these two secondary, and, in relation to each other, most distant lines, word-structure to the high value of 703 must be built up out of a total value of 1,070 for

the whole Language. On the "Combinational" the corresponding figure is 516 out of 1,070, resulting in a very considerable saving in finger movements between these extreme lines, and leaving the dominant task of word-making to the middle and most convenient line of the keyboard.

A notable weakness of the "Standard" lies in one of the most powerful letters, N, being relegated to its bottom line, and four out of the five vowels to its top line, with the result that the combinations TION, NT, SION, the prefixes in-, un-, enter-, intro-, intra-, under-, the common prepositions "in" and "on," and the thousands of other words in the structure of which N as a semi-vowel is found closely associated with the pure vowels, have to be taken between the extreme lines of the keyboard in a disjointed fashion. These defects ought to have been guarded against. The more nearly all the representative letters and combinations can be brought into close and sympathetic touch with each other the more pleasant is their manipulation.

The MIDDLE LINE of the "Standard" keyboard is of itself enough to prove the entire absence of scientific method in its arrangement, since, starting from the letter D, the consonantal sequence onwards to L is purely alphabetic, and on ending with L drops down to the bottom line and continues with M and N as if it were quite a matter of indifference where all these letters should be placed. Hence we have the sequence D F G H J K L, none of them, except H, letters in the first rank of importance, monopolising the most efficient positions on the most efficient line for word-making, and the really powerful letters of our Language relegated to the weaker lines and positions of the keyboard. Furthermore, having only one vowel, A, on its middle line, placed in an extreme *reverse* position, very few words can be written on it, and the typist is constantly referred upwards or downwards for the most frequent letters, thus causing a much more restless movement of eyes and fingers than should be necessary. By comparison, without an upward or downward movement, the "Combinational" can write on its middle line alone some 500 words, the majority of them within its central area, and amongst the most commonly used in the Language; and so powerful is the vowel and consonantal distribution of letters on this line that, by reference upwards or downwards for one other vowel or consonant, the number of words begins to increase in an almost geometrical progression.

One of the real differences between the two keyboards, as will already have been divined, consists in the process of writing on the "Standard" being from the outskirts—the weakest area of the keyboard—towards the centre; but on the "Combinational" from the centre, where the eyes and most efficient fingers naturally adjust themselves, to the outskirts for letters of the lowest value and infrequent use. It need not be said that it is the latter process which agrees with the natural position of the efficient fingers and represents a great gain in ease and speed, though more stress is to be laid on the gain in mental and visual ease than on speed.

But the most serious defect of the "Standard" is not only that the most powerful consonants are shunted off the most efficient line, but also that the leading vowels are banished to the outskirts of the keyboard; and by far the greatest of them, and the most powerful of all the letters, E, is placed on the upper line in a *reverse* position. The next most important vowel, and the third letter in numerical value for word-making, A, is wholly shunted, as already noted, to the extreme *reverse* end of the middle line. What follows is to be carefully noted. In typing, these two most valuable vowels, *together numerically greater in sum-total than all of the remaining three vowels,* are taken by the less efficient hand—point one; they are taken as regards A and almost as regards E, in the least efficient positions—point two; the mind, eyes, fingers, have to travel backwards to the left instead of forwards to the right, as is natural to them in ordinary writing and reading—point three; the important diphthong EA is not only taken in a reverse position but the

usual diphthongal *order* of these letters is also reversed—point four. All these points, in continuous writing for any length of time, tell a strong tale against the psychological habits of the mind, and are bound to induce more brain fatigue, besides being less accessible to the fingers. As regards the remaining vowels, I O U, the U is transposed *reversely* from its natural order in the most frequent of all the diphthongs, OU, and is assigned a position of advantage on the keyboard in excess of its value as a word-maker, whilst its companions I and O, two of the most powerful word-makers, are thrust towards the outskirts of the keyboard. These are most serious defects with respect to the collectively five most important letters in our Language, and operate strongly against mental ease of writing. And when, in addition, the strong combinational sinews of our Language are seen to be ignored, it is scarcely necessary to carry our critical analysis further. The whole of our argument may be summed up in a small compass. Within the central area of the new Keyboard 90 out of every 100 English words are written by the only two naturally firm-touching fingers of each hand. It is the sole Keyboard on which the most efficient letters, the most efficient structural combinations of our Language, and the most efficient area of the Keyboard, are all brought together under the direct manipulation of the most effective fingers for quickest writing.

Yet, notwithstanding this truly scientific linguistic-structural arrangement, it should be noted that every letter of the alphabet is assigned its exact position on each line of the keyboard towards the central E, in accordance with its strict numerical value as a word-maker; with the exceptions that, for combinational purposes, the positions of S and T, C and P, X and K, are interchanged, resulting in S and P being advanced one key nearer and K one key further from the centre than is warranted by their numerical values; but in ease of fingering there is little to choose between the first and second positions from the centre.

In connexion with the highly important question of "combinations," it is necessary (in order to anticipate a possible objection) to add a few more words on what we have already termed the reverse action or travel of mind, eyes, and fingers. Bearing in view the necessity of all letters being located towards the centre of the keyboard in the order of their numerical values, it was not possible to get all the combinations arranged in their natural serial order from left to right, as in the cases of EA, OU, IOU, EOU, IO, AU, EU, EO, WH, MB, LY, XP, BJ. Some of the most important combinations are taken in the reverse order of fingering, but this drawback is ameliorated, if not indeed altogether quite abolished in effect, by the central position of these combinations and the togetherness of their component letters. Notwithstanding that TH, ST, ND, and others are taken in reverse, they are scarcely felt so by the mind, since the fingers come together on them almost simultaneously. Had, for instance, the T of the TH combination been so located that T should have been at the centre next to E on the middle line of our keyboard, and its other component H at the extreme reverse end of the line (where V now is), then there would have arisen a distinct feeling in the mind of having to travel backwards or reversely to write this most frequent of all combinations, instead of which the two letters TH are next each other on the same line within the central area, and the mental, visual, and digital action at a minimum. Other combinations—such as GH, NS, CT, SP, IE, OA are neutral as regards either backward or forward movement, their letters being located one under the other. In the writing of all the combinations there are, for reasons just given, no awkward movements of the fingers, and especially is this so in the central area, where so much word-making is done. The two efficient fingers of each hand are there strictly kept to the left and right hand sides of the *memoriter* dividing line, and it is as impossible for the hands or fingers to conflict with each other as for those of a pianoforte

student when playing five-finger exercises. It is the rule of pianoforte virtuosi never to disturb the still-hand position in fingering so long as it can be maintained, owing to its superior ease, gracefulness, and fluency; and this is fully justified in the central area of the "Combinational" keyboard. Only letters outside this area need be taken by the weaker third finger, but there is no great loss of speed, and greater certainty of touch, if now and then B, F, and W are taken by a stretch of the second finger.

In concluding this critical section of our exposition we do not mean to assert that every word in our Language can be written with greater ease on the "Combinational" than on the "Standard," for if the letters of the Alphabet were flung haphazard on to a keyboard it would still write some words better than any other keyboard that could be arranged. A properly disposed keyboard must be judged by the great bulk of its performance, and not by isolated words. The new keyboard is not so much directed towards an increase of speed, though it necessarily does yield greater speed, as to an increase in mental, visual, and digital ease of writing: of which three aspects, however, by far the greatest stress must be laid on mental ease.

A few examples, out of many thousands of words, may be given of the usage of combinations on the keyboard.

(1) *L and R monosyllables with any two vowels.*

EAL: s.eal, st.eal, st.eal.th, real, d.eal, h.eal.th, m.eal, heal, peal, etc.
LEA: lea.se, lea.st, lea.d, lea.p, p.lea.se, c.lear, p.lea.d, c.lea.n, etc.
ALE: ale, s.ale, st.ale, t.ale, sh.ale, sc.ale, h.ale, m.ale, b.ale, dale, etc.
EAR: ear, s.ear, t.ear, sh.ear, Earl, n.ear, d.ear, ear.th, year, ear.n, etc.
REA: rear, t.rea.t, th.rea.t, rea.m, d.rea.m, c.rea.m, b.rea.th, b.rea.ch, etc.
ARE: are, st.are, rare, t.are, sh.are, d.are, c.are, sp.are, b.are, f.are, etc.
OUR: our, s.our, t.our, h.our, p.our, sc.our, f.our, c.our.t, m.our.n, etc.
ROU: rou.se, rou.nd, rou.t, sh.rou.d, rou.gh, th.rou.gh, g.rou.nd, p.rou.d, b.rou.ght, sp.rou.t, etc.
OAR: oar, s.oar, roar, roa.m, h.oar, h.oar.se, h.oar.d, b.oar.d, b.roa.d, etc.
ORE: ore, s.ore, st.ore, t.ore, sh.ore, p.ore, sp.ore, c.ore, sc.ore, etc.

Similar monosyllables can be formed from: rai-, -air, -eir, -ier, roa-, -ure, -ire, -ile, -ole, -ule, -oul, -ail, lai-, oil, -oir, -oal, loa-, etc.

(2) *Some L and R polysyllables*: s.eriou.s, s.erial, s.alary, arrear, array, area, early, alar.m, ally, rural, error, erra.nd, arra.nt, st.erile, c.ereal, lei.s.ure, p.illar, p.arole, c.arrier, b.arrier, c.urious, f.urious, sp.urious, n.eural, s.orro.w, n.arro.w, b.urro.w, b.iliary, c.orollary, t.error, am.eliora.te, s.urrou.nd w.arrior, m.ural, m.oral, m.olar, s.olar, p.olar,—and so on all through the Language. N.B.—Many of these L and R words have also "fixed" combinations in them.

(3) "*Fixed*" *combinations*, of which there are two or three in each monosyllable:—Ea.st, st.a.nd, st.ea.m, st.a.mp, st.i.ng, st.ra.ng.e, st.i.nt, st.ra.nd, st.au.nch, st.a.ck, c.oa.st, etc., sh.ea.th, sh.ea.f, sh.ou.t, sh.o.ck, sh.u.nt, etc., th.ou.gh, th.ou.ght, th.a.tch, th.i.ng, th.i.ck, wh.i.ch, w.ea.th.er, wh.e.th.er, s.ou.nd, f.ou.nd, m.ou.nd, p.ou.nd, b.ou.nd, ch.e.st, ch.ea.p, ch.ea.t, ch.ie.f, ch.oi.ce, ea.ch, t.ea.ch, t.ou.ch, p.ou.ch, sp.e.nd, sp.ee.ch, sp.ea.k, sc.a.nt, sc.ou.t, sc.e.nt, Sc.o.tch, m.ea.nt, au.nt, t.au.nt, s.ou.ght,—and so on all through the Language.

(4) *Monosyllables with only one combination are very numerous*: Th.e, th.ese, th.at, th.ere, th.is, th.ose, th.ine, th.en, th.em, th.us, ha.th, wi.th, pa.th, ba.th, bo.th, my.th, wid.th, dep.th, etc., a.nd, e.nd, sa.nd, se.nd, te.nd, ha.nd, me.nd, mi.nd, fo.nd, fi.nd, fu.nd, etc., st.one, st.ep, st.em, st.ay, st.ab, te.st, ne.st, pe.st, ca.st, ma.st, fa.st, etc., si.ng, so.ng, pa.ng, fa.ng, etc., sh.ame, sh.ot, sh.ut, sh.ine, sh.one, sh.od, ash, ca.sh, wa.sh, da.sh, me.sh—and so on, with all the other combinations, throughout the whole Language.

ESSAY-REVIEWS

THE MECHANISM OF THOUGHT, by Thomas Beaton, M.D., on: **In Search of the Soul and the Mechanism of Thought, Emotion, and Conduct.** In Two Volumes. By Bernard Hollander, M.D. [Vol. I, pp. x + 516; Vol. II, pp. vii + 361.] (London: Kegan Paul, Trench, Trübner & Co.; New York: E. P. Dutton & Co. Price £2 2s. net.)

From the time, towards the end of the eighteenth century, when psychology was lifted out of the welter of metaphysical speculation and recognised as being a definite branch of biological study, the problem of localising the various aspects of mental function in specified regions of the brain has exercised a great fascination for all workers in the subject, anatomists, physiologists, and psychologists. With a little reflection upon the matter it becomes quickly apparent that the successful solution of the problem demands, on the one hand, a complete knowledge of the anatomical structures of the brain and their physiological activities, and, on the other, a satisfactory conception of the mind, as to how it may be regarded as built up of component functions and what relationships may exist between these component functions. Although vast strides have been taken with the result that much more clear conceptions of the structure and physiological activities of the brain and also of the correlated functions of mind can now be entertained, yet it must be explicitly stated that these conceptions are still too inconclusive to permit of any but the most generalised statements regarding the actual intimate relations of brain function and mental activity. It is, therefore, a matter for conjecture as to what amount of reliance is to be placed upon the very definite cerebral localisation which was dogmatically laid down by Francis Joseph Gall at the beginning of the nineteenth century.

At that period of scientific knowledge a system of "faculty" psychology was largely in vogue, and the exponents of this system held that the personality, the character, and the intellect were capable of being split up into various attributes, which were to be regarded as independent units so far as function went. At this time, also, the wondrously complex structure of the brain was first being recognised, and one cannot be surprised that the enthusiasts of the day jumped to the conclusion that the cumbrous pigeon-holed psychology was to be accurately fitted into the surface of the great cerebral hemispheres, so conveniently and consistently divided up naturally by the furrows and ridges forming the cerebral convolutions; and from this early attempt at cerebral localisation arose the great cult of phrenology and charlatanism with which the name of Gall is inseparably associated.

So far as Gall himself was concerned, there is no doubt but that he was one of the pioneers in the investigation of the anatomy and physiology of the brain, but it is difficult to ascribe to him that pre-eminence which Dr. Hollander implies to have existed; moreover, he chose to stand or fall by his scheme of cortical representation, and, according to the opinion of posterity, he has fallen. Even in his own times the tide of scientific opinion definitely set away from his views; the high hopes which he had raised by his statements were found to be not justified by the experience of other

observers, and, while some of his contemporaries only saw in his writings and views evidences of a profound ignorance of the facts concerning brain structure and function that were then established, others went so far as to accuse him of deliberate falsification.

The great public cult of phrenology as a means of delineating character by the examination of the living head was soon, of course, discredited by the fact that the bones of the skull are not of consistent thickness, and that, therefore, external configuration does not conform to the contour of the underlying brain. Dr. Hollander is, apparently, of the opinion that Gall had no personal interest in, and did not support this public craze over the practical outcome of his theory ; but this is very doubtful, as there is evidence that Gall examined heads and delineated character on several occasions himself. Here, of course, it may have been that, carried away by public adulation, he allowed his enthusiasm to overcome his powers of scientific criticism; and the judgment of the present day must be left at that.

However, apart from the discrediting of the charlatan side of the matter, the purely scientific adherents of the cerebral localisation theory began to experience difficulties of their own, for few psychologists could be induced to agree as to what exactly were the faculties of the mind, and as each new faculty, as it arose, had to be fitted into the scheme of localisation, while others had to be abandoned, it was soon perceived that the position was untenable, and so, by common consent, the cortical representation of the mind on a faculty basis was abandoned.

The consensus of modern opinion on the subject of the functions of the brain is to the effect that it is to be regarded as the central controlling ganglion of the nervous system, and, therefore, of the body generally, and that the intellectual operations, being the latest evolutional development of the mind, are correlated in some manner with the physiological activities of the grey cerebral cortex, this being that structure which only shows its greatest predominance in the case of the human brain. The white matter which constitutes the medulla, or stalk of the hemispheres, is composed of an innumerable number of nerve-fibres, which exist to bring the cortical nerve-cells into communication with each other and also with the termination of the main paths of sensory communication coming from the body generally, and the termination of the main paths of motor communication going out to the musculature. Apart from the topographical representation of these terminal connections, that of the motor paths being fairly definite but that of the sensory much less so, there is little or no evidence to show any differentiation of the cortical surface into separate functioning areas ; in fact, the evidence which does exist tends to indicate a differentiation by layers of cells rather than by surface areas. From the psychological aspect, also, cortical differentiation is unlikely, as, apart from regarding the general function of moral and conscious inhibition as belonging to the cortical activities, there is no possibility of separating off any discrete mental attributes such as would be required by a strict surface differentiation of the cortex.

Now, in his book *In Search of the Soul*, Dr. Hollander espouses the cause of Gall, and takes up a standpoint directly at variance with that of modern opinion. He has produced a book which must have involved a tremendous labour of literary research, which, indeed, bears every trace of the most careful and laborious construction, and for which every credit must be given ; but he thereby lays himself open to the same criticism as was given by the eighteenth-century contemporaries to his master, Gall, and the reader is left rather with a feeling of regret that so much energy and so much labour should have been expended on the attempt to reopen a blind side-track, when there is so much to be done in the direct line of the advance of knowledge.

The first volume of *In Search of the Soul* is an admirable piece of work, and would be, as the author describes it, a valuable book of reference, but

for the stressing, on the one hand, and the belittling, on the other, which Dr. Hollander has found necessary in order to make his point. The second volume is disappointing. One was prepared for a restatement of the old faculty psychology, and in this expectation was fulfilled; but one was certainly intrigued at the prospect as to how the author would incorporate such vivifying modern conceptions as that of the defence mechanisms of the organism as a whole, of the repressions and the activities in the out-of-consciousness sphere of the mind, etc., and of these there is no mention at all. Dr. Hollander's speculative chapters do not really appear to throw any new light on the problems of present-day psychology or metaphysics, and in regard to his chapters on the practical extension of the Gallian thesis to the realm of medicine and surgery, though they may be safely left to the judgment of the professional reader, yet one feels that it is necessary to pass just one comment for the benefit of the layman, who might otherwise gain, perhaps, a mistaken impression. The day when a delusional state can be treated by means of a surgical operation is not yet come, and an operation for any disorder of the brain is only undertaken when there is very definite evidence of the locality of the disease process or injury; even so, in the very limited number of cases in which this condition is fulfilled, the manufacture of an artificial aperture in the bony skull-case is a matter not unattended by unfortunate sequelæ for the patient, as many a man wounded in the head in the late war knows to his cost.

THE PRODUCTIVITY OF THE SEA FISHERIES, by Prof. J. JOHNSTONE, D.Sc.: on **The Resources of the Sea.** By Prof. W. C. MCINTOSH, M.D., LL.D., D.Sc., F.R.S. Second Edition. [Pp. xvi + 352, with 32 tables, figs. in the text, and 19 plates.] (Cambridge: at the University Press, 1921. Price 35s. net.)

A FEW words about the early history of the British fisheries may be useful to readers of Professor McIntosh's book. Towards the end of the medieval period both the English fisheries and the great Skanian one in the Baltic had become decadent. The latter nearly disappeared because of natural causes—the decrease in the inflow of salt, Atlantic water through the Skagerak which was the consequence of a periodic decrease in tide-generating force. The former diminished (though it did not disappear) because of the relaxation of ecclesiastical discipline that followed the dissolution of the monasteries, and possibly also because of economic changes that are now difficult to trace. About the same time also the great Dutch herring fishery of the North-East Coast of Britain began to attain great dimensions. It is said that about 2,000 vessels came annually from Holland to fish in British waters.

The success of the Hollanders was regarded with much jealousy by James I and his son. Therefore this British Solomon, with the assistance of his lawyers, invented the theory of a Traditional British Naval Sovereignty for the discouragement of the Dutch, and attempts were made to exact a tribute. These were unsuccessful, but, from the time of Cromwell onward, the great Dutch herring fishery began to dwindle away. The wars of the Spanish Succession, the necessity for the defence of a long land frontier, and the Dunkirk privateers diminished the fishing marine so much that by the time that the Peace of Utrecht was concluded the Dutch maritime and fishery dominance had almost passed away. So far as fishery is concerned, nothing took its place.

In spite of repeated efforts by British statesmen from the time of Elizabeth to that of George II, no success in establishing a national fishery industry on a big scale was attained. All these efforts took much the same form—the creation of Royal Fishery Societies or Companies under Court patronage. The methods advocated and tried were, as a rule, rather slavish copying of the

Dutch vessels and apparatus of fishing and curing, and for some reason or other these were alien to the genius of the British fishermen. In 1752 the last of the societies was launched and was successful for a time. Though it ultimately failed it led up to the great Scottish industry—which culminated in the summer of 1914. From it proceeded the "Board of British White Herring Fishery," which, later on, became the Fishery Board for Scotland. The policy involved in the Act of 1752 was responsible for the bounty system which was so unfairly condemned by Adam Smith, but which probably kept the embryo industry alive through the trying period of the French revolutionary and Napoleonic wars.

After the peace of 1815 the British fisheries in England, Scotland, and Ireland simply leaped upwards. Until 1845 everything pointed to Ireland becoming a great fishery country, in spite of the administration of the growing industry. But in 1847 came the potato failure, and after that the pestilence, and then the emigration. In 1830 (when the bounty system had ceased) there were about 30,000 vessels on the fishing register; in 1914 there were about 5,000.

In Scotland, under an administration that was efficient in the highest degree, and which was, even from the beginning, scientific, the fishing industry made continual progress. It was predominantly a herring fishing industry depending on an export trade. In England the herring fishing was never so important as in Scotland, and trawling by deep-sea smacks was the principal method developed. Until about 1890 there was no administration worth calling such, and it was only about the beginning of the present century that the Central Fisheries Authority thought it worth while to prosecute scientific research. The record of the industry has been, however, one of continued growth, but in the later decade of the nineteenth century a remarkable change in its methods occurred.

In 1870 the first vessel designed for steam fishing was built at Grimsby. She was called the *Tubal Cain*, and was an iron smack. She never really went to sea under steam, but, later on, several steam trawlers were built as a great experiment and did actually fish. The engineers consulted were unfavourable to the idea: they thought that it was not probable that a screw could grip the water sufficiently to propel the vessel and tow a trawl-net at the same time. Besides, the cost of coal was too great—it was then eleven shillings a ton! Nevertheless, the first steam trawlers were very successful: so much so that, by the beginning of the eighties, there was quite a fleet of them at work on the East Coast. Even at that time they were working in Scottish waters.

Steam trawling came into existence quite unblessed by the English administration. Indeed, in 1872 Mr. E. Holdsworth, who had been Secretary of the great Royal Commission of 1863, doubted whether steam power would ever be a success in fishing. The Scottish Fishery Board, however, thought differently. Steam trawling was very unpopular with all other branches of fishermen, and there was a great outcry against it. It was accused of destroying the spawn and fry of fishes, and it was also blamed for catching too many! At last the Government appointed a Royal Commission to examine the question, and Lord Dalhousie, Mr. Ed. Marjoribanks, Prof. Huxley, Mr. W. S. Caine, and Mr. T. F. Brady composed this body. Unlike the Royal and Departmental Commissions with which we are now familiar, this one really did something. It appointed Prof. McIntosh as a Sub-Commissioner; a programme of scientific research was prepared and investigations were made. Legislation followed, and the question of the effect of trawling was largely resolved.

Did intensive fishing by means of trawl-nets deplete a ground of its stock of fish? This was a matter that had to be investigated, and so a number of "stations" in the Firth of Forth, St. Andrew's Bay, the Moray Firth, and the

Firth of Clyde were selected for experiment. Some of these areas were closed to commercial fishing and others were left open, and periodic trawls were made by the Scottish Fishery Board steamer *Garland* in order to detect what differences (if any) would be the result of the closure. Prof. McIntosh was himself the scientific member of the Board (for the constitution of that body provided for such a member as well as a scientific staff), and he, if anyone, is familiar with the results of these famous experiments. Their results are tabulated in the book under notice, but I doubt if any reader will be able to digest them, and in any case there is little doubt that they were inconclusive. Far more searching investigation was necessary.

At any rate, the conclusion that Prof. McIntosh makes is that there was no evidence that trawling could deplete or impoverish a fishing ground. The question is partly a natural history one and partly one that is to be discussed in the light of very exhaustive statistical data. In so far as it is a question of natural history no one is better able to judge than our author, with his unrivalled record as a marine zoologist. His deliberate opinion, expressed in this book, is that no operations possible to man can have any appreciable effect in reducing the productivity of a fishing ground. One must, because of the personal authority of our author, hesitate to differ from him.

Nevertheless, many people do differ. The Scottish Fishery Board, a body with great traditions and capable of very sound judgments, does differ, and so would most owners of steam fishing vessels. Without doubt a fishing ground can be rendered *less profitable* from the commercial point of view by long-continued and increasing exploitations. Much depends on a precise statement as to what one means by "depletion," "impoverishment," or "loss of productivity," and so far no one has been able to give us a satisfactory analysis of these conceptions.

This book is published in 1920—after a very interesting though undesired experiment on the great scale. In August of 1914 it became necessary, for military reasons, to prohibit trawling over large areas of the North Sea, the Irish Sea, and the English Channel. Fishing, therefore, fell off to an enormously greater extent than could have been possible in any designed experiment. How did the stock of fish on those grounds, that had presumably been depleted by the last thirty years' trawling, recover during the four years of partially suspended fishing? The question is being investigated, and I have, myself, examined statistics relating to the Irish Sea. This is pretty clear: if one had not known about the war, and the military restrictions, and had had only those statistics to serve as the material for an opinion, it would have been impossible to deduce that anything had occurred other than the "natural fluctuations" that are always to be seen in the productivity of a fishing ground. This conclusion, if it be sound and is confirmed for the North Sea, must be of significance in respect of Prof. McIntosh's attitude.

REVIEWS

MATHEMATICS

Introduction to the Theory of Fourier's Series and Integrals. By Prof. H. S. CARSLAW, Sc.D., D.Sc., F.R.S.E. [Pp. xi + 323.] (London: Macmillan & Co., 1921. Price 30*s*. net.)

THE task of the teacher of applied mathematics is to present mathematics to his students as a powerful instrument for the solution of the problems of nature. This is the spirit in which Prof. Carslaw has written his book, and it is a happy circumstance that he should have chosen as his theme the work of Fourier which, growing out of the needs of mathematical physics, marked a new stage in the development of pure mathematics. The beginner in applied mathematics is sometimes apt to be impatient with the niceties of pure mathematics. Of course, you *can* construct series which do not converge and integrals which cannot be differentiated, but these curiosities *never* appear in the solution of a physical problem. Sooner or later he learns that this is not the case; that Nature is more subtle than even the purest mathematician. Prof. Carslaw has succeeded in giving us a logical treatment of his subject without that air of unreality which is so often the price of modern mathematical rigour. When he is careful of a logical point he carries the conviction that, if this point is neglected, sooner or later it will turn up in our physical investigations to our undoing. When he says, "However, this method does not give a rigorous proof of these very important expansions for the following reasons: (i) . . .; (ii) . . .; (iii) . . .," the reader is in a position to appreciate the more elaborate proof which follows. This method is not only sound exposition; it is calculated to foster the true genius of physical science, which consists in inspired guessing followed by a critical analysis of the guess.

The present book is a second edition, and new editions are not popular with mathematicians. The old book is an old friend, and we know our way among its pages; we hesitate to set it aside in favour of a new, even if a better book. But in this case the recent advances in the subject have conspired with fifteen years of further thought on the part of the author to make a considerable revision inevitable. One result has been the division of the book into two volumes. The present volume is devoted to a fuller treatment of the pure mathematics of the subject, while the application to the theory of the conduction of heat is reserved for a second volume to be published later. We find ourselves hoping that when this volume appears it may not be too closely confined to the theory of heat, but that Professor Carslaw will use his great powers of exposition to give us some account of the applications which the work of Fourier has found throughout the whole realm of mathematical physics.

For the present we have a clear and adequate discussion of infinite series and integrals, leading up to a theory of Fourier series and integrals of a generality sufficient to cover all the present needs of mathematical physics, while abundant references will assist the reader who wishes to follow the subject further in its modern developments.

The two appendices, the first giving some account of practical harmonic analysis, and the second an exhaustive bibliography, provide a useful and appropriate conclusion to a valuable work.

G. B. J.

ASTRONOMY

Periodic Orbits. By F. R. Moulton, in collaboration with D. Buchanan, T. Buck, F. L. Griffin, W. R. Longley, and W. D. MacMillan. [Pp. xv + 524.] (Washington: Carnegie Institution, 1920.)

This volume contains in collected form the researches on the subject of periodic orbits carried on from 1900 onwards by Prof. F. R. Moulton or by students who made their doctorates under his direction. The substance of many of the chapters had been published previously in various mathematical or astronomical journals, but the Carnegie Institution has rendered a valuable service in publishing the investigations in collected form and *in extenso*. A tribute should be paid to the quality of the paper and of the printing; the type used is very clear, and great care has been taken in the printing of complicated mathematical formulæ and equations. A handsome volume is the result.

The distinguishing feature of the treatment throughout is its mathematical rigour. The validity of the methods used for the solution of differential equations is established and the consequence of solutions in infinite series is proved. In cases where several types of periodic orbits are possible the existence of the several types is formally proved and the method of obtaining the orbits is developed. And here it must be mentioned that careful regard has been had to practical requirements, so that the developments are always in a form applicable to practical problems in celestial mechanics.

In Chapter I methods of solution of certain types of differential equations, required in the subsequent chapters, are explained and the convergence of the solutions is examined. Chapter II treats of questions dealing with elliptic motion which are classic in celestial mechanics, but, instead of following the ordinary treatment, the periodicity of the motion is established and direct solutions in terms of the time as variable parameter are constructed by the methods developed in the preceding chapters. This application of these methods at once indicates their analytical power. Chapter III deals with the spherical pendulum, the same methods being used: it contains, amongst other things, a new and rigorous treatment of Hill's differential equation with periodic co-efficients. In Chapter IV periodic orbits about an oblate spheroid are discussed. Such orbits are not in general closed geometrically, but, considering the orbit in a revolving meridian plane passing through the particle, several classes of closed periodic orbits can be obtained; if the period of rotation of the line of nodes is commensurable with the period of motion in the revolving plane, the orbit is also closed in space. This problem has an application in the case of an oblate planet, such as Jupiter. Chapters V, VI, and VII deal with the periodic orbits of satellites oscillating about the straight line equilibrium points. Lagrange showed that if two finite spherical bodies revolve about their common centre of mass in circles, there are three points in the line of the masses such that if small masses are placed at them and projected so as to be instantaneously at rest relatively to the revolving system, they will always remain fixed relatively to it. Moulton shows that, if one of the small bodies is given a slight displacement, it may, under certain conditions, revolve in the vicinity of the equilibrium point in an orbit closed relatively to the revolving system. The existence of such orbits and their direct construction by two different methods are detailed: the corresponding problems are then solved for the case when the finite masses describe elliptical instead of circular orbits. In Chapter VIII generalisations of these problems to the case of n bodies are given: it is shown that there are $\frac{1}{2}n!$ straight-line solutions, such that under proper initial projections the bodies will remain always collinear and that to each one of these solutions there are $(n + 1)$ points of libration near which oscillating satellite orbits are possible. Chapter IX deals with corresponding periodic orbits near the Lagrangian equilateral triangle points. In Chapter X are discussed the

isosceles triangle solutions of the three-body problem: if a third body is started from the centre of gravity of two equal masses with initial conditions so chosen that it moves in a straight line and remains equidistant from the other bodies, periodic orbits may exist whether the third body is infinitesimal or finite in mass and whether the finite bodies move in a circle or in ellipses. All these cases are fully discussed. Chapters XI and XII deal with related subjects: the periodic orbits of infinitesimal satellites and inferior planets and of superior planets respectively. The latter case has no direct bearing upon the problems presented by the solar system, but, in the former case, the results are of direct practical application, particularly in the lunar theory. Analogous problems have been treated by Hill and Brown. It is shown that there are three families of satellites and of inferior planets whose motion is direct and an equal number with retrograde motion. Darwin, for a particular value of the mass-ratio, found the three satellite orbits but only one planetary orbit. The methods used in these two chapters are very general, and are applicable to any ratio of masses of the finite bodies. In Chapter XIII, the periodic orbits of a particle subject to the attraction of n-spheres having prescribed motions are discussed. Chapter XIV deals with a problem which is similar to that presented by the satellite systems of Jupiter and Saturn, the periodic orbits of k finite bodies revolving about a relatively large central mass, together with many related questions. Chapter XV contains a discussion of limiting cases of periodic orbits, viz. closed orbits of ejection, which pass through one of the masses. This is followed by a final chapter, in which an attempt is made to trace out the evolution of periodic orbits as the parameters upon which they depend are varied. An enormous amount of computation was involved in the preparation of this chapter. The non-existence of isolated periodic orbits is shown, and much light is thrown upon the inter-relationship of various families of periodic orbits although the discussion is admittedly in certain respects incomplete.

The labours of Prof. Moulton and of his collaborators have brought to light many new families of periodic orbits, which have been discussed with greater generality and rigour than previously. This volume is a fitting monument to their work; it will prove invaluable to all future students of the subject.

H. S. J.

PHYSICS

The Theory of Relativity. By ROBERT D. CARMICHAEL, Professor of Mathematics in University of Illinois. Second Edition. [Pp. 112, with 8 figures.] (New York: John Wiley & Sons. London: Chapman & Hall, 1920. Price 8s. 6d. net.)

THIS volume forms one of an excellent series of Mathematical Monographs by American authors, edited by Mansfield Merriman and Robert S. Woodward. The first edition was published in 1913, but the successful formulation by Einstein of a general theory of relativity embracing gravitation has called for a second edition in which an additional chapter has been added dealing with this wider theory, the treatment of the older theory remaining precisely as in the first edition.

Being a monograph, the subject is necessarily treated somewhat briefly. The author has nevertheless endeavoured throughout to distinguish between what is based upon experience and what is postulated in the theory and to examine to what extent the deductions from the postulates are compatible with experiment. It is precisely in this respect that some accounts of the theory are defective. For a logical, concise treatment of the special theory requiring a relatively slight acquaintance with mathematics the first six chapters can be strongly recommended.

The account of the generalised theory given in the last chapter is based

largely upon Eddington's *Report on the Relativity Theory of Gravitation*, published by the Physical Society. The discussion is very much condensed, and the author would have been well advised had he endeavoured to cover less ground and to simplify the treatment. The theory of tensors is dealt with in some detail; would it not have been wiser, in such a limited exposition, to have indicated the bases of Einstein's exposition without attempting to give all the mathematics entering into it? The applications to the three-crucial phenomena could then have been explained in greater detail and the extension of the theory to electro-magnetism need not have been so summarily disposed of. As the volume stands the two parts seem disjointed: the one clear, elementary, and free from mathematics; the other condensed, difficult for the average student of physical science, and full of mathematics. We hope that in a future edition they will be better harmonised.

H. S. J.

A Brief Account of Radio-activity. By F. P. VENABLE, Ph.D., D.Sc. [Pp. vi + 54.] (London: George G. Harrap & Co. Price 3*s*. 6*d*. net.)

ON producing this little book of 52 pages the author has sought to replace the usual page or two on radio-activity which is included in text-books for students of general chemistry. It is written simply, and is devoid of mathematical notation, for which reasons it should appeal to that larger class of individuals who are generally interested in science.

On the whole, there is a general lack of orderly arrangement in the presentation of the facts. To give instances, one is merely informed in Chapter I of the "discharging action" of radio-active salts. Chapter II opens with a statement to the effect that the radiations possess the power of forming ions, which leads to a brief description of ionisation in gases, whilst in the latter part of the same chapter one is informed that "radiations from radio-active bodies can discharge both positively and negatively electrified bodies." Chapter III describes changes in radio-active bodies, although one must read Chapter IV to ascertain the nature of the alpha particle.

A chapter on the Structure of the Atom begins with a section on the properties of radium, and another follows on the energy evolved by that element. One is glad to find brief descriptions of the Thomson and of the Rutherford models of the atom included in the chapter.

Short as the account is, instances of repetition are by no means lacking.

Several well-known diagrams and tables appear in the text with advantage, whilst many of the recent advances in radio-activity are briefly narrated. The book should do much to stimulate interest in a most fascinating subject.

L. H. C.

The Thermionic Vacuum Tube. By H. J. VAN DER BIJL, M.A., Ph.D. [Pp. xix + 391.] (London and New York: McGraw-Hill Book Company, 1920.)

THIS volume will provide somewhat startling reading for the general physicist who has not had the opportunity of following the recent applications of thermionics. The subject is of phenomenal growth during the past eight years, but military necessity until recently forbade publication of most of the details. The last two years, however, have coincided with a pause in the development of the subject, and volumes wholly devoted to the thermionic tube and its applications are now appearing in France, England, America, and Germany. Dr. van der Bijl's work is a particularly attractive survey of the work done in America. Happily he has realised that more or less parallel investigations were carried out in all four countries, and has not seriously attempted to allot responsibility here and there for particular developments. The treatment throughout is essentially the practical one of a telephone engineer,

but this must not be taken to mean that the important theoretical portions of the subject are omitted.

The first five chapters are devoted to the physics of the thermionic tube. The remaining five deal with the various applications of the device, most prominent, of course, being the discussions of the triode as rectifier, amplifier, and alternating current generator. It is a pity that much of this treatment is marred by the author's persistent use of parabolic equations to represent the principal voltage-current characteristic of a tube. Such expressions have no theoretical basis, and should be replaced by those suggested by C. D. Child and I. Langmuir. Anyone attempting to determine the alternating current amplitude of a triode generator using the parabolic relation, very soon finds that it does not contain the one term that really matters.

The treatment of the internal action of a three-electrode tube is based on an application of Maxwell's theory of the shielding action of a grating. Experiment is necessary for cases in which the approximations of Maxwell's analysis are not valid. After reading these chapters one feels grateful for the appreciable inertia of the electron. A triode would function as neither amplifier nor oscillation generator if the electrons followed the lines of electric force.

Much space is devoted to methods of measuring the amplification factors of thermionic tubes. Such work, of course, is highly important in practice where tubes have to function in parallel or to be interchangeable. But Dr. van der Bijl must have found that his method of determining the amplification factor described on page 159 is not accurate, the values obtained being determined almost wholly by the sensitivity of the galvanometer used.

The volume is well printed, and contains many excellent diagrams. To the student of Radiotelegraphy it is sufficient to say that it is uniform with Seelig's translation of Zenneck's *Wireless Telegraphy*.

E. V. APPLETON.

METEOROLOGY

Où En Est La Météorologie? By ALPHONSE BERGET. [Pp. 300.] (Paris: Gauthier-Villars et Cie.)

THIS book is one of a series which gives résumés of the most recent advances in various branches of science, and endeavours to indicate also the most important outstanding problems that await solution. This particular volume does not therefore aim at being a text-book of meteorology.

It is evident that, to M. Berget, meteorology is interesting only in so far as it is applied physics, and for that reason one finds subjects such as the kinetic theory of gases and actinometry expounded at considerable length, while climatological statistics occupy but little space.

At the outset it is surprising to find a physicist opposed to the use of the absolute scale of temperature in meteorology, and also to the expression of pressure in megadynes per square centimetre instead of in the older units, but, on proceeding further, one is tempted to suspect that the author's evident prejudice against the work of German scientists leads him to dislike any idea that is "outre-Rhin" in origin. Certainly, he loses no opportunity of denouncing the "savants d'outre Rhin," and even Helmholtz does not quite escape his strictures. A spirit of this kind is deplorable in a scientific work. The book certainly deserves to be widely read in this country, as it is quite an education to find how different a view can be held on the Continent as to the exact position of meteorological research, and as to who in recent years has done most to advance it. M. Berget is a great admirer of the American Weather Bureau; he is an advocate of M. Guilbert's rules for predicting the behaviour of cyclonic depressions, and makes astonishing claims of success for this method; but for the millennium in forecasting he looks to the harmonic analysis of the curve for barometric pressure on the lines of the work of

Vercelli, of Milan. There are few references to really recent work, and the names of Bjerknes, Shaw, and W. H. Dines, for instance, do not once occur even in the chapter devoted to the exploration of the upper atmosphere.

E. V. N.

Clouds: A Descriptive Illustrated Guide-book to the Observation and Classification of Clouds. By GEO. AUBOURNE CLARKE, F.R.P.S., F.R.MET.SOC. [Pp. xvi + 136, with 70 plates and 17 figures.] (London: Constable & Co., 1920. Price 21*s*. net.)

MR. CLARKE has divided his book into two parts. In the first he outlines the international classification of clouds, and in a chapter entitled "Cloud Forms and Transformations" discusses the different forms in detail. Among many other interesting observations those relating to lenticular clouds are of especial interest. These clouds occur very often at Aberdeen with south-west winds, and rather less often with westerly winds—that is to say, in winds that have crossed the mountains. This is in agreement with observations abroad, and also in this country Capt. C. J. P. Cave has found that at Ditcham Park they accompany north-west winds that have crossed the South Downs. At Aberdeen the accompanying south-west wind has the characteristics of a föhn wind developed on a small scale. In dealing with cumulo-nimbus clouds it is observed that at Aberdeen a sub-type is of frequent occurrence in which the anvil-shaped extension of false cirrus is so well developed that the cloud sometimes consists of little else. Such clouds are seen in boisterous weather with northerly winds, and from them fall frequent showers of rain, hail, or sleet. Stratus clouds persist over Aberdeen for days at a time as unbroken grey sheets, often accompanied by exceptionally good visibility underneath. It would be interesting to have the author's explanation of this, for an inversion of temperature, or at least a region of small lapse rate, would be expected at a moderate elevation with these clouds, and this would hinder the removal of dust particles by convectional currents from the air near the ground, and would tend to produce poor visibility.

The second part of the book is the most valuable, but does not lend itself to discussion, for it consists of a large selection of excellent cloud forms, abundantly illustrating each type. It cannot be too strongly recommended, to professional and amateur alike.

E. V. N.

Meteorology: An Introductory Treatise. By A. E. M. GEDDES, D.Sc., O.B.E., M.A. [Pp. xx + 390, with 20 plates and 103 diagrams.] (London: Blackie & Sons, 1921. Price 21*s*. net.)

MR. GEDDES' book is very welcome, as no comprehensive text-book of Meteorology has been produced in this country in recent years which could be regarded as a standard work for the non-mathematical student. No aspect of modern meteorology can be said to have been neglected in this work, but it is unfortunate that in the section devoted to weather forecasting the novel theories of Professor Bjerknes and the enterprising experiments of the Norwegian Meteorological Service receive no mention, but this could hardly have been avoided without delaying the publication of the book. It is gratifying to note that atmospheric electricity receives so much attention, as it is not easy to keep in touch with this somewhat specialised branch of meteorology. It is to the advantage of the book that the author seeks to express rather the views of English meteorologists as a whole than to advance particular theories. The style throughout is easy and attractive, and the reader is not obliged to have already a knowledge of physics in order to follow the reasoning. For the majority of the fine illustrations the author is indebted to his fellow-citizen, Mr. G. A. Clarke, who has done so much to stimulate the study of clouds and the direct visual observation of the weather.

E. V. N.

CHEMISTRY

A Dictionary of Applied Chemistry. Edited by Sir Edward Thorpe, C.B., LL.D., F.R.S., assisted by eminent contributors. Vol. I, revised and enlarged edition, with illustrations. [Pp. x + 752.] (London: Longmans Green & Co., 1921. Price 60s. net.)

The manifold applications of chemical science in the Great War, and the general realisation of the fundamental importance of a flourishing chemical industry have made the publication of a new edition of Sir Edward Thorpe's well-known *Dictionary of Applied Chemistry* a very desirable and very necessary undertaking.

The style and arrangement of the previous edition has been retained and in general one may say that the volume under review is similar to its predecessor, with its good points and its failings magnified.

On the one hand, several articles have been fully revised, or entirely rewritten so that many of them form really reliable short monographs on the subjects dealt with. Such an article as Mr. J. J. Manley's exposition on Precision Weighing, for instance, is well worth studying, and should form a standard reference on the subject.

On the other hand, however, it is much to be regretted that the volume is still without an index, which renders it valueless for many purposes of quick reference, so that one is compelled against one's will to turn to the inevitable Ullmann's *Enzyklopædie*; and again, there is considerable overlapping of articles and lack of cross-references, which seriously diminish the value of the work.

As an instance we may take the case of Berberine; on looking this up on page 584, we find a 1½-column article giving a short account of the subject; apparently, therefore, that is all that there is known about it.

If, however, one happens to look up *Barberry* (pp. 534–8) one finds there a long and detailed account of the same product (8 columns), bringing the whole subject up to 1918; no cross-reference whatever is given to the latter article under the heading of Berberine.

This is merely a typical example; again, the concise article on Azo Dyes by the late Dr. J. C. Cain contains the names of many hundred colours, but no attempt whatever seems to have been made to index them in any place, so that for most purposes the article is almost useless, although, without any very apparent reason, one or two names occur here and there in the book, such as "Azo Coccine, see Azo Colouring Matters," but no reference is made to most of the other colours.

These weaknesses, which detract very seriously from the value of the book as a work of reference, are due chiefly to what one may perhaps term inadequate staff work, and it is to be hoped that in the later volumes, which are promised soon, these defects will be remedied; best of all, the last volume might well contain a comprehensive index to the whole Dictionary.

F. A. Mason.

Applied Colloid Chemistry. By Wilder D. Bancroft, Professor of Physical Chemistry at Cornell University. [Pp. viii + 345, with numerous diagrams.] (New York: McGraw-Hill Book Company, 1921. Price 18s. net.)

It is rare to find a treatise on colloidal chemistry written in such an interesting manner as this text-book. The empirical nature of the subject, with its accumulation of undigested facts, places considerable difficulties in the way of comprehensive treatment. The author, in his subdivision of the subject, has emphasised the importance of adsorption in colloidal chemistry. In this respect he has adopted the classification of Freundlich's *Kapillarchemie*, but the general methods of presentation are very different in the two cases. Indeed, the two works are to some extent complementary. It is intended

that this book should serve as the general introduction to a series of volumes on industrial colloid chemistry. It is an encyclopædia of information, with copious references to original papers up to 1920.

Special attention should be directed to the chapter on fog and smoke, which is a welcome contribution to this subject. It is considered that reference would be facilitated if the chapter on the properties of colloidal solutions were subdivided. The space afforded to semipermiable membranes is scarcely adequate, and the section on emulsions could be considerably extended.

This book is not only a valuable addition to the literature of applied colloid chemistry, but is also a useful text-book for students. W. E. G.

Creative Chemistry. By Edwin E. Slosson, M.S., Ph.D. [Pp. xii + 311, with 38 illustrations.] (London: University of London Press, 1921. Price 12*s*. 6*d*. net.)

Creative Chemistry is an attempt by the Literary Editor of *The Independent* to present some of the achievements of modern chemistry in a form such as would appeal to the lay mind. The author does not flatter his readers by assuming that the style which appeals to the "well-informed person" is that of the very popular press, and it is surprising that there are still literary editors in America capable of the following sentence: "The farmer in any country is apt to be set in his ways, and when it comes to inducing him to spend his hard-earned money for chemicals that he never heard of and could not pronounce he—quite rightly—has to be shown." Apart, however, from style the book is not all that could be desired from the point of view of accuracy. The section on explosives is particularly confused; trinitrotoluene is apparently regarded as an organic nitrate, the principle involved in the preparation of cordite is ignored, the nitro-cellulose being regarded as a mere absorbent of the nitro-glycerine, and cordite and similar substances are considered to be high explosives. A little more attention to the English, to the punctuation, and to the accuracy of some of the statements would have produced a readable book of a somewhat "racey" description.

O. L. B.

Experimental Organic Chemistry. By Augustus P. West, Ph.D. (New World Science Series. Edited by John W. Ritchie.) [Pp. xiii + 469.] (London: George G. Harrap & Co., 1921. Price 10*s*. 6*d*. net.)

Every teacher of organic chemistry has his own particular methods, which he is anxious to impress upon the world with the result that class-books continue to multiply. The author, a Professor of Chemistry in the University of the Philippines, has had special problems to face and has compiled a text-book of organic chemistry with very full directions for the preparation of a number of the commoner organic compounds. By making the experiments illustrative of the text he has avoided the cookery-book type of volume so often encountered. The subject-matter is well presented, and the experiments, in general, very fully described so as to present little difficulty to the novice; the concentrations or gravities of acids used are, however, not given, and an undesirable vagueness hovers over the term "concentrated nitric acid." Dangerous experiments are avoided, and the student is repeatedly warned not to get phenol on his hands, but it is surprising that no caution is given over the distillation of nitro-benzene, and in one preparation instructions are given to add concentrated nitric to concentrated sulphuric acid. There seem to be two objections to the book: the American spelling and the unusual practice of writing, for example, ortho-phenylenediamine as $(O)C_6H_4(NH_2)_2$. Organic chemistry is sufficiently complicated without worrying the elementary student with an alternative spelling of sulphur, etc., and a notation other than that almost universally adopted. O. L. B.

Practical Physiological Chemistry. By SIDNEY W. COLE, M.A. [Pp. xvi + 405, sixth edition.] (Cambridge: W. Heffer & Sons, 1920. Price 16s. net.)

THAT little more than a year should have elapsed since the issue of the fifth edition is eloquent testimony to the popularity of this work. The present edition does not differ materially from the last one, which was fully reviewed in these columns (July 1920), but it has been revised, and one or two new methods have been introduced which enhance the value of the book. The continued success of the book is assured.

P. H.

The Chemistry of Plant Life. By ROSCOE W. THATCHER, M.A., D.Agr. [Pp. xvi + 268.] (New York: McGraw-Hill Book Company, 1921. Price 18s. net.)

THE importance of a knowledge of chemistry to botanists, and more especially to plant physiologists, is gradually coming to be more generally recognised, with the result that some colleges are already providing courses of chemistry especially adapted to the needs of students of botany as distinct from agriculture. The book under review is the outcome of such a tendency, which is making itself felt also in America, and is compiled from the author's lecture notes on a course of Phytochemistry delivered by him as Professor of Plant Chemistry to the students of the Plant Science group of the University of Minnesota. While acknowledging his indebtedness to a number of other works on the same subject, the author explains that an attempt has been made to arrange the material in such a way as to proceed from simpler chemical principles and substances to those of more complex structures, which results in an arrangement of the groups to be studied in an order which is quite different from what their biological significance might suggest. The author is to be congratulated on producing a very readable little book on the subject which, with the exception of a number of mis-spellings of author's names, is on the whole singularly free from errors.

P. H.

The Physiology of Protein Metabolism. By E. P. CATHCART, M.D., D.Sc., F.R.S. (Monographs on Bio-chemistry.) [Pp. iv + 176.] (Longmans, Green & Co., 1921. Price 12s. 6d. net.)

THIS monograph first appeared in 1912. It consisted, in the words of the author, of "a discussion of the more important results published in the last decade and their bearing upon the work of the earlier investigators." Another cycle of years is almost complete, and has added a mass of new work to the abundant literature of protein metabolism; work associated with the names of Lusk, Van Slyke, Folin, Osborne, Mendel, and many others, chiefly in the United States. These years have brought us a more careful definition of the broad facts of the problem from the greater exactness of the newer methods, some misconceptions have been righted and some perplexities removed, but there has been no fundamental revolution of thought. Therefore the new edition of this monograph, whilst it marshals all the later work in its proper place in the discussion, is little changed in structure. Indeed, one moves with some feeling of congestion amongst the crowd of evidence that has been packed in the small confines of this volume. Happily the author is concerned to paint us a broad view, and, in avoiding undue attention to the experimental study of the metabolism of particular constituents of the protein molecule, saves his picture from eclipse in a mass of detail.

If there has been any startling accession of knowledge in this field it is to be found in the fascinating story of the discovery of the "accessory food substances." Ultimately no theory of metabolism may disregard the evidence

for these bandits of nutrition, but their rôle in the intimate metabolism of material—for rôle they must have—remains to the worker of to-day and to-morrow. However, in some of the earlier experiments designed on the general plan of feeding carefully controlled diets, faulty conclusions as to the general dietary balance and the efficiency of various sources of nitrogen, may well have been drawn where the deficiency more probably lay in a lack of vitamines. Some of the work quoted in the text may be open to this criticism.

A valuable new chapter on the influence of the non-nitrogenous food-stuffs upon protein metabolism and an exhaustive bibliography complete a book which the teacher, the worker, and the advanced student will gratefully welcome. R. K. C.

Bibliotheca Chemico-Mathematica. Compiled and Annotated by H. Z. and H. C. S. [2 vols. Pp. xii + 964, with 127 plates containing 247 portraits and facsimiles.] (London: Henry Sotheran & Co., 1921. Price 3 guineas net.)

Those interested in the history of science should find this "catalogue of works in many tongues on exact and applied science" a never-failing source of interest whenever they may delve among its many pages. In the preface the compilers make it clear that, as a catalogue of books for sale, it is not complete, but they claim that few of the great books will be found missing. This claim is, perhaps, rather far-reaching, except with a restricted definition of the term "great books," but the number of rare and interesting works that are included is amazing.

The authors have made their catalogue appeal to those who are interested in the historical aspect by the inclusion of very illuminating notes upon a large number of the more interesting works, indicating the chief subjects of historical interest in the volume with occasional biographical details, and by the reproduction in plate form of numerous title-pages, portraits, and figures of apparatus. Although a complete catalogue is still wanting, the compilers have done much for the historian of science in providing a useful reference book, in which is collected much information obtainable otherwise only by wearisome search.

To the collector and to the librarian the value of the book is obvious, providing, as it does, much bibliographical information, and a unique guide to the current values of books of this kind. O. L. B.

BOTANY AND AGRICULTURE

Text-book of Pastoral and Agricultural Botany for the Study of the Injurious and Useful Plants of Country and Farm. By John W. Harshberger, Ph.D., Professor of Botany, University of Pennsylvania. [Pp. xiii + 294.] (Philadelphia: P. Blakiston's Son & Co., 1920; London: Methuen & Co. Price 7s. net.)

The scope and arrangement of this book are very different from those of most text-books of agricultural botany, and the book is written from a different point of view. The first nine chapters deal with poisonous plants, these not only being enumerated and described, but the toxicology of the plants especially in relation to stock being dealt with. Thus, after a description of the plant there follows an account of the symptoms produced by it, a statement of the poisonous principles concerned, and in some cases an account of the treatment indicated.

Feeds and feeding are dealt with in a single very brief chapter, which is followed by six chapters dealing with the main crop plants. The book closes with chapters on weeds and agricultural seeds.

The arrangement of material in this book is certainly unusual, and it is difficult to understand what particular advantage is supposed to accrue from its adoption. The chief value of the book undoubtedly lies in the part dealing with poisonous plants, for here information is collected in one place which is probably not to be found within the covers of any other single volume of such small compass, and for this reason it should be found useful to agricultural students in this country. W. S.

How to Teach Agriculture: A Book of Methods in this Subject. By ASHLEY V. STORM, Ph.D., Professor and Chief of the Division of Agricultural Education and Director of Short Courses, University of Minnesota, and KARY C. DAVIS, Ph.D., Knapp School of Country Life, George Peabody College for Teachers. [Pp. vii + 434.] (London: J. B. Lippincott Company, 1921. Price 12*s*. 6*d*. net.)

THIS book is mainly intended for the teacher of agricultural students, but it will probably be of help and interest to the intelligent student as well. Sixteen of its eighteen chapters are designed to inform the reader how to organise and manage teaching in agriculture, and how to teach the various branches of the subject. The range of information given is very wide, among the hundreds of questions dealt with being "The Socratic method," "Skill," "Class Work in Animal Husbandry," "Soil Laboratory Work," "Responsibility to Local Press," and so on. Although the outlook of the book is possibly not identical with that of most teachers of agriculture in its various branches in this country, it undoubtedly contains a wealth of information based on the experience of those who are well acquainted with the matters on which they write. The work is well printed on good paper and is well written and illustrated. W. S.

The Bases of Agricultural Practice and Economics in the United Provinces, India. By H. MARTIN LEAKE, M.A., Sc.D., F.L.S. [Pp. viii+277.] (Cambridge: W. Heffer & Sons, 1921. Price 15*s*. net.)

TRUE advance in agriculture implies progress both in general farming methods and in the recognition and application of economic factors, and while the details vary from place to place, the general underlying principles remain the same. In the present volume, the author has recognised this fact, and, while his exposition deals with the development of agricultural practice and economics in India, his deductions are of much wider bearing.

Hitherto progress in India has been slow, as the climate presents many problems, and the needs of the people are distinctly specialised and vary considerably with locality. A forward movement is now taking place, and the advance promises to move steadily along various lines in accordance with the particular needs of the various districts.

On the agricultural side, special attention is given to the relations between the factors governing the growth of crops, distinction being drawn between those which can be brought under control, and those which cannot. Hybridisation, selection, manuring, and methods of cultivation are discussed from the point of view of the Indian agriculturist. On the economic side it appears that the most promising line of development is in co-operation of various kinds, as independent action without co-operation threatens to stultify advance.

The book is exceedingly suggestive, and should be read not only for its bearing on Indian agriculture, but for its breadth of vision and the wider view of the science of agriculture that it opens out.

W. E. B.

Les Mouvements des Végétaux. Du Réveil et du Sommeil des Plantes. Par Réné Dutrochet. [Pp. viii + 121.] (Paris: Gauthier-Villars et Cie, 1921.)

THIS little reprint of two memoirs of Dutrochet in the series of *Les Maîtres de la Pensée Scientifique* will be welcomed by all plant physiologists. In 1837 Dutrochet collected his more important works into two volumes bearing the title *Mémoire pour servir à l'histoire anatomique et physiologique des végétaux et des animaux*. It is from this work that the two memoirs on the movement of plants and sleep of plants have been reprinted. Dutrochet was a strong opponent of vitalism, maintaining that life phenomena should all be explicable on the laws of physics and chemistry, and it was from this point of view that he attacked the problems of plant and animal physiology, and so became one of the outstanding figures of biology in the early years of the nineteenth century. His outlook was the same as that of physiologists to-day, and his work on movements in plants and on endosmosis formed the basis for the subsequent great development of these subjects. We are glad to learn that the publishers contemplate also reprinting in the same series the work of Dutrochet on endosmosis. W. S.

ZOOLOGY

An Introduction to Zoology. By C. H. O'DONOGHUE, D.Sc., F.Z.S. [Pp. x + 501, with 178 illustrations in text.] (London: G. Bell & Sons, 1920. Price 16s. net.)

WE have felt some reluctance in writing a review of this work in view of certain facts connected with its issue. In the copies originally issued the author in his preface states " . . . much if any good it may contain is due *indirectly* to the excellent courses given by Prof. A. Dendy, F.R.S., and Prof. J. P. Hill, F.R.S., of the University of London, to both of whom the author is deeply indebted."

In the copies issued for review at a later date, the page vii of the preface has been cut out, and a new one gummed in; the new preface reads like the old one except for the addition of the following paragraph after the words " deeply indebted ": " In the case of Prof. Hill, moreover, my indebtedness is more direct, since, for several years, I was in charge of the practical classes accompanying his lectures, and so had an opportunity of becoming familiar with them. This was of considerable advantage to me in preparing the book, *parts of which are more or less directly based upon his lecture notes*; and I desire, therefore, to express to him my sincere thanks." (The italics are mine.)

The book is bristling with errors, some minor, yet very misleading for the student, others much more serious. Some of these mistakes are as follows: Page 15, " internal nares," instead of " external nares "; page 27, " parietal " instead of " parasphenoid "; page 44, " dermis " and " epidermis " wrongly placed; page 62, " spermatic " instead of " splenic "; page 67, the author confuses " arteries " and " veins "; page 81, nerve missed out in diagram. Articles on Protozoa very " shaky." Diagram on page 196 wrong. Page 254, " myelencephalon " confused with " metencephalon," and " pineal " with " pituitary "; page 259, innervation of eye muscles confused; page 310, " pulmonary veins " do not open into " right atrium "; pages 368–370, description of " Reduction " in germ-cell inaccurate; page 463, Darwin misquoted.

Throughout, the author's grammar is slipshod, and the English bad. We have no doubt that the author should have got someone to read the proofs carefully, for this is most desirable in a book destined to be used by students. As it stands now, it only seems to confuse them with regard to many problems which they are expected to understand clearly.

J. BRONTË GATENBY.

The Pigmentary, Growth and Endocrine Disturbances induced in the Anuran Tadpole by the early Albation of the Pars buccalis of the Hypophysis. [Pp. 112, with 19 plates, 10 figures and 11 tables.] (American Anatomical Memoirs. Wistar Institute of Anatomy and Biology, Philadelphia. October 1920. Price $3.)

THE animal principally used in these experiments, of which this memoir is an account, was *Rana boylei* Baird, although *Bufo boreas* was also so employed. Larva 3½–4 mm. long proved to be most suitable for the purpose, since at that time the reflex action has not made its appearance and the hypophysea ingrowth is fairly readily removed without damage to the brain or other tissues. Both species proved fairly easy to rear under the conditions demanded by the experiment, and could be maintained for relatively long periods of time after operation. This early removal of the epithelial component of the hypophys induces disturbances in the pigmentary system, the ratio of growth and alterations in the structures of most other endocrinic organs. The changes in pigmentation result from a diminution in free epidermal pigment, a reduction in number and content of the epidermal melanophores and an expansion of the xantholeucophores. The result is the production of a silver grey "albino" and the cause of the changes is studied by reciprocal skin exchanges. The neural lobe and pituitary floor appear to be dependent upon the presence of the epithelial hypophyses for their complete development. The thyroid and adrenal cortex are strikingly diminished in size, the adrenal medulla suffers a qualitative change as revealed by staining. It is interesting to find that in cases where the extirpation was incomplete partial albinos were produced, but these did not show such deep-rooted changes in the other endocrine organs.

The growth of the tadpole after buccal hypophysectomy is retarded slowly at first but increasing to a maximum in the mid-larval period. However, by feeding with fresh anterior lobe of beef pituitary, a practically normal growth rate can be restored and since the animals do not metamorphose they attain an even larger size than normal tadpoles. These growth "maintaining" substances cannot be extracted by boiling in water or alcohol, nor does this procedure destroy them since they are left in the residue.

The paper is printed and illustrated in the excellent manner that is now associated with the publications of the Wistar Institute, and forms a valuable piece of physiological experimentation.

C. H. O'D.

PHYSIOLOGY

Essentials of Physiology. By F. A. BAINBRIDGE, M.D., D.Sc., F.R.C.P., F.R.S., and J. ACWORTH MENZIES, M.A., M.D. [Pp. iv + 497, with 194 illustrations. Fourth edition.] (London: Longmans, Green & Co., 1920. Price 14*s*. net.)

THIS is an unusually excellent little book. It contains in less than 500 pages of clear type, with nearly 200 illustrations, not merely a résumé of the main facts of physiology, but, in addition, a connected and reasoned account of the science, with no inconsiderable amount of lucid and concise explanation.

The illustrations are good as well as numerous; but mammalian cardiac nerves would be more interesting than those of the frog in fig. 112, and a schema of the lung more helpful than the diagram given on p. 275.

Judged from the order of presentation of the various systems, the work is intended as an aid to revision rather than as a first book for the beginner. It is particularly well adapted for the former purpose, and we recommend it, cordially, to the student of medicine as a valuable help towards polishing, summarising, and co-ordinating his earlier impressions of text-book physiology.

W. L. S.

ENGINEERING

The Mechanical Principles of the Aeroplane. By S. BRODETSKY, M.A., Ph.D. [Pp. vii + 272, with 119 illustrations.] (London: J. & A. Churchill, 1921. Price 21*s*. net.)

DR. BRODETSKY takes his place between Greenhill, Bothezat, Bader, and Joukowsky on the one hand, and Bairstow and Thomson on the other.

The former attempt to place Aerodynamics on a basis of mathematical physics, the latter are content to build on a foundation of results obtained experimentally in the wind tunnel and the field.

Dr. Brodetsky follows both methods, but it must be admitted that there seems little hope of bridging the gap between them until the mechanism of turbulent flow is better understood than at present.

This is a field of effort which should attract the best intellect to be found among students of Aerodynamics.

The introduction is an ambitious attempt to classify the problems of flight in eight pages.

Chapter I, on dimensions, should be digested with thoroughness, and should lead to an irresistible preference for a consistent system of units, and thence to the sole employment of the C.G.S. system, and of the practical units derived from it.

Chapter II is restricted to dynamics of a particle in a resisting medium. and is useful as an introduction to bomb-dropping rather than to aeroplane theory. More than due weight is given to Lanchester's contributions to the theory and nomenclature of stability. Bryan's long pitch and tumbler flight or plain pitching, stalling, and looping are preferable to phugoid, and to such a phrase as "catastrophic instability."

Chapters III and IV develop the rigid dynamics of the aeroplane along the lines of Bryan's classical work, and, later on, Chapters VII and VIII go further into the numerical determination of rotary and linear derivatives, and their application to actual problems of stability, and of free and forced oscillations.

Much of the treatment is fresh and worth the consideration of those whose professional business it is to carry out such calculations as well as of the ambitious student.

Chapters V and VI recapitulate the well-worn methods of conformal representation applied to the flow of an ideal fluid without and with free stream lines. It cannot be said that this abstract theory has much relation to the flow of viscous fluids as we observe them, yet the student simply cannot afford to neglect the acquirement of at least some familiarity with the method and its results, if only to clear his mind of any lingering false analogy with the principles of rigid dynamics. In particular, Bernoulli's theorem can be applied where we have established experimentally the general lines of flow.

The discussion of airscrew theory in six pages at the end of Chapter VII will give some idea of the severe condensation from which the exposition suffers or benefits according to the point of view.

The author has not adopted the nomenclature of the Royal Aeronautical Society, which imposes on the reader the burden of interpreting yet another system of symbols.

Yet these criticisms are rather of detail, for this must take its place as one of the most serious and valuable text-books yet published towards establishing a rational theory of the aeroplane. A. R. L.

The Dynamics of the Airplane. By KENNETH P. WILLIAMS, Ph.D. (Mathematical Monographs, No. 21.) [Pp. viii + 138, with 50 diagrams.] (New York: John Wiley & Sons; London: Chapman & Hall, 1921. Price 13*s*. 6*d*. net.)

THE application of dynamical principles to the motion of the aeroplane is

of increasing importance, both from the practical point of view and from the standpoint of the teacher and student of mechanics. There is no part of dynamical theory that does not enter into the discussion of aeroplane motion, and, although a full treatment of the subject is bound to be very difficult, it is yet possible to present it in such a way as to appeal to such as have only mastered the elements of applied mathematics. Professor Williams' aim is to provide an elementary account, based mainly on the course of lectures delivered by Professor Marchis in Paris in 1919. The dynamical problem of the aeroplane is set forth in detail. After an introductory chapter on the aerodynamics of the plane and of the cambered surface, there are special chapters on straight horizontal flight, descent and ascent, circular flight (including circular or helical descent), performance from the point of view of ceiling and radius of action, and stability and controllability, including the effects of gusts of wind. There is also a brief discussion of the propeller. The book thus gives a considerable amount of matter that is not to be found set out conveniently elsewhere: the treatment is on the whole clear and interesting.

There are, however, several objectionable features in the book. The notation is not consistent, and therefore confusing. Errors of a literary or typographical kind are frequent enough to be irritating. References to French literature on the subject are marred by offences against the laws of French orthography. The writer refers several times to a well-known English book on aeronautics, and in each case the name of one of the joint authors is given wrong: the name of the author of another English work is spelt wrongly. Such errors do not, of course, destroy the value of the book. Far more important, however, are some statements of dynamical theory which one cannot commend to students of mechanics. Thus, it is not correct to say that " the resultant pressure is a vector. . . . We know all there is to know about it if we know its magnitude and its moment about some point." In another place the author says of a rigid body: " the force determines the instantaneous motion of the centre of gravity, and the couple determines the rotation which the machine is momentarily undergoing "; surely in each case it is the acceleration that is determined.

The subject matter of the book is useful and valuable. It is to be hoped that, if a second edition is issued, care will be taken to eliminate mistakes of the kind indicated, and to improve the treatment in the direction of presenting the subject in a more unified manner.

MISCELLANEOUS

Principles of Human Geography. By ELLSWORTH HUNTINGTON and SUMNER W. CUSHING. [Pp. xiv + 430, with 118 illustrations and maps.] (New York: John Wiley & Sons; London: Chapman & Hall, 1921. Price 21s. net.)

THE authors of this very comprehensive study of geography state that it is their purpose, in the first instance, to set forth the great principles of geography in its human aspects, then to provide a text-book for students able to think for themselves, and to furnish would-be teachers with a solid grounding in the human relationships which they are eager to teach.

The physical background of geography is more or less taken for granted, and a study made in every case of man's relation to his physiographic environment. In turn, a careful study is made of man's relation to physical environment in general, to location, land forms, bodies of water, soil and minerals, climate, vegetation, and animals, and to man himself. As can be imagined from such an arrangement, a certain amount of overlapping or repetition is almost unavoidable, but probably not more than is advantageous to the young teacher.

On the whole, the book can be strongly recommended for the purpose for

which it has been written, and, in spite of its rather heavy price, should be found at least in the library of every teacher of geography, and in that of every school in which geography is properly and seriously taught. A word of caution as to its use may be necessary. The authors are right in claiming that their book is written for students able to think for themselves. If, in reading this book, this habit is not carefully exercised, one source of error will at once arise. So many generalisations are made that, in the absence of definite statements as to the relative importance of the physiographical principles involved, it is possible for any one of the many conclusions to have a quite disproportionate value placed upon it. It is easily seen how this may arise in a book where an attempt is being made to connect physiographical environment with such widely different considerations as human character, activity, mentality, and even international relationship and history.

It is rather unfortunate that, in one of the few items where the physiographical background is explained in detail, namely tides, so inadequate an explanation should be given. Also in the reference to the London water-supply a misleading impression is inevitable to the American reader, for whom the book is primarily written.

Geographers are proverbially careless in their choice of terms, and it seems a great pity that a loose habit of this kind should be persisted in by modern writers. The use of the term "subtropical" to indicate a special climate—as opposed to monsoon—is, to say the least of it, very unfortunate.

W. C. Brown.

The Great Riddle. By Frank Horridge. [Pp. iii + 99.] (London: Kegan Paul, Trench, Trübner & Co., 1921. Price 3s. 6d. net.)

This book is an essay on the effects of natural forces and conditions in the creation. It is evident that the author knows nothing about natural forces and conditions, or the creation. It is truly the strangest mixture of rubbish and antique scientific knowledge we have read for some time. Indeed "the Great Riddle," to us, is to know why and how the book was ever published. Mr. Frank Horridge, in addition to being a respondent in the question of "the Great Riddle," is also author of *Ballades of Olde France, Alsace, and Old Hollande.* Would that Mr. Frank Horridge had kept at the Ballades, and left the Great Riddle alone! We do not claim to be able to judge Olde Ballades, but we feel sure that we are correct in saying that Mr. Frank Horridge has not altogether been successful in this preliminary canter upon the fields of the Biological Science. We hope that he will refrain from publishing his further promised work "on a comprehensive and all-embracing scale." The last has been too much for us.

J. Bronté Gatenby.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

Elementary Algebra. Part II. By C. V. Durrell, M.A., Senior Mathematical Master, Winchester College, and R. M. Wright, M.A., Assistant Master, Eton College. London: G. Bell & Sons, 1921. (Pp. xxiii + 551 + lxxxv.) Price 5*s.* 6*d.* net.

Exponentials Made Easy, or the Story of "Epsilon." By M. E. J. Gheury de Bray. London: Macmillan & Co., St. Martin's Street, 1921. (Pp. x + 253.) Price 4*s.* 6*d.* net.

Three Lectures on Fermat's Last Theorem. By L. J. Mordell, Manchester College of Technology. Cambridge: at the University Press, 1921. (Pp. 31.) Price 4*s.* net.

A Treatise on the Integral Calculus, with Applications, Examples, and Problems. By Joseph Edwards, M.A., Principal of Queen's College, London. Vol. I. London: Macmillan & Co., St. Martin's Street, 1921. (Pp. xx + 907.) Price 50*s.* net.

Cinematique et Mecanismes. Par R. Bricard, Professeur au Conservatoire des Arts et Métiers. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel. (Pp. 212.) Price 5 fcs.

Traité Pratique de Geométrie Descriptive. Par J. Geffroy, Ingénieur des Arts et Manufactures, Professeur à l'École Centrale. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel. (Pp. ii + 191.) Price 5 fcs.

Statique et Dynamique. Par Henri Beghin, Professeur à l'École Navale. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel. (Pp. viii + 200.) Price 5 fcs.

Simple Lessons on the Weather for School Use and General Reading. By E. Stenhouse, B.Sc., Associate of the Royal College of Science, London. London: Methuen & Co., 36 Essex Street, W.C. (Pp. viii + 135, with 12 plates and 62 other illustrations.)

Les Théories d'Einstein. Nouvelle Édition Épurée, accrue de Notes Liminaires, d'un Exposé des Théories de Weyl, et de Trois Notes de MM. Guillaume, Brillouin, et Sagnac sur leurs propres idées. By Lucien Fabre. Paris: Payot et Cie, 106 Boulevard Saint-Germain, 1921. (Pp. 255.) Price 7 fr. 50.

Vorlesungen über die Theorie der Wärmestrahlung. Von Dr. Max Planck, Professor der Theoretischen Physik an der Universität Berlin. Vierte, abermals Umgearbeitete Auflage. Leipzig: Verlag von Johann Ambrosius Barth, 1921. (Pp. xi + 224.) Price 26 marks.

Moderne Magnetik von Felix Auerbach. Leipzig: Verlag von Johann Ambrosius Barth, 1921. (Pp. viii + 304, with 167 text-figures.) Price 48 marks.

The Scientific Papers of the Honourable Henry Cavendish, F.R.S. Vol. I: The Electrical Researches; edited from the published papers and the Cavendish Manuscripts in the possession of His Grace the Duke of

Devonshire, K.G., F.R.S. By James Clerk Maxwell, F.R.S., Cavendish Professor of Experimental Physics in the University of Cambridge. Revised by Sir Joseph Larmor, F.R.S., M.P., Lucasian Professor of Mathematics. Vol. II: Chemical and Dynamical, by Sir Edward Thorpe, F.R.S.; with contributions by Dr. Charles Chree, F.R.S., Sir Frank Watson Dyson, F.R.S., Sir Archibald Geikie, O.M., F.R.S., Sir Joseph Larmor, F.R.S. Cambridge: at the University Press, 1921. (Pp. Vol. I, xxviii + 452; Vol. II, xii + 496.) Price £6 net the two volumes.

The Reign of Relativity. By Viscount Haldane. London: John Murray, Albemarle Street, W., 1921. (Pp. xxiii + 427.) Price 21*s*. net.

Electrons and Ether Waves. Being the Twenty-third Robert Boyle Lecture, on 11th May, 1921. By Professor William Bragg, F.R.S. London: Oxford University Press, 1921. (Pp. 14.) Price 1*s*. net.

Rayonnement. Principes Scientifiques de l'Éclairage par A. Blanc, Professeur à la Faculté des Sciences de Caen. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel. (Pp. vi + 212.) Price 5 fcs.

Traité de Dynamique. By Jean D'Alembert. Les Maîtres de la Pensée Scientifique. Collection de Mémoires et Ouvrages. Publiée par les soins de Maurice Solovine. Paris: Gauthiers-Villars et Cie, Éditeurs, Libraires du Bureau des Longitudes, de l'École Polytechnique, Quai des Grands-Augustins, 55, 1921. (Pp. Part I, xl + 102; Part II, 186.)

A French-English Dictionary for Chemists. By Austin M. Patterson, Ph.D.; formerly Editor of *Chemical Abstracts*. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. xvii + 384.) Price 18*s*. net.

Theorie Cinetique des Gaz. Par Eugène Bloch, Professeur au Lysée St. Louis. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel. (Pp. 174.) Price 5 fcs.

Factory Chemistry. Preparatory to Courses in Metallurgy and Metallography. By Wm. H. Hawkes, A.B., M.Sc., Department of Chemistry, Ford Institute of Technology, Detroit, Michigan. London: Longmans, Green & Co., 39 Paternoster Row; New York: Fourth Avenue and 30th Street, 1921. (Pp. vii + 59.) Price 4*s*. 6*d*. net.

Biochemistry. A Study of the Origin, Reactions, and Equilibria of Living Matter. By Benjamin Moore, M.A., D.Sc., F.R.S., Whitley Professor of Biochemistry, University of Oxford. London: Edward Arnold, 1921. (Pp. vii + 340.) Price 21*s*. net.

Fundamental Principles of Organic Chemistry. By Charles Moureu, Member of the Institute and of the Academy of Medicine, Professor at the Collège de France. Authorised Translation from the Sixth French Edition by Walter T. K. Braunholtz, B.A., A.I.C. With an Introduction by Sir William J. Pope, K.B.E., F.R.S., Professor of Chemistry in the University of Cambridge. London: G. Bell & Sons, Ltd., 1921. (Pp. xviii + 399.) Price 12*s*. 6*d*. net.

The Formation of Colloids. By the Svedberg Professor of Physical Chemistry in the University of Upsala. London: J. & A. Churchill, 7 Great Marlborough Street, 1921. (Pp. viii + 127, with 22 illustrations.) Price 7*s*. 6*d*. net.

Dairy Bacteriology. By Orla-Jensen, Dr. Phil., Professor of Technical Biochemistry in the Polytechnic College, Copenhagen; formerly Director of the Swiss Experimental Dairy Station. Translated from the Second Danish Edition, with additions and revisions by P. S. Arup, B.Sc., F.I.C., Chief Chemist to English Margarine Works (1919), Limited. London: J. & A. Churchill, 7 Great Marlborough Street, 1921. (Pp. xii + 180, with 70 illustrations.) Price 18*s*. net.

Animal Proteins. By Hugh Garner Bennett, M.Sc., Member of the Society of Leather Trades' Chemists. London: Bailliere, Tindall & Cox, 8 Henrietta Street, Covent Garden, 1921. (Pp. xiii + 287.) Price 15*s*. net.

Petrographic Methods and Calculations. With some Examples of Results achieved. By Arthur Holmes, D.Sc., A.R.C.S., D.I.C., F.G.S., F.R.G.S. London: Thomas Murby & Co., 1 Fleet Lane, E.C. 4. (Pp. xix + 515, with 4 plates and 83 diagrams.) Price 31*s*. 6*d*. net.

Elements of Engineering Geology. By H. Ries, Ph.D., Professor of Geology, Cornell University, and Thomas L. Watson, Ph.D., Professor of Geology, University of Virginia, and State Geologist of Virginia. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. iv + 365, with 252 figures.) Price 22*s*. net.

The Elements of Vegetable Histology. By C. W. Ballard, Associate Professor of Materia Medica and Director of the Microscopical Laboratory, College of Pharmacy, Columbia University; Microanalyst, Department of Health, City of New York. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. xiv + 246, with 75 figures.) Price 18*s*. net.

Les Mouvements des Végétaux. Du Réveil et du Sommeil des Plantes. By Réné Dutrochet. Les Maîtres de la Pensée Scientifique. Collection de Mémoires et Ouvrages. Publiée par les soins de Maurice Solovine. Paris: Gauthier-Villars et Cie, Éditeurs, Libraires du Bureau des Longitudes, de l'École Polytechnique, Quai des Grands-Augustins, 55, 1921. (Pp. viii + 121.)

Agricultural Economics. By James E. Boyle, Ph.D., Extension Professor of Rural Economy, College of Agriculture, Cornell University. London: J. B. Lippincott Company. (Pp. ix + 443, with 90 illustrations.) Price 12*s*. 6*d*. net.

Lichens. By Annie Lorrain Smith, F.L.S., Acting Assistant, Botanical Department, British Museum. Cambridge: at the University Press, 1921. (Pp. xxviii + 464, with 135 figures.) Price 55*s*. net.

A Text-book of Botany for Medical and Pharmaceutical Students. By James Small, D.Sc., Ph.D., F.L.S., Professor of Botany in the Queen's University of Belfast, late Lecturer in Botany to the Pharmaceutical Society of Great Britain. London: J. & A. Churchill, 7 Great Marlborough Street, 1921. (Pp. x + 681, with 1,350 illustrations.) Price 25*s*. net.

The Garden of Earth. A little book on Plant-life, Plant-growth, and the Ways and Uses of Plants. By Agnes Giberne. London: Society for Promoting Christian Knowledge; New York: The Macmillan Company, 1921. (Pp. xiv + 178, with illustrations.) Price 6*s*. 6*d*. net.

Insects and Human Welfare. An Account of the more important Relations of Insects to the Health of Man, to Agriculture, and to Forestry. By Charles Thomas Brues, Assistant Professor of Economic Entomology, Bussey Institution, Harvard University. Cambridge: Harvard University Press; London: Oxford University Press, 1920. (Pp. xii + 104, with 42 figures.) Price 10*s*. 6*d*. net.

Insect Pests of Farm, Garden, and Orchard. By E. Dwight Sanderson; formerly Entomologist of the Delaware, Texas, and New Hampshire Agricultural Experiment Stations, and Director of the New Hampshire and West Virginia Agricultural Experiment Stations. Second Edition, Revised and Enlarged. By Leonard Marion Peairs, Professor of Entomology, West Virginia University. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. vi + 707, with 604 figures.) Price 26*s*.

Ameboid Movement. By Asa A. Schaeffer, Ph.D., Professor of Zoology, University of Tennessee. Princeton: Princeton University Press; London: Oxford University Press, 1920. (Pp. vii + 156, with 46 figures.) Price 10*s*. 6*d*. net.

The Direction of Human Evolution. By Edwin Grant Conklin, Professor of Biology in Princeton University. London: Oxford University Press. (Pp. xiii + 247.) Price 12*s*. 6*d*. net.

Nature All the Year Round. By J. Arthur Thomson, M.A., LL.D., Professor of Natural History, Aberdeen University. London: The Pilgrim Press, 16 Pilgrim Street, E.C. 4. (Pp. viii + 253, with 52 illustrations by Alice M. Davidson.) Price 12*s*. 6*d*. net.

Mountain and Moorland. By J. Arthur Thomson, M.A., LL.D., Regius Professor of Natural History in the University of Aberdeen. London: Society for Promoting Christian Knowledge; New York: The Macmillan Co., 1921. (Pp. viii + 176, with 7 illustrations.) Price 6*s*. net.

The New Stone Age in Northern Europe. By John M. Tyler, Professor Emeritus of Biology, Amherst College. London: G. Bell & Sons, 1921. (Pp. xx + 310, with 22 illustrations.) Price 15*s*. net.

Prehistory. A Study of Early Cultures in Europe and the Mediterranean Basin. By M. C. Burkitt, M.A., F.G.S., with a short Preface by L'Abbé H. Brueil, Professor at the Institute of Human Palæontology, Paris. Cambridge: at the University Press, 1921. (Pp. xx + 439, with 47 plates.) Price 35*s*. net.

Insanity and Mental Deficiency in relation to Legal Responsibility. A Study in Psychological Jurisprudence. By William G. H. Cook, LL.D., of the Middle Temple, Barrister-at-Law; King Edward VII Research Scholar of the Middle Temple. Thesis approved for the Degree of Doctor of Laws in the University of London. London: George Routledge & Sons; New York: E. P. Dutton & Co., 1921. (Pp. xxiv + 192.) Price 10*s*. 6*d*. net.

La Forme et le Mouvement. Essai de dynamique de la vie. Paris: Ernest Flammarion, Éditeur, 26 Rue Racine. (Pp. xi + 173, with 15 figures.) Price 4 fr. 50 net.

Addresses on Psycho-analysis. By J. J. Putnam, M.D., Emeritus Professor of Neurology, Harvard University, with a Preface by Sigmund Freud, M.D., LL.D. London: The International Psycho-analytical Press, and George Allen & Unwin, and Vienna and New York, 1921; the International Psycho-analytical Library, No. 1, 1921. (Pp. viii + 470.) Price 12*s*. 6*d*. net.

Psycho-analysis and the War Neuroses. By Drs. S. Ferenczi (Budapest), Karl Abraham (Berlin), Ernst Simmel (Berlin), and Ernest Jones (London), with an Introduction by Prof. Sigmund Freud (Vienna). London, Vienna, and New York: The International Psycho-analytical Press and George Allen Co.; the International Psycho-analytical Library, No. 2. (Pp. 58.) Price 7*s*. 6*d*. net.

Vitamines. Essential Food Factors. By Benjamin Harrow, Ph.D., Associate in Physiological Chemistry, College of Physicians and Surgeons, Columbia University. London: George Routledge & Sons. (Pp. xi + 219.) Price 10*s*. 6*d*. net.

Life of Alfred Newton, Professor of Comparative Anatomy, Cambridge University, 1866–1907. By A. F. R. Wollaston, with a Preface by Sir Archibald Geikie, O.M. London: John Murray, Albemarle Street, W., 1921. (Pp. xv + 332, with 5 illustrations.) Price 18*s*. net.

Aeroplane Performance Calculations. By Harris Booth, B.A., Assoc. M.Inst.C.E., F.R.Ae.S., Late Technical Adviser to the Air Department of the Admiralty, Technical Adviser to the Commercial Aeroplane Wing Syndicate. London: Chapman & Hall, 11 Henrietta Street, W.C. 2, 1921. (Pp. xv + 208.) Price 21*s*. net.

La Construction du Vaisseau de Guerre. Par E. Jammy, Ingénieur aux Forges et Chantiers de la Mediterranée. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel. (Pp. iv + 208.) Price 5 fcs.

Télégraphie et Téléphonie sans Fil. Par C. Gutton, Professeur à la Faculté des Sciences de Nancy. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel. (Pp. 188.) Price 5 fcs.

The Electric Furnace. By J. N. Pring, M.B.E., D.Sc., Research Department, Royal Arsenal, Woolwich. London: Longmans, Green & Co., 39 Paternoster Row, 1921. (Pp. xii + 484, with 19 plates and 240 figures.) Price 32*s*. net.

Principles of Radio Communication. By J. H. Morecroft, Associate Professor of Electrical Engineering, Columbia University, assisted by A. Pinto and W. A. Curry. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. x + 935, with numerous figures.) Price 45*s*. net.

From a Modern University. Some Aims and Aspirations of Science. By Arthur Smithells, Professor of Chemistry in the University of Leeds. London: Oxford University Press, 1921. (Pp. 124.) Price 12*s*. 6*d*. net.

A Manual of Seismology. By Charles Davison, Sc.D. Cambridge: at the University Press, 1921. (Pp. xi + 256, with 100 figures.) Price 21*s*. net.

Hyperacoustics. By John L. Dunk. Division II: Successive Tonality. London: J. M. Dent & Sons; New York: E. P. Dutton & Co., 1921. (Pp. xi + 160.) Price 5*s*. net.

Logic. Part I. By W. E. Johnson, M.A., Fellow of King's College, Cambridge, Sidgwick Lecturer in Moral Science in the University of Cambridge. Cambridge: at the University Press, 1921. (Pp. xl + 255.) Price 16*s*. net.

Le Mouvement Scientifique Contemporain en France. I. Les Sciences Naturelles. By Georges Matisse, Docteur és Sciences. Paris: Payot et Cie, 106 Boulevard Saint-Germain, 1921. (Pp. 160, with 25 figures.) Price 4 fcs.

Critical Microscopy. How to get the Best out of the Microscope. By Alfred C. Coles, M.D., D.Sc., M.R.C.P., F.R.S. Edin. London: J. & A. Churchill, 7 Great Marlborough Street, 1921. (Pp. viii + 100, with 8 plates.) Price 7*s*. 6*d*. net.

Optical Theories. Based on Lectures delivered before the Calcutta University. By D. N. Mallik, B.A., Sc.D., F.R.S.E., Professor, Presidency College, Calcutta. Second Edition (revised). Cambridge: at the University Press, 1921. (Pp. ii + 202.) Price 16*s*. net.

A First Course in Statistics. By D. Caradog Jones, M.A., F.S.S., formerly Lecturer in Mathematics at Durham University. London: G. Bell & Sons, 1921. (Pp. ix + 286.) Price 15*s*. net.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By F. PURYER WHITE, M.A., St. John's College, Cambridge.

THE German *Encyclopædia of Mathematics* was begun in the closing years of the last century, but so far only volume 1 (Arithmetic and Algebra) and the first division of volume 2 (Analysis, Real Numbers) have been completely published. During the present year, however, considerable progress has been made. In Analysis, L. Bieberbach (Heft 4) gives an account of recent investigations in the theory of functions of a complex variable, to supplement Osgood's article, which appeared in 1900. In Geometry four parts have appeared: M. Zacharias and W. F. Meyer (III. 1. 6) conclude their article on "Synthetic Geometry" (Euclidean and non-Euclidean), G. Berkhan and W. F. Meyer (III. 1. 7) write on recent "Geometry of the Triangle," H. Rothe (III. 1. 7) on Grassmann's "Ausdehnungslehre," C. Segre (IV. 2. 7) on the algebraic geometry of hyperspace, and E. Salkowski (III. 3. 5) on triply-orthogonal systems of surfaces.

Several mathematical journals have ceased publication, largely no doubt owing to the enormous cost of production; a list is appended, with the last volume in each case:

Archiv der Mathematik (**28**, 1920).
Bibliotheca Mathematica (**14**, 1914–15).
Nouvelles Annales de Mathématiques (**79**, 1920).
Zeitschrift für Mathematik und Physik (**64**, 1917).

The *Mathematische Annalen*, after fifty years' publication by Teubner of Leipzig, has now, beginning with volume 81, been transferred to the firm of Springer in Berlin; it will in future not be strictly limited to Pure Mathematics.

Nature, for August 11, 1921, contains a list, probably incomplete, of leading German scientists who died during the war. Of mathematicians it includes R. Dedekind (February 1916), R. Helmert (June 1917), G. Frobenius (August 1917), and G. Cantor (January 1918).

An account of the work of Theodore Reye (1838–1919) is given by F. Schur (*Math. Annalen*, **82**, 1921, 165); he is perhaps best known by his book *Geometrie der Lage*, first published in 1866–8, and by his work on the tetrahedral complex, obtained as the locus of the intersections of corresponding planes of two collinear spaces.

H. G. Zeuthen (1839–1920) of Copenhagen is associated with Enumerative Geometry, with the Zeuthen-Segre invariant of an algebraic surface, and with books on the history of mathematics, notably *Die Lehre von den Kegelschnitten im Altertum*, 1886. Notices of his work are given by C. Juel (*Bull. de l'acad. roy. de Danemark*, 1919–20, 67), and by H. W. Richmond (*Proc. Lond. Math. Soc.*, **19**, 1921, xxxvi.).

Karl Rohn (1855–1920) made important contributions to our knowledge of quartic surfaces, in particular of ruled quartics and quartics with a triple point. He also showed that the maximum number of separated ovals possible for a quartic surface is ten. A list of his papers and an account of his work is given by O. Hölder (*Berichte d. Sächs. Akad.*, **72**, 1920, 109)

An extremely interesting biography of S. Ramanujan (1887–1920) is given by G. H. Hardy (*Proc. Lond. Math. Soc.*, **19**, 1921, xl.), together with a list of his papers.

For Martin Krause (1851–1920) and his work on the theory of hyperelliptic functions see a notice by G. Herglotz (*Berichte d. Sächs. Akad.*, **72**, 1920, 105).

A Memorial Volume to Henri Poincaré (*Acta Mathematica*, **38**, 1921) contains valuable analyses of his work by Poincaré himself and by J. Hadamard, biographical notices by P. Appell and by P. Boutroux, and correspondence with Mittag-Leffler and L. Fuchs.

History.—G. A. Miller (*Amer. Math. Monthly*, **28**, 1921, 256) has a note on the remarkably inaccurate formula $\frac{1}{2}a\ (a + 1)$ for the area of an equilateral triangle of side a, which is to be found in Boethius and Gerbert. Hankel called the letter in which Gerbert discusses the formula "the first mathematical paper of the Middle Ages which deserves this name," but Miller endeavours to show that this praise is not merited.

J. W. L. Glaisher (*Mess. Math.*, **51**, 1921, 1) writes on the early history of the signs + and −, which first occur in Widman's *Rechenung*, printed at Leipzig in 1489. He comes to the conclusion that they are derived from algebra and not from commerce, as was thought by De Morgan and Gerhardt.

An account of the first work on mathematics printed in the New World is given by D. E. Smith (*Amer. Math. Monthly*, **28**, 1921, 10); it was a commercial arithmetic entitled *Sumario Compendioso* by Juan Diez, and was printed in the City of

Mexico in 1556; it also contains some six pages of algebra, mostly on quadratic equations.

An interesting report on the progress of the complete edition of the works of Leibniz, which was undertaken by an International Commission in 1907, is given by Erdmann (*Sitzungsber. Preuss. Akad.*, 1921, 114). So far the only published result has been the first volume of a catalogue of the Leibniz MSS., of which 100 copies were distributed among the libraries of the world (*e.g.* the British Museum) in 1908. The second volume, entrusted to the Paris *Académie des Sciences*, has not appeared. Berlin has now undertaken the whole of the production, estimated to occupy thirty-nine octavo volumes, of which the first, containing non-scientific correspondence to 1676, is ready for the press.

A review of the work of Thomas Jan Stieltjes (1856–94) on continued fractions and on the integrals associated with his name is given by R. D. Carmichael (*Bull. Amer. Math. Soc.*, **27**, 1921, 170).

Logic and the Theory of Aggregates.—The *Monist* (vol. 31, July 1921, 437) contains a translation of a paper by M. Winter, "On the Logical Introduction to the Theory of Functions," in which the author attempts to appraise the value of the study of logistics.

E. L. Post (*Amer. Journ. Math.*, **43**, 1921, 163) writes on the general theory of Elementary Propositions, expanding and generalising the matter contained in vol. i., pt. i., sec. A, of Whitehead and Russell's *Principia Mathematica*.

L. E. J. Brouwer (*Proc. Amst. Acad.*, **23**, 1921, 949) deals with an intuitional basis for the theory of aggregates; H. Weyl (*Math. Zeitschr.*, **10**, 1921, 39), in a lecture given at the Mathematical Colloquium at Zürich in 1920, describes his own work and Brouwer's on this subject.

Other papers in this division are by M. Pasch (*Math. Zs.*, **11**, 1921, 124) on the origin of the number-concept, by C. Burali-Forti (*Rend. Lincei*, **30** (1), 1921, 175; **30** (2), 1921, 26) on real numbers and by Brouwer (*Proc. Amst. Acad.*, **23**, 1921, 955) on the question whether every real number can be expressed as a decimal.

Algebra and Analysis.—J. H. M. Wedderburn (*Trans. Amer. Math. Soc.*, **22**, 1921, 129) investigates division algebras, *i.e.* linear associative algebras in which division is permissible by every element except zero.

A. McAulay (*Proc. Roy. Soc.*, A. **99**, 1921, 292) has a paper on Multenions and their use in relativity theory.

Sir Thomas Muir (*Trans. Roy. Soc. S. Africa*, **10**, 1921, 21) publishes a note on axi-symmetric orthogonants.

M. Bôcher applied the statical problem of the positions of

equilibrium in a plane containing fixed particles repelling with forces inversely proportional to the distance to prove that if all the roots of a polynomial $f(x) = o$ lie on or within any closed polygon then all the roots of $f'(x) = o$ lie within that polygon. J. L. Walsh (*Trans. Amer. Math. Soc.*, **22**, 1921, 101; *Comptes Rendus*, **172**, 1921, 662) has further developed this method and applied it to locate the roots of the Jacobian of two binary forms.

L. E. Dickson (*Trans. Amer. Math. Soc.*, **22**, 1921, 167; *Amer. Journ. Math.*, **43**, 1921, 102) investigates completely which general homogeneous polynomials can be expressed as determinants with linear elements. Speaking geometrically his result is that the equation to every plane curve, every quadric surface, and a sufficiently general cubic surface can be expressed in this way, but that no other general surface or variety in higher space can be so expressed. Incidentally he points out that the proof given by Jessop (*Quartic Surfaces*, p. 161) of the number of disposable constants in the equation to a "determinant" quartic surface is not valid; a similar argument applied to a binary form would lead to incorrect conclusions.

L. E. Dickson (*C.R.*, **172**, 1921, 636) also investigates the theory of triples of polynomials in n variables with a composition theorem, viz. such that we have

$$f(x_1, x_2, \ldots x_n)\,\phi(\xi_1, \xi_2, \ldots \xi_n) = F(X_1, X_2, \ldots X_n),$$

where the $X_1, X_2, \ldots X_n$ are bilinear in the $x_1, x_2, \ldots x_n$, and the $\xi_1, \xi_2 \ldots \xi_n$.

E. T. Bell (*Amer. Math. Monthly*, **28**, 1921, 258) proves that if p is an odd prime not dividing $4^r - 1$, then the Bernoullian number B_{2pr} has its numerator divisible by p, the particular case in which $r = 1$ having been discovered by J. C. Adams.

H. Cramer (*Arkiv för Mat.*, **15**, 1921, No. 5) establishes a number of theorems concerning prime numbers, in particular concerning the order of the difference between two consecutive primes and concerning Landau's question whether there is always at least one prime between n^2 and $(n + 1)^2$.

Viggo Brun (*Proc. Camb. Phil. Soc.*, **20**, 1921, 299) obtains approximate formulæ for the function $[x]$, the number of integers not exceeding x, and for $\pi(x)$, the number of primes not exceeding x.

E. Landau (*Göttingen Nachrichten*, 1921, 88) gives a shorter version of Hardy and Littlewood's solution of Waring's Problem.

J. Liouville published in 1858–65 a series of eighteen articles in which he stated results which express equalities between sums of values of general arithmetical functions when the arguments of the functions involve the divisors of two numbers whose sum is given. An account of these formulæ, with refer-

ences to subsequent proofs, is given in Dickson's *History of the Theory of Numbers*, vol. ii; this should now be supplemented by a couple of notes and two long papers by E. T. Bell (*Bull. Amer. Math. Soc.*, **27**, 1921, 273, 330, and *Trans. Amer. Math. Soc.*, **22**, 1921, 1, 198).

An interesting article by L. E. Dickson (*Bull. Amer. Math. Soc.*, **27**, 1921, 312) points out that numerous writers, including Gauss, have thought erroneously that when they have found all *rational* solutions of a homogeneous equation they have thereby found *all integral* solutions of the corresponding non-homogeneous equation. For example, the equation $X^2 + 5Y^2 = Z$ can easily be solved in rational numbers, but to deduce therefrom the solution of $x^2 + 5y^2 = zw$ in integers involves a knowledge of all divisors of all numbers which can be put into the form $x^2 + 5y^2$. He follows this up (*ibid.*, **27**, 1921, 353) by developing a new method in Diophantine Analysis, based on the theory of ideals.

A more elementary paper by the same author (*Amer. Math. Monthly*, **28**, 1921, 244) deals with rational triangles and quadrilaterals.

G. H. Hardy and J. E. Littlewood (*Proc. Lond. Math. Soc.*, **20**, 1921, 15) investigate the lattice-points (*i.e.* points whose co-ordinates are both integral) of a right-angled triangle.

With regard to the Riemann Zeta-function we may note a paper by H. Weyl, whose versatility is wonderful, on the order of $\zeta(1 + ti)$ as t tends to infinity (*Math. Zs.*, **10**, 1921, 88), and one by Hardy and Littlewood (*ibid.*, **10**, 1921, 283) on the number of zeros of $\zeta(\sigma + it)$ on the critical line $\sigma = \frac{1}{2}$.

Georges Humbert, whose death on January 22, 1921, was announced in *Nature* for March 17, first became known by his geometrical applications of the theory of functions. Of late, however, the theory of algebraic numbers had engaged his attention and a memoir on ternary Hermite forms in an imaginary quadratic field has appeared since his death (*C.R.*, **172**, 1921, 497, and *Liouville*, **4**, 1921, 3).

W. H. Young (*Proc. Roy. Soc.*, A. 99, 1921, 252) finds sets of sufficient conditions for the transformation of the variables in multiple integrals, by a method involving the notion of "associated summabilities."

If $f(x, y) = f(x, y_1, y_2 \ldots y_{k-1}, \ldots)$ is a finite, measurable function of any number of variables we can define four partial derivates with respect to x (upper right, upper left, lower right, lower left), being the upper and lower limits of $\{f(x+h, y) - f(x, y)\}/h$, for $h > O$ (right) and $h < O$ (left). G. C. Young (*Proc. Lond. Math. Soc.*, **20**, 1921, 182) proves that the points at which one of the upper derivates, being finite, is not equal to the lower derivate on the other side form a

set of linear content zero. Similar questions are dealt with by S. Banach (*C.R.*, **173**, 1921, 457).

Denjoy's extension of the Lebesgue integral to functions whose set of points of non-summability is not restricted to have content zero, but can be any non-dense closed set, has already got into the textbooks (see Hobson : *Real Variable*, 2nd edition, vol. i. cap. 8). The Denjoy integral has been applied by R. L. Borger (*Bull. Amer. Math. Soc.*, **27**, 1921, 325) to widen still further the conditions under which the Cauchy-Goursat Theorem is valid. Denjoy himself, in a series of papers (*Proc. Amst. Acad.*, **23**, 1921, 50, 220 ; *C.R.*, **172**, 1921, 653, 833, 903, 1218 ; **173**, 1921, 127) continues his researches on "totalisation" and on trigonometrical series.

G. H. Hardy (*Proc. Camb. Phil. Soc.*, **20**, 1921, 304) finds a necessary and sufficient condition that a series should be summable (*C*, 1).

There are two general methods of transforming a series into one converging more rapidly ; the first is associated with Euler and Stirling, and was developed later by Markoff; the second, invented by Kummer, was extended by Leclert and Catalan. H. B. A. Bockwinkel (*Nieuw. Archief voor Wiskunde*, **13**, 1921, 383) now shows that they are essentially the same.

G. H. Hardy (*Mess. Math.*, **50**, 1921, 165) continues his useful notes on the Integral Calculus with a note on Mellin's Inversion Formula, and S. Pollard (*ibid.*, **50**, 1921, 151) evaluates some definite integrals by means of Fourier's Integral Theorem.

J. Wolff (*Proc. Amst. Acad.*, **23**, 1921, 585) discusses a theorem of Picard on the behaviour of a uniform analytical function in the neighbourhood of an isolated essential singularity.

The Italian mathematicians are unrivalled for the elegance of their geometrical treatment of matters connected with the theory of functions ; so that we welcome a series of articles by G. Castelnuovo (*Rend. Lincei*, **30** (1), 1921, 50, 99, 195, 355) on Abelian Functions and another by F. Severi (*ibid.*, **30** (1), 1921, 163, 204, 231, 276, 296, 328, 365) on simple integrals of the first kind belonging to an algebraic surface.

A. Buhl (*Annales de Toulouse*, **10**, 1921, 175) discusses the circumstances which reduce the number of double integrals of the second kind belonging to an algebraic surface.

On the elliptic function transformation of the seventh and higher orders there are papers by A. Berry (*Mess. Math.*, **50**, 1921, 187) and J. H. McDonald (*Bull. Amer. Math. Soc.*, **27**, 1921, 366).

H. Villat (*Annales de l'école normale supérieure*, **38**, 1921, 183) generalises the Schwarz method for conformal representation to the case of doubly-connected areas which need not be symmetrical.

J. F. Ritt (*Annals of Math.*, **22**, 1921, 157) in a paper on the conformal mapping of a region into a part of itself, shows that a ring cannot be shrunk conformally into a ring lying in its interior.

E. Goursat (*Annales de Toulouse*, **10**, 1921, 65, *Bull. de la Soc. Math. de France*, **49**, 1921, 1) continues his researches on the application of Bäcklund transformations to partial differential equations of the second order and to systems of Pfaffian equations (see his *Equations aux dérivées partielles du second ordre*, t. 2, cap. 9). In this connection there is also a paper by G. Cerf (*C.R.*, **172**, 1921, 518), who deals with equations of the third order in two independent variables.

R. H. Fowler and C. N. H. Lock (*Proc. Lond. Math. Soc.*, **20**, 1921, 127) led thereto by their work on spinning projectiles, complete the theory of Schlesinger and Birkhoff on asymptotic expansions of the solutions of linear differential equations, including the particular integrals.

Historically, transcendental problems have frequently arisen as limiting cases of algebraical problems, as in the case of the development of integral equations by Volterra and Fredholm and earlier by Sturm in his treatment of differential equations of the second order with boundary conditions as the limiting case of difference equations with boundary conditions, which is an abbreviated form of a restricted system of algebraic equations. R. D. Carmichael (*Amer. Journ. Math.*, **43**, 1921, 69), in a paper entitled " Boundary Values and Expansion Problems," proposes to systematise this process and begins by developing methods for algebraic systems corresponding to " variation of parameters " and to Green's Function.

T. Carleman (*C.R.*, **172**, 1921, 655) discusses a class of integral equations with an asymmetrical kernel ; R. Wavre (*ibid.*, **172**, 1921, 432), G. Julia (*ibid.*, 1279), S. Pincherle (*ibid.*, 1395), and G. Bertrand (*ibid.*, 1458) deal with Fredholm's equation.

St. Bóbr (*Math. Zs.*, **10**, 1921, 1) generalises von Koch's theorem on the absolute convergence of an infinite determinant.

If $K(s, t)$ is a symmetrical function such that $\int_a^b\int_a^b K(s,t)$ $\theta(s)\,\theta(t)\,dsdt$ is not negative, for every function θ continuous in the interval (a, b), $K(s, t)$ is said to be of " positive type." J. Mercer (*Proc. Roy. Soc.*, A. **99**, 1921, 19), examines the class of functions arising by linear transformation of functions of positive type.

S. Szidon (*Math. Zs.*, **10**, 1921, 121) answers two questions raised by W. H. Young on Fourier series ; he shows that it is not sufficient for $\Sigma\, q_n \cos nx$ to be a Fourier series that the sequence q_n should be monotonic and converge to zero as n

tends to infinity; and, secondly, that the sequences, found by Young, which compounded with an arbitrary Fourier series $\Sigma (a_n \cos nx + b_n \sin nx)$ give a Fourier series $\Sigma\lambda_n (a_n \cos nx + b_n \sin nx)$ are the only sequences with this property.

H. Steinhaus (*Proc. Lond. Math. Soc.*, **20**, 1921, 123) gives the first example of a thoroughly divergent orthogonal development, *i.e.* he shows how to define an orthogonal, normalised and complete sequence of functions $\phi (x)$, integrable (L), together with their squares, in the interval (a, b), so that a suitable integrable function $f (x)$ can be found, whose Fourier-like expansion $\sum_1^\infty \phi_i(x) \int_a^b f(t)\, \phi_i(t)\, dt$ is divergent for every value of x in (a, b).

G. N. Watson (*Proc. Lond. Math. Soc.*, **20**, 1921, 189) connects the product of two hypergeometric functions with hypergeometric functions of two variables of Appell's fourth type.

P. Humbert (*C.R.*, **173**, 1921, 217) investigates a formula of multiplication for Kummer's function $\phi (\alpha, \gamma, x)$ which is one of the confluent hypergeometric functions; he also develops (*Proc. Roy. Soc. Edin.*, **41**, 1921, 1), the theory of the confluent hypergeometric functions of two variables, which is related to the potential equation in space of four dimensions.

H. Faxen (*Arkiv för Mat.*, **15**, 1921, No. 13) obtains series for the integrals

$$\int_y^\infty e^{-x(t \pm t^{-\mu})}\, t^v dt \text{ and } \int_y^\infty e^{-x(t \pm t^{-\mu})}\, t^v \log t.\, dt,$$

which include the Hankel-Bessel functions, the integral-logarithm, and the error-function as special cases.

S. Chandra Dhar (*Tôhoku Math. Journ.*, **19**, 1921, 175) obtains series for the solutions of Mathieu's equation of the second kind in a different form from those obtained by E. L. Ince.

Geometry.—Connected with a plane cubic curve are two curves of class three, called by Cayley the Pippian and the Quippian (the former is now usually called the Cayleyan). W. P. Milne and D. G. Taylor (*Proc. Lond. Math. Soc.*, **20**, 1921, 101) show how the pencil of class-cubics given by these two curves may be defined geometrically from apolar relations with regard to the pencil of cubics through the intersections of the given cubic and its Hessian. W. P. Milne (*ibid.*, **20**, 1921, 107) also generalises properties of corresponding points on the Hessian of a plane cubic, regarding such points as degenerate conics apolar to the net of polar conics of a given cubic.

K. W. Rutgers (*Proc. Amst. Acad.*, **23**, 1921, 797) examines the number of degenerations possible in linear systems of plane cubics.

G. T. Bennett (*Proc. Lond. Math. Soc.*, **20**, 1921, 59) discusses the three-bar sextic curve.

H. Hilton (*Proc. Lond. Math. Soc.*, **20**, 1921, 93) writes on a plane curve of order n having a multiple point of order $n-1$, and a conic of $2n$–point contact; and also on some special types of quintic and sextic curves (*Rend. Palermo*, **44**, 1920, 341).

By means of the quadratic transformation F. D. Murnaghan (*Amer. Math. Monthly*, **28**, 1921, 203) investigates a certain cubic twisted cubic curve which is associated with the tetrahedron in much the same way as the Kriepert hyperbola is with the triangle.

Two tetrahedra are said to be conjugate if the faces of one are the polar planes of the vertices of the other with regard to a quadric; when this is so, four faces of one meet four faces of the other in four lines belonging to the same system of generators of a quadric, whence the German description "hyperboloide." Two tetrahedra which are conjugate in four ways are fundamental for the quartic surfaces studied by Schur (see Jessop, *Quartic Surfaces*, p. 193). A. Baruch (*Rend. Palermo*, **44**, 1920, 261) investigates such tetrahedra, showing that from the first pair two other tetrahedra may be constructed so that each of the four is conjugate to any other in four ways.

A. Emch (*Amer. Math. Monthly*, **28**, 1921, 46) gives an account of a method for the construction and modelling of algebraic surfaces by considering them as generated from two projective pencils; in particular he applies the method to a quintic surface with a nodal quartic curve and to a cubic cyclide.

An equation $\Sigma a_{ik}, x_i x'_k = O$ $(i, k = 1, 2, 3, 4)$ gives a correlation between two spaces; the classification of such correlations is, of course, a generalisation of the classification of quadrics; it is undertaken by K. Kommerell (*Math. Zs.*, **10**, 1921, 217).

The property that if two triangles are inscribed in a conic their sides touch a second conic and there is a third conic to which they are both self-polar may be generalised to a property of two tetrahedra and a twisted cubic (Hurwitz) and in fact to a property of two $(n + 1)$ *-gons* inscribed in a curve of order n in space of n dimensions. H. S. White (*Trans. Amer. Math. Soc.*, **22**, 1921, 80) gives a proof of the general theorem, depending on a special (2, 2) correspondence.

Hurwitz in 1887 showed the existence of "singular" correspondences on an algebraic curve (sufficiently specialised), which need *two* equations to express them. V. Snyder and F. R. Sharpe (*Trans. Amer. Math. Soc.*, **22**, 1921, 31) give examples of such correspondences between two curves, obtained by ruled surfaces through the curves.

C. Rosati (*Rend. Palermo*, **44**, 1920, 307) also considers

singular correspondences; he limits himself to curves of genus two and to the symmetrical case, showing that we may regard the correspondence as having two real positive valencies.

J. de Vries (*Proc. Amst. Acad.*, **23**, 1921, 462, 466) considers the involutory transformation of the lines of space which is defined from two involutions on two straight lines, in which P, P' are corresponding points on the first line, and Q, Q' corresponding points on the second, by associating the lines PQ, $P'Q'$.

A bilinear equation $\Sigma c_{rs} p_r q_s = O$ $(r, s = 1 - 6)$ between the co-ordinates of two lines is said to define a bilinear connex of pairs of lines, the conjugate lines of a given line forming in general a linear complex. The equation may also be interpreted as a reciprocity between pairs of points on a quadric variety in space of five dimensions. C. Segre (*Rend. Palermo*, **44**, 1920, 139) examines the case in which $c_{rs} + c_{sr} = O$, when the connex is said to be "alternate."

Segre (*Rend. Lincei*, **30** (1), 1921 200, 227) also has a couple of notes on the surface of Veronese, which is of the fourth order and of two dimensions in space of five dimensions, and can be represented point for point on a plane.

S. Carrus (*C.R.*, **173**, 1921, 69, 219, 437) and A. Demoulin (*Mém. de la Soc. Roy. de Liège*, **11**, 1921, 1) deal with triply-orthogonal systems of surfaces.

A rectilinear congruence for which the asymptotic lines on the two focal surfaces correspond is called, after Weingarten, a W-congruence. Papers on these congruences have been published by L. P. Eisenhart (*Annals of Math.*, **22**, 1921, 161), G. Fubini (*Rend. Lincei*, **30** (1), 1921, 273; **30** (2), 1921, 22), and P. Tortorici (*Math. Zs.*, **10**, 1921, 255).

E. P. Lane (*Amer. Journ. Math.*, **43**, 1921, 52) continues the work of Wilczynski on the general theory of congruences in connection with a completely integrable system of partial differential equations.

J. K. Whittemore (*Annals of Math.*, **22**, 1921, 217) determines minimal surfaces which contain straight lines; and A. Demoulin (*Bull. de l'Acad. Roy. de Belgique*, **7**, 1921, 293) investigates geodesics on Enneper's minimal surface.

Extensions of minimal surfaces to higher space are given by C. L. E. Moore (*Bull. Amer. Math. Soc.*, **27**, 1921, 211) and A. Palatini (*Annali di mat.*, **29**, 1921, 191).

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Chief Assistant, Royal Observatory, Greenwich.

The Age of the Earth.—At the meeting of the British Association in Edinburgh in September last a joint discussion on the age of the earth took place under the auspices of the sections for

Mathematics, Geology, Zoology and Botany. Previous references in these notes to this subject will be found in SCIENCE PROGRESS, **10**, 281, 1915–16, and **14**, 553, 1920 ("The Sources of Stellar Energy"). The inadequacy of Lord Kelvin's original estimate of twenty or thirty million years has long been admitted. His estimate was arrived at in three different ways. But the argument from the temperature gradient inside the earth's crust has been invalidated by the discovery of radio-activity. In fact, Lord Rayleigh, in the discussion at the British Association, stated that if we suppose the radium content found in the superficial layers of the earth to extend for twenty miles downwards the whole output of heat from the earth can be accounted for without assuming the existence of the primeval store of energy which was postulated by Lord Kelvin. The existing state of things cannot be used as a firm basis from which to explore the past.

As regards the sun's heat, radio-activity cannot explain its maintenance, and gravitational contraction is not sufficient. The giant red stars radiate heat at something like 1,000 times the rate at which the sun does, and the contraction theory alone is inadequate to explain their present existence. Some other source of stellar energy must be postulated. Prof. Eddington pointed out that if the radiation of δ Cephei was derived from gravitational contraction, the density must increase 1 per cent. in forty years. This star is a variable, whose variation is believed to be due to periodic pulsations. A change of density must therefore be accompanied by a change of the period of pulsation. In the case of δ Cephei the change of period is known with some accuracy to be only 0·08 seconds per annum, or 1 per cent. in 58,000 years, corresponding to a change of density of 1 per cent. in 29,000 years. It follows that the star has sources of energy other than gravitational, and that Lord Kelvin's time-scale must be increased in a ratio of about 29,000 to 40, or 700 to 1. In the case of the sun, which is a dwarf star, and farther advanced in its evolution, a smaller—though still large—factor would be required. Kelvin's argument from the heat of the sun is therefore invalid.

Lord Rayleigh considers that the most accurate estimate of the age of the earth can be derived from the rate of radio-active disintegration. Uranium passes through a series of successive stages during its disintegration which terminate in an isotope of lead, having an atomic weight less than that of "ordinary" lead, but chemically indistinguishable from it. The order and rate of this disintegration through successive stages are known with a high degree of accuracy, so that a determination of the amount of the isotope of lead present in minerals containing uranium enables the time when disintegration commenced to

be assigned without very great uncertainty. In this way an age of about 1,000 million years is derived from pre-Cambrian rocks.

Helium is another product of disintegration of uranium, and the helium content of radio-active minerals can also be utilised. In this way only a lower limit of the age of the mineral can be assigned, as some leakage of the gas is probable. Lord Rayleigh finds that, in general, ages of about one-third those given by estimates of lead are thus obtained.

Prof. Sollas pointed out the necessity of making sure that the radio-active clock was not as much too fast as Lord Kelvin's was too slow. Lord Rayleigh had assumed that, since the rate of disintegration of uranium is not affected by any change of temperature or pressure which we can apply, uranium has always disintegrated at the same rate. Prof. Sollas drew attention to the phenomenon of pleochroic haloes occurring in uranium-bearing black mica. These haloes, which have been studied by Prof. Joly, are formed by the X-rays expelled by uranium in the course of its disintegration. In general, the ranges of the rays are consistent with those found at the present time. In the case of the two inner rings, however, the ranges are greater than normal, and Prof. Joly has concluded from this that in Caledonian times there existed a metope of the uranium we now know, with different properties. If this conclusion is correct, the radium clock has not been keeping uniform time, and estimates of the age of the earth which are based upon it are liable to an error of an uncertain amount.

Prof. Gregory dealt with the geological estimate of the age of the earth, based upon the salinity of the sea. Estimates obtained in this way varied from 70 to 150 million years. He pointed out that the argument suffered from three fundamental objections. It was assumed that the sea was originally fresh, although the oldest fauna, the Cambrian, had marine characteristics, and the contrast between the fresh water and marine fauna in Palæozoic times was as sharp as it is to-day. There was also no allowance for the large supplies of sodium chloride raised from beneath the earth's surface by magmatic waters. Further, a uniform rate of denudation was postulated, whereas there have been alternating periods of quick and slow crustal movements: the earth is now under the influence of a time of quick movement with, consequently, denudation faster than the average. Taking these three causes separately, Prof. Gregory estimated that, to allow for them, the age of the earth deduced from the salinity argument should be multiplied two-fold, three- or four-fold and five-fold respectively. He concluded by stating that the best-known geological estimates of the age of the earth require to be multiplied ten- or twenty-fold

in order to agree with the physical estimates, and that this increase is consistent with the geological evidence. On the other hand, Prof. Sollas dissented from this view and was not prepared to accept such an increase.

Dr. Jeffreys stated that from considerations of the temperature distribution downwards in the earth's crust, allowing for the radio-active content, and also from the tidal theory of the origin of the solar system, he had separately derived concordant estimates of about 2,000 million years since the solidification of the earth's crust. Thus, with revised data, two of Lord Kelvin's methods of reasoning have been brought into agreement with the results derived from other physical methods. Lord Kelvin's third method—the contraction hypothesis—is not valid on account of the existence of other sources of stellar energy.

It will have been gathered that, on the whole, there is now a satisfactory agreement between the results of arguments based upon astronomical, physical and geological considerations. These arguments combine in indicating an age of the earth, since solidification, of about 1,000 million years.

The Theory of Saturn's Rings.—An important contribution to this theory has been made by Dr. G. R. Goldsborough in a paper entitled "The Influence of Satellites upon the Form of Saturn's Rings" (*Phil. Trans.*, Sec. A, 223, pp. 101–30, 1921). It was shown by Maxwell in 1856 that the rings could only be stable for small disturbances if they were composed of meteorites sufficiently small. Spectroscopic observations have confirmed this result. From this point of view, the observed divisions in the rings have been attributed to the disturbing action of the satellites of Saturn. It has been supposed that in the positions in which a single particle moving in a circular orbit would have a period commensurate with one of the satellites, the disturbing action would gradually increase by a resonance effect until in time a ring would be swept clear of particles. Lowell, at the Flagstaff Observatory, has in fact found that, in addition to the well-known and easily observable rings, there are a large number of additional fine divisions in the rings which occur at intervals corresponding to periods commensurable with that of the satellite Mimas. This theory has never received adequate theoretical investigation, in spite of the fact that some doubt was thrown upon it by Tisserand. Moreover, the theory in this form is not complete, as the mutual attractions of the particles composing the ring are left out of consideration. The paper by Goldsborough, previously referred to, supplies this lack. He considers the effect of a satellite upon a number of particles forming a single ring round the planet and subject to their mutual gravitational attraction, as well as to that of Saturn and of the satellite.

Considering first the case in which the particles in the rings are all equal, it is confirmed that, in certain places, the perturbations would increase until the particles in a particular ring would leave the ring and mingle with those of other rings, so leaving—theoretically—a "division." The effect of unequal particles is then taken into consideration, and it is found that the effect of the inequality in mass would be to make the divisions become more extended and therefore more readily visible.

If some of the particles are assumed to be very small, Cassini's division is at once obtained, produced by the satellite Mimas Theory, indicates that the ring should commence at a mean instance of 16″·9, which is in exact accordance with observation. Particles of larger mass cause an extension of the division outwards from Saturn. By making use of the observed width of this ring (from 16″·87 to 17″·64), an upper limit for the mass of the particles can be obtained. This upper limit is supposed to hold also for the remaining rings.

In general, it is found that any satellite may be expected to produce at least two zones of clearance. The first zone extends from the origin (the centre of Saturn) to a distance dependent upon the mass of the satellite in question. The second zone extends from a second and greater distance, which is also dependent upon the mass of the satellite, up to the satellite itself.

For the various satellites, the dimensions deduced for the clearance near the origin are :

Mimas	4″·59
Enceladus . . .	6″·16
Tethys	7″·30
Dione	9″·34
Rhea	13″·07
Titan	29″·94

The first three of these limits fall within Saturn itself, and therefore need not be considered. The value 9″·34 corresponds approximately with the distance of the inner edge of the Crêpe Ring, the observed value being 10″·83, whilst the value 13″·07 agrees almost exactly with the observed value (13″·00) of the distance of the inner edge of ring B.

Here the theory meets with a difficulty. Although this numerical agreement is good, the existence of the Crêpe Ring within the dissipative area of Rhea, and of all the rings (the distance of whose outermost edge is only 20″) within the dissipative area of Titan require to be explained. A somewhat closer examination of the theory indicates that the rate of dissipation of the rings by the satellites must be a very slow process ; the rate is not uniform, however, but is greatest near the outer limits of the areas under consideration and decreases uniformly

inwards. Dione, therefore, is responsible for the clearance of particles from the surface of Saturn to the commencement of the Crêpe Ring. On the other hand, Rhea is only effective near the boundary of its zone of clearance, so that the Crêpe Ring is undispersed in the weaker part of its field. The bright rings are within the weak part of Titan's field of clearance, and have not yet been dispersed. In course of time the Crêpe Ring will be dispersed by Rhea and the bright rings by Titan.

As regards the outer zone of clearance, it is found that the innermost satellite, Mimas, will cause a clearance from a distance of 20"·2 up to the satellite itself. The outer edge of ring A is at distance 20"·01, so that the theory indicates with considerable precision the termination of the whole ring. For the other satellites, the inner limits of the corresponding zone fall within the area already cleared by Mimas.

Of the other zones indicated by the theory, all fall outside the outer edge of the outermost ring, except the Cassini division, which is produced by the action of Mimas.

This discussion, therefore, gives a rational account of the existence of the Crêpe Ring and the two bright rings, separated by the Cassini division, and assigns the limits of distance from Saturn with a fair degree of accuracy. In arriving at these results, no account was taken of the oblateness of Saturn, or of the influence of one ring of particles upon another. The numerical agreement between observation and theory might be still further improved by their inclusion.

The theory does not account for the numerous fine divisions observed by Lowell in the bright rings, though their existence is not excluded by it. In Goldsborough's discussion, the disturbance of the ring of particles by a satellite was examined with a view to determining under what conditions there would be a large departure from a fixed circle. Such departure results in collisions with adjacent rings of particles and the clearance of a ring in the neighbourhood of the original circle. But the orbits in which the departure from the circular form does not become great with increase of time may yet become dynamically unstable, if further small arbitrary displacements are imposed upon them. The possibility of further divisions is therefore not excluded.

Although, therefore, the theory is in a sense not complete, it carries the discussion of the ring system a considerable stage further than previously.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

The *Quarterly Journal of the Royal Meteorological Society* for July 1921 (vol. xlvii, No. 199), contains an important paper by Dr. G. C. Simpson, F.R.S., explaining the manner in which the

heavy rain in India associated with the south-west monsoon, from June to September, is brought about.

In former times both the south-west wind and the rain were attributed to convectional currents due to the excessive heat over the inland regions of India, but several considerations make this explanation appear unsatisfactory. For instance, the temperature contrast between land and sea is greater in May, before the monsoon sets in, than during the rainy season; the average temperature is higher in years of small than in those of large rainfall; lastly, the hottest part of India during the monsoon gets practically no rainfall at all.

During July, a typical monsoon month, the winds over India circulate round an area of low pressure centred in the north-west. For convenience the air motion near the ground can be divided into ten separate streams. Now the mountain-ranges of India and the Malay Peninsula form a kind of box, having two openings only, one between Burmah and Ceylon, and the other in the north-west; it is only through these openings that air can freely reach the interior regions. Of the ten air-streams, three cross the western Ghats almost perpendicularly. The warm, damp air ascends 4,000 ft. and condensation takes place at a level of about 500 ft.; the total rainfall for the month amounts to well over fifty inches in the central parts of the range. Proceeding to Burmah and Assam, three other trajectories also encounter mountain-ranges directly, in each case producing extremely heavy rain; at Cherrapungi, in Assam, where the winds encounter an almost vertical rise of 4,000 ft., the annual rainfall averages 424 inches, the greatest amount recorded in the whole world hitherto. The seventh air-stream moves westwards across Northern India from the Bay of Bengal, entering an enclosed area, but without encountering any sudden barrier; the air rises only gradually and the rainfall is therefore moderate. The remaining three trajectories pass into India through the gap in the north-west. These are particularly interesting, as it is in the north of this area that the dry region occurs. This dry region is the tract of desert between Multan and Jacobabad, and is also the region of lowest pressure. The presence of the Himalayas and the mountains of Afghanistan and Baluchistan do not permit air to flow in except from the south and east, and owing to the moderate pressure gradient the amount which arrives is not large. The supply would, however, suffice to cause rain, were it not for two factors. First of these is the extremely high temperature prevailing over the desert, which causes the damp air flowing in from the sea to warm up and attain a comparatively low relative humidity. Secondly, as soon as the air begins to ascend it encounters a very dry upper current from the west which blows during the

monsoon, as has been proved by observations made with the aid of kites. In consequence cloud is not formed, and the uninterrupted sunshine maintains the high surface temperature already mentioned, which is in itself a factor which tends to prevent the formation of cloud and rain. There results a vicious circle tending to maintain drought.

Turning now to the question of the origin of the moisture in the south-west monsoon, maps giving the mean pressure and prevailing winds of the world in July show a large area of low pressure over Asia and a high pressure area over the southern part of the Indian Ocean between South Africa and Australia. The air that reaches India forms part of the south-east trade winds before crossing the Equator and then turns towards the north-east to form the south-west monsoon, which is a part of the general circulation of air round the Asiatic low pressure system. This south-west wind is very humid, having traversed over 4,000 miles of ocean. Now in May, before the rains have begun, there is also a considerable inflow of air into the Indian area, conditions, as judged from charts of India alone, appearing to be very similar to those in July, except that the inflow is a little weaker and the air that arrives from the ocean is somewhat drier. Pressure and wind-charts for the world show that the south-east trades do not now cross the Equator, and that northerly winds prevail over the Arabian Sea. The air has not followed the very long course over the ocean that it takes in July, and is consequently relatively dry. The explanation given for the drought over the Indian desert during the rainy season now applies for the country as a whole, and there is simultaneously great heat and a general absence of rain. It is noteworthy that, when the rains fail, conditions generally resemble those of May, except that in a failure of the rains the supply of air to India is small because of a tendency for high pressure to develop over Western India and not, as in May, because of the relatively feeble development of the high pressure system over the southern part of the Indian Ocean.

To sum up, the primary cause of the south-west monsoon is the high temperature over the land in the Northern Hemisphere as a whole, which tends to lower the pressure there and cause a south-west wind over India; the connection between temperature, pressure, and rainfall is, however, a complicated one when considered in detail; for instance, over India abnormally high temperature is associated with a feeble development of the south-west monsoon wind and a rainfall below the average.

Het Vliegveld, of February 26, 1921, contains a paper on "Fog Banks," by A. C. Nell, in which observations of fog made by Dr. Georgii, while in command of an air-squadron in Flanders during the war, are summarised.

Fog is always preceded by mist. Banks of mist are met with by aviators, which banks are not visible from the ground, and in them the air is not saturated. These occur even in strong winds, and are always associated with an inversion of temperature. There are two important kinds of ground-fog. In one there is a rise of temperature immediately above the ground; this kind occurs both in strong and light winds in the summer, and has no definite upper boundary, passing into mist and then clear air. The second kind occurs in anticyclones in winter; here there is a definite upper boundary, and the temperature falls with height until a warm stratum is encountered at the top. These fogs generally extend over a wide area and are thickest in the centre of the area of high pressure, forming an umbrella-shaped surface co-extensive with the high pressure. When the latter becomes larger the area of fog also expands.

PHYSICS. By J. Rice, M.A., University of Liverpool.

Contact Potentials, Photoelectric, and Thermionic Emission and Electrochemical Action.—The existence of contact potentials between different metals of the order of a volt is a matter which has given rise to a great deal of controversy during the past century. The whole question has been raised again during the past few years by the researches of Richardson, Millikan, Langmuir, and their co-workers on thermionic and photo-electric effects, the most recent contribution being a paper of Millikan's to the September number of the *Physical Review.* As is well known, contact potentials were first observed by Volta, who found that the metals could be arranged in a series so that each was positive with respect to those preceding it, but that of course in a complete circuit of metals the P.D.s would balance and there would be no E.M.F. In a circuit containing an electrolyte Volta considered that the effect of the liquid was to equalise the potentials of the metal surfaces in contact with it, and so to permit the contact potential at the junction of the metals to function as an E.M.F. However, a few years later, Ritter observing that chemical change always accompanied the action of the voltaic pile, drew the conclusion that the chemical change should be regarded as the cause of the electrical phenomena. The dispute was rather an indefinite one at the time, since clear conceptions of the energy relations involved had not as yet made their appearance in scientific literature. However, each point of view was ably defended by its champions; contact theorists such as Kohlrausch, Hankel, Lord Kelvin, Ayrton, Perry, and Clifton obtained in various ways evidence for the existence of potential differences established between metals or between a metal and a liquid by contact and then separation, and succeeded in measuring several such quantities and de-

monstrating that their algebraic sum round a chain of them in a cell was equal to the E.M.F. of the cell. The physical chemists, however, whose point of view has been put most completely by Ostwald and Nernst, but also had powerful adherents long ago such as le Roux, Edlund, and Maxwell, maintain that these contact potentials as measured are quite illusory, and are really differences between the potential in the air near one metal and that in the air near the other metal, the films of air close to the plates adhering to them when they are separated and causing the plates to behave as if a real difference of potential existed between them. To this group of workers the only real *contact* difference of potential between two metals is represented by the Peltier effect, whose order of magnitude is very much smaller than the "Volta" P.D.s, and according to them the E.M.F. of a cell is built up of electrode potentials between metal and electrolyte which owe their origin entirely to chemical action in the cell. In defence of their position they point out that when current passes round a circuit of different metals there is no absorption or evolution of heat at a junction at all consistent with a real Volta effect, but only with the very much smaller and admitted Peltier effect; and further, that the application of the second law of thermodynamics to their hypothesis undoubtedly yields certain relations between the E.M.F. of the cell, the heat of reaction, and the equilibrium constant of the chemical reaction involved which have been verified by experiment. Possibly the most notable of the contact theorists was Lord Kelvin, who, together with Helmholtz, maintained that the Peltier effect does not correspond directly to the contact potential, but is related to the temperature coefficient of the contact potential, thus:

$$\frac{P}{T} = \frac{dV_c}{dT},$$

where P is the Peltier coefficient and V_c is the contact P.D. A very complete and lucid account of all this earlier work will be found in the *Report to the British Association on the Seat of the E.M.F. in the Voltaic cell*, prepared by Sir Oliver Lodge in 1884, and until recently it would seem that the contact theory had "retired beaten," and the electrochemists held the field.

However, as mentioned above, the appearance of thermionic and photo-electric emission has changed things very considerably, and, further, Nernst's "solution pressures" do not appear to be so much in favour with physical chemists as of old. The main features of this new work are to be found in Richardson's papers, dating from 1902 onwards, on thermionic emission (which are practically summarised in his books on the "Electron

Theory of Matter" and "Emission of Ions from Hot Bodies"); in papers by Langmuir and his co-workers which are summarised and dealt with from the new standpoint in a comprehensive manner in the *Trans. Amer. Electrochemical Soc.*, vol. **xxix** (1916), pp. 125–82; and in two contributions by Millikan, one to vol. vii of the *Physical Review* (1916), pp. 18–32, and the other to last September's issue of the same journal.

What Richardson succeeded in establishing against considerable opposition and scepticism was that the emission of electrons from a heated metal is an intrinsic property of the metal and is not caused by secondary chemical reactions, nor is it dependent on or influenced by an electrostatic field around the metal. He showed that if a positive field is maintained around a metal body by bringing near to it an electrode charged to such a positive potential that no electrons will return to the metal surface, then the *saturation* current thus produced across the surface is equal to $aT^{\frac{1}{2}}e^{-b/T}$, where a and b are constants. This equation can be explained by reference to kinetic theory, assuming that the conduction electrons in a metal are ordinarily held within the metal by an electric force at the surface, just as the molecules of a liquid are prevented from escaping by the cohesive force which gives rise to surface tension. There will thus be an amount of work to be done on an electron before it can be freed from the influence of the surface and can escape under the small accelerating field to the surrounding anode. Let this work be w; then kinetic theory shows that the number of electrons which escape per sec. per sq. cm. of the metal surface is $N(kT/2\pi m)^{\frac{1}{2}}e^{-w/kT}$, where N is the number of conduction electrons in each c.c. of the metal and k is the gas-constant per molecule (*i.e.* the Boyle-Charles constant R for a given amount of gas divided by the number of molecules in this amount), whose value is about $1 \cdot 35 \times 10^{-16}$. The connection between this formula and the previous one is obvious, and gives the interpretation of the constants a and b. Many experiments were carried out by other workers which seemed to show that this thermionic emission was due to chemical action; but the recent work of Langmuir under such extreme conditions of vacuum that chemical action between the metal and any residual gas was entirely inadequate to explain the effects observed, have succeeded in demonstrating that the emission is an intrinsic property of the metal, and that w has characteristic values for each metal. If ϵ is the electronic charge, w/ϵ (or ϕ as it is denoted by Richardson) is also characteristic, and is a measure of a sort of potential difference between the inside and outside of the metal; but, in view of what will be mentioned presently, P.D. is hardly a correct term to apply to it.

Turning now to the photo-electric effect, we have the famous equation of Einstein deduced by an application of the quantum hypothesis which will hardly bear very strict investigation. Yet, although its theoretical foundations are extremely weak, the work of A. Ll. Hughes and of Millikan and his pupils have established its experimental validity to an extremely high order of accuracy. The equation relates to the ***maximum*** velocity with which an electron can escape from a body when the surface is illuminated with monochromatic light of frequency ν. If v is this maximum velocity, then

$$\tfrac{1}{2} mv^2 = h\nu - p,$$

where h is Planck's constant, so that $h\nu$ is the energy quantum for light of this frequency and the kinetic energy with which the electron escapes is equal to the balance of energy left out of this $h\nu$ after work against surface forces has been done, the *least* possible amount of work being p. Richardson, in the *Phil. Mag.*, **23**, p. 615, and **24**, p. 570 (1912), considerably strengthened the theoretical basis of this equation, and also identified Einstein's p quantity with the w quantity of his own thermionic result given above.

Admitting, therefore, the existence of this surface-work term *as an intrinsic property of the body*, let us proceed to show its relation to contact potentials. Consider two metallic bodies placed in a vacuous enclosure maintained at constant temperature T. Let w and w^1 refer to the work functions corresponding to the escape of electrons through the surfaces of each body respectively. Let V and V^1 be the potentials *in the space just outside* the surfaces of the metals. The work required to remove an electron from the first body, transport it across the space between and carry it into the second metal is $w + (V - V^1)\,\epsilon - w^1$. [N.B., the electronic charge is negative, but ϵ is regarded here as its mere magnitude, *i.e.* as a positive number, so that $(\mathrm{V} - \mathrm{V}^1)\,\epsilon$ is work done *on* the electron in travelling from the place where the potential is V to the place where it is V^1.] Now, by a well-known result in electronic theory, this work must also be equal to $k\mathrm{T} \log n/n^1$, where n and n^1 are concentrations of conduction electrons in each metal. Hence

$$\begin{aligned} \mathrm{V} - \mathrm{V}^1 &= (w^1 - w + k\mathrm{T} \log n/n^1)/\epsilon \qquad (1) \\ &= \phi^1 - \phi + k\mathrm{T}/\epsilon \cdot \log n/n^1 \end{aligned}$$

Suppose now, that the two metals are placed in contact, then from the values of electric conductivity and thermo-electric measurements we are justified in assuming that n and n^1 become, under these circumstances, numbers of the same order of magnitude, and the term $k\mathrm{T}/\epsilon \cdot \log n/n^1$ is found to lead to comparatively small E.M.F.s of the order of magnitude

of those required to account for the Peltier effect. Hence, we may practically write (1) as

$$V - V^1 = \phi^1 - \phi \qquad (2)$$

if the metals *have been put in contact*, whether they remain so or are separated. Hence, the contact P.D. of the two metals is thus identified as the difference of their ϕ or w/ϵ quantities. Suppose, now, we denote this contact P.D. by K and illuminate the first of these metals with light of frequency ν, and apply to the other the greatest opposing potential, the "photo-potential," which just allows photo-electric current to flow. Calling this P, we cannot write $P\epsilon = h\nu - w$, but

$$(P + K)\epsilon = h\nu - w \qquad (3).$$
$$\text{or} \quad P + K = h/_\epsilon \cdot \nu - \phi$$

This equation has been very carefully tested in the work of Millikan. It is found to be in excellent agreement with fact. In particular, it has yielded in his hands one of the most precise determinations of Planck's constants. Another feature which has been tested is the connection which it gives between ϕ and the "threshold" value of the frequency, *i.e.* the *least* value of ν which just gives any photo-electric effect at all; the relation is

$$h\nu_0 = w = \phi\epsilon$$

ν_0 is determinable by observation, and ϕ can be calculated from the application of (3) to measurements of photo-potentials and contact P.D.s.

Langmuir, in the paper referred to, has pointed out that it is not correct to regard ϕ as a P.D. in the usual sense of the term. We define a P.D. as existing between two points, when the work done on an element of charge in transportation is V q and the transportation of the δq *does not appreciably affect the value of V*. But the mechanism of photo-electric and thermionic emission seems to be such that the work involved by the removal of a small but *finite* amount of charge ϵ is due to the back attraction on the ϵ of its mirror image, *i.e.* its induced charge on the surface of the metal, or the balance of opposite charge left unneutralised by its removal. It can be shown easily, on the law of inverse squares, that the amount involved in removing ϵ from a point distant x from the surface to infinity is $\epsilon^2/4x$, and that if we assume the inverse square law to hold down to a distance x_0, and for nearer distances adopt any plausible hypothesis as to attractive force which does not lead to infinite singularities, then $w = \epsilon^2/2x_0$ approximately; *i.e.*

$$\phi = \epsilon/2x_0$$

If this equation is tested from the known values of ϕ and ϵ, it yields quite reasonable values for x_0, all of the order 10^{-8} cm.,

as one would expect from the values of atom dimensions and distances apart calculated on other grounds. It is interesting to note also that it gives an explanation of a remark of Richardson's that the values of ϕ for some elements—such as sodium, potassium, and carbon—are inversely proportional to the *cube roots* of their atomic volumes.

In the latest paper, *Phys. Review*, September last, Millikan carries the matter to a further stage, drawing a distinction between what he calls "intrinsic" and "spurious" contact E.M.F.s. He first of all splits the w term in the photo-electric equation into two parts—w_1 required to detach an electron from its parent atom, and w_2 to remove it through the surface of the metal; for it has been found that insulators exhibit the photo-electric effect as well as conductors. Hence, using V_m and $V_m{}^1$ for the stopping potentials of each of the metals faced successively by a heavily oxidised surface of copper to which this stopping potential is applied, and K and K^1 for the contact E.M.F.s between each metal and the oxidised surface, we have from (3)

$$\epsilon(V_m + K) = h\nu - (w_1 + w_2)$$
$$\epsilon(V_m{}^1 + K^1) = h\nu - (w_1{}^1 + w_2{}^1)$$

$$\therefore \epsilon\,(V_m - V_m{}^1 + K - K^1) = (w_1{}^1 - w_1) + (w_2{}^1 - w_2). \quad (5)$$

But in this analysis w_2 and $w_2{}^1$ are clearly the analogues of w and w^1 in Richardson's analysis, *i.e.* the pure surface work terms. Hence $(w_2{}^1 - w_2)/\epsilon$ is the contact E.M.F. between the metals and so is equal to $K - K^1$. Hence by (5)

$$\epsilon\,(V_m - V_m{}^1) = w_1{}^1 - w_1 \quad (6).$$

Now Millikan has found that the stopping potentials for sodium, potassium, and lithium surfaces freshly cut and *tested at once* facing the oxidised copper surface are accurately the same; *i.e.* $V_m = V_m{}^1$ (if we generalise this result) and so by (6)

$$w_1{}^1 - w_1 = 0.$$

This is a most important result in connection with the Quantum hypothesis. It involves one of two alternatives:

(1) Either $w_1 = w_1{}^1$, *i.e.* the work required to detach an electron from an atom is the same for all atoms, which is violently at variance with all our knowledge on this matter, or—

(2) In the above analysis $h\nu - w_1$ has to be replaced by $h\nu$, *i.e.* the photo-electron which escapes with the maximum velocity from a metal under the influence of light with a frequency ν has exactly the energy $h\nu$, when it arrives at the *inner* surface of the metal on its way out. This implies that the incident light energy is not absorbed in whole quanta at all, but that absorption continues within the atom, until the electron

is able to *escape* from the atom with the energy $h\nu$, the absorption being actually $h\nu$, plus the energy required to pull the electron out of the atom ; or it implies that the incident light is able to impart energy $h\nu$ to a *free* electron in the metal.

The point is not settled at the moment, but Millikan inclines to the last alternative.

PHYSICAL CHEMISTRY. By W. E. GARNER, M.Sc., University College, London.

The Ionic Theory.—The theory of ionisation propounded by Arrhenius in 1888 has withstood the criticism of the last thirty years, and has survived the extensive experimental investigation carried out during this period. In spite of the incompleteness of its application to moderately concentrated solutions of strong electrolytes, no simple modification has yet been devised which replaces the original theory in every-day use. The conductivity of aqueous solutions can be satisfactorily explained on no other assumption than that of the dissociation of the electrolyte into ions. Uncertainty arises, however, as to the value of the conductivity ratio $\frac{\Lambda v}{\Lambda \infty}$ for the determination of the degree of ionisation. In fact as G. N. Lewis points out, it does not seem possible to ascribe any definite meaning to the degree of dissociation. Since the law of mass action does not apply to solutions of strong electrolytes, the existence of molecules in the chemical sense is doubtful.

The changes in the equivalent conductivity Λ of a salt in aqueous solution may be ascribed to variations either in the mobility or in the number of the ions. Arrhenius assumed that the mobility of the ions was independent of the concentration, but grave doubt is thrown on the accuracy of this assumption by Lewis, MacInnes, and others. That relatively large changes in the mobility of the ions do occur is shown in the measurements of transport numbers of ions. MacInnes (*J.A.C.S.*, 1921, **43**, 1217) points out that the anionic transport number for hydrochloric acid varies from 0·167 to 0·156 between 0·01 and 1·0N, and that of lithium chloride from 0·668 to 0·694 for the same range. The transport number for potassium chloride is, however, independent of the concentration. It seems probable that the mobilities of the ions of this salt vary with concentration at the same rate, rather than that their mobilities are constant. In any case, it appears that a change in the mobility of the hydrogen ion of approximately 7 per cent. occurs between 0·01 and 1·0N.

So far as conductivity and the mobility of the ions are concerned, a simplication of the theory of dissociation would be obtained if the assumption were made that strong electrolytes

are completely ionised at all concentrations. MacInnes computes the changes in the mobility of the ions which would be required to explain the variations in conductivity on this assumption. He finds that between 0·01N and 1·0N, the change in conductivity of the chlorine ion, $T_{Cl}A$ is from 61·6 to 47, and that the value of this product is independent of the nature of the univalent cation present. The agreement is especially good if the viscosity of the solution be taken into account. The conductivity of an aqueous solution is thus given by the sum of two conductivities, one due to the anion, and one due to the cation, *i.e.* the Kohlrausch law is extended to solutions of electrolytes up to 1·0N.

Much more attention is now being paid to the alternative view that solutions of electrolytes are completely ionised. The theoretical work of Sutherland, Bjerrum, Milner, and Ghosh in this field has shown that conductivity can be adequately explained on this assumption. A paper published by Ghosh (*Zeit. Phys. Chem.*, 1921, **98**, 211) gives a summary of his views on ionisation. In this theory the undissociated molecules of the Arrhenius theory are replaced by a group of ions held together by electrostatic forces, those ions becoming active which possess a sufficiently high kinetic energy to overcome the electric field. The ratio of the active to the inactive ions is given by the relation $a = e^{-\frac{A}{nRT}}$ where A the work required to separate the electrical group is equal to $\frac{NE^2}{Dr}$ where r is the distance between the ions, when these are arranged in a cubic lattice, D the dielectric constant, and E the electrical charge. The degree of ionisation a calculated from this formula is accurate between 0·01 and 0·0002 N. The author does not apply his equation to concentrated solutions.

The thermodynamic treatment of dilute solutions possesses a much wider range of application than any theory based on kinetic grounds. The thermodynamic degree of dissociation has the same value, from whatever physical property it is derived. In this respect it possesses a considerable advantage over the degree of dissociation calculated from conductivity data. G. N. Lewis and Merle Randall (*J.A.C.S.*, 1921, **43**, 1150) have published a summary of several chapters on this question in their new book on *Chemical Thermodynamics*. They point out that the "true" degree of dissociation cannot be logically defined, since the fixing of the limiting distance within which two ions are associated and beyond which they are dissociated—is quite arbitrary. On this account it would be expected that the degree of dissociation would be dependent on the experimental methods which were employed in its

determination. Even if the mobility of the ions were independent of the concentration the ratio calculated from the conductivity results would have only a limited value. It is otherwise with the activity coefficient or the thermodynamic degree of dissociation. This is taken as $\gamma = \frac{a\pm}{m}$ for a binary electrolyte like KCl where $a\pm$ is the mean activity of the two ions and m is the number of gram mols in one litre. This ratio, of which a large number of values are given in the paper, may be used to displace the degree of dissociation in the older approximate formulæ.

Lewis shows that the activity coefficients, whether calculated from the electromotive force, the vapour pressures of the solute or of the solvent, distribution ratios, or freezing-point data, possess the same value. Remarkable agreement is obtained over a wide range of concentration. The results with sulphuric acid are especially striking; the activity coefficient of sulphuric acid (0–60 per cent.) is the same, whether calculated from freezing-points, electromotive force or vapour pressure data. Where the vapour pressure of the solvent is employed it is necessary to derive the activity of the solute a_2 from the activity of the solvent a_1. Lewis and Randall (*J.A.C.S.*, 1921, **43**, 233) have shown that

$$\int d \log a_2 = -\int \frac{N_1}{N_2} d \log a_1,$$

where N_1 and N_2 denote the mol fractions of the solute and solvent and $N_1 + N_2 = 1$. For concentrated solutions this furnishes a fairly satisfactory method of determining the ratios of the activities of the solute between two concentrations.

The authors have developed the methods of determination of the thermodynamic degree of dissociation from freezing-point data. The vant' Hoff factor i is a quantity which has been used in the interpretation of dilute solutions of electrolytes. The relation $(i - 1)$ is not a measure of the thermodynamic degree of dissociation, nor does it give any valuable information regarding the thermodynamic properties of the solute. The factor i merely gives the ratio of the lowering of the activity of the solvent, to the lowering which would be produced by a normal undissociated substance. The activity of the solute can be obtained from the relationship $d \log a_2 = \frac{d\delta}{\lambda m}$ where δ is the freezing-point lowering at the molality m and λ is the molecular lowering of any undissociated solute at infinite dilution.

In the same paper Lewis has made a noteworthy generalisa-

tion with respect to mixtures of electrolytes in solution. In discussing this question the authors introduce a new term, viz. the ionic strength μ = sum of the stoichometrical molality of each ion $\times$ the square of its valency and the whole divided by two. The new general principle is that "in dilute solution the thermodynamic degree of dissociation of a given strong electrolyte is the same in all solutions of the same ionic strength," and is independent of the particular character of the other strong electrolytes present. Very satisfactory calculations of the solubility of salts in solutions of mixed electrolytes can be made on these assumptions.

Lewis points out that, while the results are in agreement with the theory of complete dissociation, no definite proof of this theory is yet forthcoming.

ORGANIC CHEMISTRY. By O. L. BRADY, D.Sc., F.I.C., University College, London.

Synthesis of Formaldehyde and Carbohydrates from Carbon Dioxide and Water.—An important contribution towards the elucidation of the problem of the synthesis of carbohydrates by plants has been made by Baly, Heilbron and Barker (*Trans. Chem. Soc.*, 1921, **119**, 1025), who have shown that Moore and Webster's failure to detect formaldehyde in a saturated solution of carbon dioxide in water exposed in quartz vessels to sunlight, or to the light of a quartz-mercury lamp, was due to photochemical polymerisation of the formaldehyde, as produced, to reducing sugars. They find that if a stream of carbon dioxide is passed through the liquid, some of the formaldehyde produced escapes polymerisation by being removed to the back of the vessel, where it is protected from the light, and in these circumstances it can be detected at the end of the experiment.

It was found that the rays which brought about polymerisation were of longer wave length (λ = 290 $\mu\mu$) than those required for the synthesis of formaldehyde (λ = 200 $\mu\mu$), and that the addition of a compound which (1) does not interfere in a chemical sense, (2) selectively absorbs the specific rays which bring about polymerisation, and (3) is transparent to the very short wave-lengths, resulted in good yields of formaldehyde being produced by the action of ultra-violet light on a solution of carbon dioxide in water. Paraldehyde and sodium phenoxide both fulfil these conditions, as also do the metallic salts, such as ferric chloride or uranyl nitrate, which Moore and Webster describe as catalysts for the synthesis of formaldehyde, but which are apparently protective agents for this substance.

A further difficulty, however, arises in that plants grow and synthesise carbohydrates under glass which completely

absorbs light of shorter wave length than 350$\mu\mu$, the vital differing, therefore, from the laboratory process, by making use of light of longer wave length. The authors have found that this difference can be overcome by the use of a "photo-catalyst" and have been able to synthesise sugars from carbon dioxide and water in two stages under the influence of light such as is used by the living plant. In the case of the synthesis of formaldehyde, a suitable photo-catalyst has been found in a coloured basic compound, such as malachite green or p-nitroso dimethylaniline; an aqueous solution of either of these compounds saturated with carbon dioxide gives formaldehyde on exposure to light behind a thick plate-glass screen, but no formaldehyde is produced under similar conditions in the absence of carbon dioxide. These results are in contradiction to those of Osterhout (*Amer. J. Bot.*, 1918, 5, 511), who found that moist filter paper, dyed with aniline green or methyl green, when exposed to sunlight under glass, gave rise to formaldehyde, but that the presence or absence of carbon dioxide was without influence, but that oxygen was essential; but the contradiction is probably more apparent than real, as the experimental conditions were very different. The photo-catalysis of the synthesis of reducing sugars is brought about by a complex copper ion. An aqueous solution containing 17 per cent. of sodium citrate and 9 per cent. of sodium carbonate, on exposure to ultra-violet light, gives formaldehyde, and enough sugar to cause marked reduction of a 1 per cent. solution of copper sulphate when added, but no sugar is formed if the solution is exposed under a screen of thick plate-glass. If, however, the copper sulphate is added to the solution first, marked reduction is obtained after exposure under plate glass.

Moore and Webster's discovery, that many substances of bio-chemical origin give formaldehyde when exposed in aqueous solution to ultra-violet light, is apt to convey the impression that carbohydrates cannot be built up from this compound by the plant; but such compounds as acetone under these conditions give rise, not only to formaldehyde, but also to reducing sugars formed in each case through carbon dioxide, the primary product of the action of light.

It seems that the effect of ultra-violet light is to set up an equilibrium between sugar, carbon dioxide, and formaldehyde lying far over to the side of carbon dioxide; the introduction, however, of a photo-catalyst which carries the reaction from carbon dioxide and water to sugar in visible light will shift the equilibrium right over, as the system will be screened from those rays which decompose the carbohydrate.

The authors have not yet found a photo-catalyst capable of acting in both reactions, the synthesis and polymerisation of

formaldehyde. Chlorophyll is apparently such a substance; but they have not, as yet, reported any experiments with this compound, as they wished in the first instance to investigate the simplest case. They point out that the use of chlorophyll as a photo-catalyst introduces the possibility of synthesis of optically active sugars, and the results of their investigations of the action of this compound should be of the greatest interest.

The mechanism of the action of the photo-catalyst is explained on the theory recently propounded by Baly and Barker (*Trans. Chem. Soc.*, 1921, **119**, 653), which is dealt with in Recent Advances in Physical Chemistry in the October number of this Review. This explanation seems to be supported by the work of Ursprung (*Ber. Deut. Bot. Ges.*, 1918, **36**, 6) on the relation between absorption in the infra-red and visible spectrum and starch formation in the green leaf.

A Cyclic Iodine Compound.—A paper by Collie and Reilly (*Trans. Chem. Soc.*, 1921, **119**, 1550) describes a new type of organic compound containing iodine, obtained from the barium salt of diacetylacetone. When this compound is treated in alcoholic suspension with iodine a new barium salt is obtained which, when treated with dilute acids, gives a compound of the formula $C_7H_7O_2I$, which possesses rather unusual properties. Two atoms of iodine react for each atom of barium, only one half, however, appearing as barium iodide. The differential action of the iodine on the barium suggests that the barium salt of diacetylacetone must contain at least two barium atoms in the molecule, and that these must be attached in a different manner. The ordinary formula for diacetylacetone, $CH_3 \cdot CO \cdot CH_2 \cdot CO \cdot CH_2 \cdot CO \cdot CH_3$, does not provide for the formation of such a barium salt, and it is suggested that this compound has a cyclic structure.

$$HO \cdot O \begin{cases} \diagup C(CH_3) \cdot CH \diagdown \\ \diagdown C(CH_3) : CH \diagup \end{cases} C \cdot OH$$

The barium salt of such a compound would contain the metal attached in two ways, namely, replacing the hydrogen atom of a hydroxyl group attached in one case to oxygen, and in the other to carbon.

Even on such an assumption it is difficult to explain the formation of the compound $C_7H_7O_2I$ from the barium salt of diacetylacetone. The action of iodine is not analogous to the case of the sodium salt of acetyl acetone which gives tetra-acetylethane,

$$2\,CH_3 \cdot CO \cdot CH\,Na \cdot CO \cdot CH_3 \rightarrow \begin{array}{c} CH_3 \cdot CO \cdot CH \cdot CO \cdot CH_3 \\ | \\ CH_3 \cdot CO \cdot CH \cdot CO \cdot CH_3, \end{array}$$

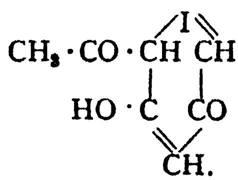
but oxidation takes place, and the yield of the new compound and its properties suggest that it is not a simple derivative of either diacetylacetone or dimethylpyrone.

The compound $C_7H_7O_3I$ is a strong acid, liberating carbon dioxide from carbonates, and forming metallic salts ; the iodine is readily removed by warm dilute silver nitrate and nitric acid, but boiling with concentrated sodium hydroxide solution does not bring about decomposition. Chlorine water does not liberate iodine, but treatment in the cold with a solution of bleaching powder gives a precipitate of calcium carbonate ; this reaction is quantitative, three carbon atoms being converted to carbonate. It is a monobasic acid of molecular weight, approximately 263, forms a mono-acetyl derivative, and reacts with two molecules of phenylhydrazine ; it is stable to boiling water, but heated with water to 130–140° it decomposes quantitatively into hydriodic acid, acetic acid, carbon, and water

$$C_7H_7O_3I \rightarrow HI + CH_3 \cdot CO_2H + 5C + H_2O,$$

certainly a remarkable reaction.

The authors consider that the only simple formula which agrees with these properties is :



On salt formation the elements of water are added, the iodine becoming quinquivalent.

The authors draw attention to the fact that a compound containing iodine in a ring with four carbon atoms has been described previously, and found to be a base ; in the above example the compound contains an iodine atom in a ring with five carbon atoms, and is an acid. This is the converse of the behaviour of nitrogen in pyrrole and pyridine.

MINERALOGY AND CRYSTALLOGRAPHY. By A. Scott, D.Sc.

The development of crystals with curved faces is fairly common, and numerous explanations of the phenomenon have been advanced. In an interesting paper on the ternary alloys of tin, antimony and arsenic, J. E. Stead (*Journ. Inst. Metals*, **22**, 127, 1919) describes some compounds which show this development of curvature in a remarkable degree. From melts con-

taining 99·5 per cent. tin and 0·5 per cent. arsenic, crystals of the intermetallic compound Sn_3As_2 separate in a pure condition in a rhombohedral form with axial ratio $a : c = 1 : 1{\cdot}2583$. From ternary melts, provided the ratio of antimony to arsenic is greater than unity, the crystals are obtained in the form of incomplete spherical shells, with diameter up to one centimetre, the best development of the curved habit being found in melts whose composition lies within the limits 70–85 per cent. tin, 25–18 per cent. antimony, and 4–5 per cent. arsenic. The matrix in which the crystals occur consists of the antimony-tin eutectic, but the arsenic-tin compound shows the same habit when the above eutectic is replaced by the eutectics of tin and lead or antimony and lead. The crystallographic details in the paper are due to L. J. Spencer, who also summarises the literature on curvature of crystals and its probable causes.

The commonest type of curved crystal is probably the microlites of hornblende, augite and so forth, which characterise many vitreous rocks. In this instance the minerals have developed in a solution of high viscosity, and the habit is due to the prevention of diffusion, so that the crystals grow in ever-changing directions, probably as the result of twinning. Crystals with capillary habit—the so-called hair crystals—possibly originate in a similar way. To this type belong the hair crystals of silver and moss-gold (*cf.* A. Beutell, *Cent. Min.*, 14, 1919), and possibly also the form of certain organic compounds which show curvature when the crystals are very thin, but not when they are thicker (*cf.* O. Lehmann, *Molekularphysik*, Leipsig, **1**, 374, 1888). In other cases the curvature is due to the formation of an aggregate of minute prismatic crystals which deviate slightly from parallel positions. The formation of vicinal faces is probably responsible for the habit of certain crystals of gypsum, diamond and so forth. The curvature of quartz, beryl and tourmaline probably arises through deformation along glide planes, while the twisted crystals of quartz and dolomite are supposed to be heterogeneous aggregates of small crystals. G. T. Prior (*Min. Mag.*, **14**, 26, 1904) has described a spherical habit in crystals of a complex sulphide of lead, tin, antimony and iron. According to C. A. F. Benedicks (*Journ. Inst. Metals*, **22**, 145, 1919) curvature is fairly common in metals. For example, antimony often occurs in curved form, while graphite in grey pig-iron develops in spherical shells if the carbon crystallises out at a late stage in the cooling when the metal is in the plastic condition, the curvature being even more pronounced in the case of temper carbon which forms when the iron is in a solid condition (*cf.* K. Honda and S. Saitô, *Sci. Rep. Tokoku Univ.*, **9**, 311, 1920). According to Benedicks, where a solid solution is crystallising, not only do

the layers differ in composition, but the temperature may not be uniform, and hence there must be a differential contraction due to variations in the coefficient of dilatation of the various layers. Thus the conditions likely to give rise to curved crystals are high vectoriality of the crystals, the formation of lamellæ of varying composition, and variable temperature, giving rise to unequal contraction of the lamellæ. An analogous explanation may be applied to the case of the curved crystals of augite described by the present writer (*Min. Mag.*, **17**, 100, 1914), where the mineral shows a fine lamellar structure approximately "parallel" to the curved faces. The tendency of certain organic compounds to develop crystals with curved faces when grown in solutions containing various dyes which are absorbed by the growing crystals has been noticed by P. Gaubert (*Bull. Soc. fr. Min.*, **28**, 286, 1905; *Compt. Rend.*, **151**, 1134, 1910; **157**, 1531, 1913, *Recherches récentes sur le facies des cristaux*, Paris, 1911), and it is again possible that the phenomenon may be explained by Benedick's hypothesis.

An elaborate study of the figures which appear on the surface of hydrated crystals during the process of efflorescence has been made by C. Gaudefroy (*Bull. Soc. fr. Min.*, **42**, 284, 1919.) It is found that while many of the dehydration figures obtained bear some relation to the structure of the original crystal, others are connected with the product of dehydration while some are more complex and difficult to explain. On the basis of the exterior form the figures may be divided into three groups:

(*a*) *Polygonal figures* determined by the structure of the original crystal. The salt is partly dissolved in its own water of crystallisation, and then, as the latter evaporates, the polyhedron which is formed intersects the surface of the original crystal in a polygon whose form is determined by certain structural planes in the original crystal. The formation of a liquid phase during the process of dehydration is probably general, since any hydrated mineral, if sufficiently fine grained, will cohere to a "solid" mass on standing in a desiccator.

(*b*) *Polygonal figures* determined by the secondary product, that is, by the structure of the salt formed by the dehydration. In this case each figure represents a single crystal of the product of dehydration; for example, on the surface of the rhombic heptahydrated zinc sulphate monoclinic crystals of the hexahydrate develop. There is no essential coincidence between the orientation of the two crystals, and various forms may occur in the same original crystal.

(*c*) *Sectoral Figures.*—In the most usual form of this type, the figure is divided into four sections resembling an hour glass structure. In general, each section is composed of an aggregate

of parallel submicroscopic crystals; diametrically opposite sections have usually similar optical orientation.

(*d*) *Elliptical Figures.*—These figures are composed of an aggregate of minute particles arranged in a heterogeneous fashion, and generally distinguished from the other types by their deep-seated development. Examination under high magnification shows the figure to have an undulating form which in the limiting case approximates to an ellipse.

Of these types, the first is the most common, and is shown by some of the monoclinic sulphates; the second is instanced by zinc sulphate; the third by gypsum, and the ferrocyanides, and the last by zinc sulphate.

The sectored figures obtained have already undergone several investigations, notably by Sohncke (*Zeit. Kryst. Min.*, **30**, 1, 1899), and Weiss (*Zeit. Deutsch. Geol. Ges.*, **29**, 211, 1877), and are known as the "Weiss'sche Brennfigur."

The former investigated the effect of temperature on the form of the figures, and found that the higher the temperature of formation, the more tendency do they show to orientate themselves parallel to the vertical axis of the gypsum crystal; the average deviation from parallelism amounted to approximately 3°. This has been verified by R. Grengg (*Zeit. anorg. chem.*, **90**, 327, 1914; *Tscher. Min. Mitt.* = **33**, 201, 1915; *Zeit. Kryst.*, **55**, 1, 1915), who has made an elaborate investigation of the subject. He endeavours to show that, in some cases, a very concentrated solution of calcium sulphate forms and this afterwards consolidates to form an orientated group of crystals of the hemihydrate $CaSO_4,\frac{1}{2}H_2O$. When the gypsum is dehydrated on the oil bath, this aggregate consists of fine acicular crystals whose optical properties indicate the above-named salt. When the gypsum plate is in contact with concentrated sulphuric acid, the composition of the needles is not so certain, but it is probable that in this instance it is anhydrite which forms. When the dehydration occurs at temperatures just under 100° C., rounded or elliptical figures are obtained. These have the greater axis inclined to the *c*-axis of the crystal at a somewhat larger angle than in the case described above, while their composition is variable, anhydrite being common, though the hemihydrate may occasionally occur.

The etch and solution figures of alum crystals have been examined by H. Bauhans (*Beitr. Kryst. Min.*, **1**, 11, 1918); and the same volume contains papers on similar phenomena in fluorspar by H. Bauhans and V. Goldschmidt (*ibid.*, **1**, 219, 1918) and in quartz, by the action of sodium borate solutions under pressure, by G. Lincio (*ibid.*, **1**, 87, 1918). The analogies between such solution and erosion have been discussed by V. Goldschmidt (*ibid.*, **1**, 183, 1918; *Bull. Imp. Acad. Petersburg*, 339, 1914).

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., University College, London.

Cryptogams.—In a concluding paper (*Trans. Roy. Soc.*, Edinburgh, No. 32), on the "Old Red Sandstone Plants from the Rhynie Chert," Kidston and Lang give restorations of *Rhynia Gwynne-Vaughani*, *R. major*, *Hornea lignieri*, and *Asteroxylon mackiei*, and discuss their bearing on general problems connected with the *Pteridophyta*. *Asteroxylon*, the most complex, it is pointed out, presented an organisation comparable with that of *Psilophyton* and encountered amongst present-day plants in the *Psilotaceæ*, where too there is a relatively simple type of construction associated with an absence of roots. A certain measure of resemblance is also recognised to the Zygopterideæ amongst the ferns. The other three species show still further simplification in the absence of leaves. A subterranean rhizomatous region which bore absorbent hairs here gave rise to a leafless and dichotomously branched cylindrical axis whose finer branches bore terminal sporangia; the simplicity here exhibited renders comparison with the sporophyte of the Bryophyta easier than in the case of other Pteridophyta, and attention is called to the possibility that *Sporogonites* may have been the fructification of a Pteridophyte. The suggestion also made that these plants bring the Pteridophyta nearer to the Algæ seems to be a mere restatement that they are the most simply organised Pteridophyta known, and appears to presuppose a relationship which may in fact not exist.

In connection with the foregoing it is interesting to note that Holloway, in a further study of the prothallus and sporophyte of *Tmesipteris*, emphasises the resemblance to the *Rhyniaceæ*. Not only, as already shown, is the embryo of this plant much simpler than that of other Pteridophytes in the fact that there is no differentiation apart from shoot and foot, but the sporophyte in its early stages of development closely resembles the prothallus. It is at first a cylindrical structure exhibiting dichotomous branching and entirely destitute of leaves. Thus the ontogeny of *Tmesipteris* includes a phase comparable with the adult condition of the Rhyniaceæ, which may perhaps represent a phylogenetic recapitulation. There is, however, the possibility that in both groups the extreme simplicity of construction is an outcome of a common saprophytic mode of nutrition.

Morphology and Anatomy.—Discussing in detail the leaf of the Iridaceæ (*Ann. Bot.*, July), Mrs. Arber points out that both in the ontogeny of the bud and the seedling there is nothing to support the view advocated by Celakovsky and Velenovsky that the bifacial leaf of *Iris* has arisen by concrescence. It is shown that the leaves of various Iridaceæ show considerable resemblance in anatomical structure to the phyllodes of *Acacia*.

Thus *Iris Douglasiana* can be compared with *A. pendula*; *Sparaxis* with *A. neurophylla; Gladiolus ornatus* with *A. incurva* and *Sisyrinchium junceum* with *A. teretifolia*.

Phormium tenax and *Dianella*, in which the ontogeny supports the view of congenital concrescence, are considered as cases apart from the normal Monocotyledonous leaf.

As in *Acacia*, so in the Iridaceæ, a winged axis is not infrequent, the wings corresponding to the plane of flattening of the phyllode.

The leaves of the Irises of the *Juno* section are held to consist of leaf-base only, whilst evidence is adduced to support the view that the dorsiventral limb of *Crocus* is a secondary development of the phyllodic petiole.

The same author has also considered the leaves of the *Helobieæ* (*Bot. Gaz.*, July), which show three types, viz. a sheathing base with a more or less radial petiole; a sheathing base and a ribbon-like lamina (= flattened petiole); a sheathing base and a differentiated pseudo-lamina. The different types recur as parallel developments in five of the seven families of the Cohort.

A number of variations in the flowers of *Stachys sylvatica* are described by Cutting (*Ann. Bot.*, July), of which the peloric and semi-peloric flowers were found to occupy positions favourable to nutrition, whilst flowers with a decreased number of parts were observed under conditions unfavourable to nutrition. Several instances of a complete andrœcium of five stamens were noted and both self- and bud-pollination was found to occur.

The fact that vigorous plants of the normally decussate *Helianthus tuberosus* are not infrequently whorled, with three leaves at each node, is a familiar fact to most. Similarly shoots of *Ligustrum vulgare* show the same phenomena, and it is also met with in *Lysimachia vulgaris, Anagallis arvensis*, etc. McAtee (*Torrey Bot. Club*, May) records such whorled leaves for several species of *Viburnum*, viz. *V. opulus, V. acerifolium, V. dentatum, V. pubescens, V. lentago*, and *V. lantana*. Also in five species of *Lonicera* and in *Sambucus canadensis*. In all these the phenomenon is chiefly associated with the more vigorous root shoots and would therefore appear to be largely conditioned by nutrition.

Comparison of normal and tetracotyledonous seedlings of *Phaseolus* show some interesting physiological distinctions to be associated with the morphological one. The normal seedlings have not only a higher green weight and dry weight, but the percentage dry weight is higher also, evidently indicating greater vigour (Harris, *Bot. Gaz.*, September).

The artificial inoculation of species of *Citrus* which are normally resistant to *Pseudomonas citri* has shown that they can

be infected and thus indicates some structural feature as the cause of their natural immunity. T. McLean (*Bull. Torrey Bot. Club*, April) has examined the stomata in a highly resistant and a very susceptible species and finds that in the former the pore is narrower than in the susceptible *Citrus*. By preventing access of water to the ante-chamber this may well account for the difference in susceptibility to disease.

Miss Rea (*New Phyt.*, June) finds that the stomatal frequency in *Campanula rotundifolia* increases with increased height of the leaf above the ground, thus agreeing with Yapp's observation on *Ulmaria palustris*. The number of hydathodes present on the upper surface of the leaves was greatest in the shade shoots, where as many as 99 were found on a single leaf. In the shade plant the hydathodes were more numerous on the upper than the lower leaves, but in normal illumination and fully insolated plants the uppermost leaves bore an apical group only.

Ecology.—Dealing with the ecology of *Urtica dioica*, Olsen (*Journ. Ecology*, September) finds that 10–20 per cent. of open sunlight is the most favourable condition of illumination, but that intensities as low as 5 per cent. are tolerated. Whilst favoured by moisture this species cannot endure water-logging of the soil in winter. In common with its most frequent associates *Mercurialis perennis*, *Stachys sylvatica* and *Chrysosplenium alternifolium*, *Urtica dioica* is rich in nitrates, showing that these are present as such in the soils on which it occurs. The most vigorous plants were found where nitrification was most active, and this occurred in the damper soils, but showed no correspondence with the acidity. The amount of phosphoric acid also appears to be an important factor.

In the same journal Salisbury and Tansley give an account of the *Quercus sessiliflora* woods of the Malvern area, some of which occur on soils derived from highly calcareous strata. It is shown that this is not inconsistent with the normal occurrence of this tree in Britain, since the surface layers in which the seedling is rooted at the critical period are almost completely leached, and, even on the soils derived from the Wenlock Limestone, may exhibit an appreciable acidity. The ground-flora is intermediate in character between that of a typical *Quercetum sessiliflorae* and that of the Oakwoods of calcareous soils. The woods on the May Hill Sandstone, the Malvernian, the Aymestry, and the Wenlock soils show a progressive increase of basicity accompanied by an increase of species normally confined to non-acid soils. In the more acid woods these are confined to the flushes, but even on the Wenlock Limestone the marked leaching of the flat wood surface results in the partial restriction of these species to the regions of maximum erosion (*e.g.* path sides, etc.).

H. H. Thomas gives some interesting notes on the Libyan desert. Only two ephemeral species were at all frequent even during the "wet" season (total rainfall 5–91 mm.) of the first four months of the year. These were a bulbous plant *Dipcadi erythræum* (Liliaceæ) and an ephemeral member of the Geraniaceæ, *Monsonia nivea*. Perennial species were rare, the chief being *Convolvulus lanatus* and *Calligonum comosum*. The latter, which is a Polygonaceous plant, responds to accumulation of sand by further growth, like *Suæda fruticosa* when buried by shingle.

Salisbury in the *Naturalist* (October) deals with the vegetation of drying mud, which shows three phases, generally marked by the presence, respectively, of *Vaucheria sessilis*, *Botrydium granulatum*, and phanerogamic seedlings. The chief feature of the vegetation is the not infrequent occurrence of rare species in great abundance. Noteworthy examples cited are *Limosella aquatica*, *Alopecurus fulvus*, *Rumex limosus* and *Elatine hydropiper*, all of which have been found in quantity occupying localities where they were apparently very rare or from which they were altogether absent for possibly many years prior to their development *en masse*. It is suggested that we here have to deal with instances of delayed germination over a period of years comparable with that demonstrated for *Alisma plantago* by Crocker.

Taxonomy.—An important contribution appears in the *Journal of the Linnean Soc.* for September, describing the plants collected by Prof. Compton in New Caledonia. These embrace new species belonging to the following genera: *Dendrobium*, *Bulbophyllum*, *Phreatea*, *Phajus*, *Calanthe*, *Sarcochilus*, *Acianthus*, *Capynema*, *Freycinetia*, *Eriocaulon*, *Costularia*, *Scleria*, *Hibbertia*, *Drymis*, *Ionidium*, *Agation*, *Pittosporum*, *Garcinia*, *Microsemum*, *Sterculia* *Elæocarpus*, *Ryssopteris*, *Boronella*, *Melicope*, *Evodia* *Zanthoxylon*, *Dutaillyea*, *Murraya*, *Dinoxylum*, *Sphenostemon*, *Euroschinus*, *Semecarpus*, *Arthroclianthus* *Phaseolus*, *Mucuna*, *Storkiella*, *Albizzia*, *Parinarium*. *Geissois*, *Codia*, *Pancheria*, *Spiræanthemum*, *Polyosma* *Terminalia*, *Clœzia*, *Myrtus*, *Eugenia*, *Casearia*, *Homalium*, *Schefflera*, *Dizygotheca*, *Eremopanax*, *Apiopetalum*, *Bikkia*, *Lucinæa*, *Randia*, *Gardenia*, *Atractocarpus*, *Cyclophyllum*, *Ixora*, *Morinda*, *Psychotria*, *Cephaëlis*, *Lagenophora*, *Erigeron*, *Blumea*, *Scævola* *Dracophyllum*, *Rapanea*, *Chrysophyllum*, *Planchonella*, *Palaquium*, *Symplocos*, *Melodium*, *Ranwolfia*, *Alyxia*, *Pterochrosia*, *Alstonia*, *Parsonsia*, *Tylophora*, *Marsdenia*, *Hoya*, *Geniostoma*, *Lindernia*, *Pseuderanthemum*, *Justicia*, *Myoporum*, *Gmelina*, *Oxera*, *Timeroya*, *Atriplex*, *Nepenthes*, *Piper*, *Hedycarya*, *Trimenia*, *Litsea*, *Beauprea*, *Grevillea*, *Stenocarpus*, *Loranthus*, *Exocarpus*, *Ricinocarpus*, *Phyllanthus*, *Acalypha*, *Cleidion*,

Macaranga, Balanops, Ficus. Several new genera are described. Mr. Baber creates *Comptonella* (Rutaceæ), *Salaciopsis* (Celastrineæ), *Montagneia* (Anacardiaceæ), *Paracryphia* (Eucryphiaceæ), and *Enochoria* (Araliaceæ). Spencer Moore creates *Tropalanthe* (Sapotaceæ), *Merismostigma* (Rubiaceæ), *Dopanthus* (Gesneraceæ), *Adenodaphne* (Lauraceæ), and *Dendrophyllanthus* (Euphorbiaceæ).

In the *Journal of Botany* for August H. W. Pugsley deals with the British forms of *Jasione montana*. Fawcett and Rendle describe a new Jamaican species of *Triumfetta* and Spencer Moore new species of *Pelargonium, Rhus, Combretum, Dactylopetalum, Tribulocarpus, Oldenlandia, Felicia, Euryops, Senecio, Schizoglossum*, and *Selago*. In the September number Col. Godfrey describes a new European *Serapias*, Miss Lister a new *Arcyria*, and Spencer Moore a new genus of Olacaceæ, viz. *Phanerocalyx*, and several new species of other genera.

PLANT PHYSIOLOGY. By CYRIL WEST, B.A., D.Sc., Botany School, Cambridge (Plant Physiology Committee).

Growth of the Higher Plants.—It will be seen, from the following brief review of recent work on the growth of the higher plants, that the last few years have been characterised by the advance made in our knowledge of this fundamental problem of plant physiology.

At the outset attention should be directed to the researches of W. L. Balls (*Phil. Trans. Roy. Soc.*, B, 1912–17), on the growth of the Cotton Plant in Egypt. This notable work, which extended over a number of years and is an experimental analysis of the various conditions controlling the growth of a single crop has, however, already been referred to in these notes (see SCIENCE PROGRESS, No. 47, 1918, pp. 412–13).

Suzuki (" On the Growth of the Rice Plant," *Bull.* No. 124 *of the Agric. Exper. Sta., Govt. of Formosa*, December 1917) has published the results of a comprehensive study of the growth of the rice plant, in which he not only records the rate of increase in dry-weight of the entire plant and of its various parts at successive stages of growth, but also gives the protein, dextrose, glucose, saccharose, starch, nitrogen, potash, soda, lime, magnesia, phosphoric acid, and ferric oxide content. An investigation of a somewhat similar character has been carried out by J. S. Burd (" Rate of Absorption of Soil Constituents at Successive Stages of Plant Growth," *Journ. Agric. Res.*, xviii, No. 2, 1919, p. 51). The chemical composition of barley grown on two different types of soil was studied, and the total dry-weight of the plant recorded at successive stages

in its growth. Attention was drawn to the remarkable losses of potash and nitrogen from the plant at an early stage in development.

Boysen-Jensen (" Studies in the Production of Matter in Light- and Shadow-plants," *Bot. Tidsskr.*, xxxvi, Heft 4, 1918, p. 219) has compared the percentage production of dry matter in a typical sun-plant (*Sinapis alba*) with that in a typical shade-loving plant (*Oxalis acetosella*). The growth of the plant has been calculated as the difference between income and expenditure, the gain in dry-weight being the result of photosynthesis and salt-uptake, the loss in dry-weight being due to the respiration of the various parts of the plant and to leaf-fall, etc.

Brenchley (" On the Relations between Growth and the Environmental Conditions of Temperature and Bright Sunshine," *Ann. Appl. Biol.*, vi, 1920, p. 211) has investigated the relation of temperature and sunshine to the rate of growth of garden peas grown in water-cultures. The rate of growth has been expressed per unit dry-weight. It was found that during the early seedling stage the rate of growth was associated with relatively warm days and nights, bright sunshine having little significant effect. During the later period the growth-rate was associated strongly with sunshine and warm days, but not significantly with night temperatures.

On the basis of measurements of the height of sunflower plants made at weekly intervals, Reed and Holland (" The Growth-rate of an Annual Plant Helianthus," *Proc. Nat. Acad. Sci.*, v, 1920, p. 135) have concluded that the growth-rate of these plants approximates closely the course of an autocatalytic reaction as expressed by the formula

$$dx/dx = Kx\,(a-x),$$

in which a = the initial quantity of material subject to transformation, x = the amount transformed at time t, and k = a constant. They have suggested that the close agreement of the actual mean height of the plants with that required by the equation of autocatalysis may indicate that the growth-rate is governed by constant internal factors rather than by external factors. As the result of a quantitative study based upon length measurements of seventy selected shoots of apricot-trees, Reed (" The Dynamics of a Fluctuating Growth-rate," *Proc. Nat. Acad. Sci.*, vi, 1920, p. 397) showed that the growth of the shoots followed a definite, though fluctuating, rate. The maximum rate was exhibited soon after the season's growth began, and fell off with some regularity to the end of the season; but three distinct intra-seasonal cycles of growth were apparent. The growth in each cycle closely resembled the rate

of an autocatalytic reaction: the growth-rate for the entire season conformed to that of a reaction consisting of two monomolecular reactions, one of which at first accelerates and subsequently retards the other. The same author ("The Nature of the Growth-rate," *Journ. Gen. Physiol.*, ii, 1920, p. 545) measured the rate of increase in height of walnut-trees. The young trees showed distinct growth-cycles in a single season, but in each cycle growth proceeded at a rate corresponding to that of an autocatalytic reaction. In two subsequent papers Reed ("Slow and Rapid Growth," *Amer. Journ. of Bot.*, vii, 1920, p. 327; also "Correlation and Growth in the Branches of Young Pear-trees," *Journ. Agric. Res.*, xxi, No. 11, 1921, p. 849) emphasises the relationship between the rate of growth and the final size of the plant organ—length of shoots of apricot and pear-trees respectively. The amount of growth "yet to be made" appears to be an exponential function of the final size. The rate may be affected by (1) a variation in the activity of the catalyst, or by (2) a variation in the supply of potential growth material.

The suggestion that the rate of growth of a plant organism is similar to an autocatalytic reaction has received the support of Rippel ("Die Wachstumskurve der Pflanzen und ihre mathematische Behandlung durch Robertson und Mitscherlich," *Fühling's Landw. Ztg.*, lxviii, Heft 11–12, 1919, p. 201; also "Die Wachstumskurve," *Ber. d. deutsch. bot. Gesellsch.*, xxxvii, Heft 3, 1919, p. 169).

It has been suggested by V. H. Blackman ("The Compound Interest Law and Plant-growth," *Ann. Bot.*, xxxiii, 1919, p. 357) that the dry-weight of an annual plant, at least in its early stages, increases at continuous compound interest. This can be expressed mathematically by the formula $W_1 = W_0e^{rt}$, where W_1 = the dry-weight of the plant at the end of time t, W_0 = the initial weight of the seed or seedling, e = the base of the natural logarithms, and r = the rate of interest. The value r was termed by the author the "efficiency index" of the plant as a producer of new material, and was regarded as an important physiological constant, but it has since been shown by Kidd, West, and Briggs ("What is the Significance of the Efficiency Index of Plant Growth?" *New Phyt.*, xix, 1920, p. 88) that r is not constant at all, and consequently can be of no value in comparing the efficiency of different plants, except over strictly comparable times and phases of development. L. R. Waldron ("Rate of Culm Formation in *Bromus inermis*," *Journ. Agric. Res.*, xxi, No. 11, 1921, p. 803), who studied the rate of culm-formation in *Bromus inermis*, showed that the rate of increase is not fixed, but takes place at an accelerating rate. The Compound Interest Law has no real

significance when applied to the process of culm-formation in this grass.

Mitscherlich (" Das Gesetz der Pflanzenwachstums," *Landw. Jahrb.*, liii, Heft 2, 1919, p. 167; " Ein Beitrag zum Gesetz der Pflanzenwachstums," *Fühling's Landw. Ztg.*, lxviii, Heft 7–8, 1919, p. 130; and " Zum Gesetz der Pflanzenwachstums,"—*Fühling's Landw. Ztg.*, lxviii, Heft 21–22, 1919, p. 419) has attempted to apply to plant-growth, as measured by dry-weight increase, the following formula:

$$\log(\sqrt[n]{A} - \sqrt[n]{y}) = \log \sqrt[n]{A} - c.x$$

where n = a variable quantity indicating the probable number of environmental factors, A = the maximum possible dry-weight attainable by the plant in question, y = the dry-weight of the plant at time x, the time x being expressed in vegetation periods of arbitrary length.

These various formulæ to express the growth-rate of plants are briefly considered and critically examined by O. Schüepp (" Ueber Form und Darstellung der Wachstumskurven," *Ber. d. deutsch. bot. Gesellsch.*, xxxviii, Heft 5, 1920, p. 193.

Suggestions for the procedure to be adopted in a quantitative study of the growth-rate of plants are put forward by West, Briggs, and Kidd (" Methods and Significant Relations in the Quantitative Analysis of Plant-growths," *New Phyt.*, xix, 1920, p. 200). In this paper reference is made to previous attempts to evaluate constants for plant-growth and certain significant relations for a quantitative analysis of plant-growth are given. In the first of a series of papers in which an attempt is made to analyse the results of elaborate experiments carried out in Germany on the growth of maize about fifty years ago by Kreusler and his co-workers, Briggs, Kidd, and West (" A Quantitative Analysis of Plant-growth. I," *Ann. Appl. Biol.*, vii, 1920, p. 103) have utilised two of the significant relations referred to in the preceding paper; namely, the Relative Growth-rate curve, which is the weekly percentage increase in dry-weight plotted against time, and the Leaf-area Ratio curve, which is the leaf-area in $cm.^2$ per gm. plotted against time. It is shown that the growth-rate of maize varies greatly in magnitude at different periods in its life-cycle in a perfectly definite manner, and that the curve for leaf-area per unit dry-weight throughout the season exhibits a correspondence with the growth-rate curve, thus indicating that the physiological basis for increased and decreased relative rate of growth is a corresponding change in the assimilating area per unit dry-weight. In the second paper of this series (*Ann. Appl. Biol.*, vii, 1920, p. 202) the authors have expressed the

rate of growth per unit leaf-area instead of per unit dry-weight, and have made use of a third significant relation, Unit Leaf-rate, which they define as the weekly rate of increase of dry-weight in mgs. per $cm.^2$ It is found that the unit leaf-rate does not undergo a perfectly definite type of variation as does the relative growth-rate, but fluctuates about a mean value. The general conclusion which emerges from a study of the correlation between unit leaf-rate and various environmental factors is that the unit leaf-rate is correlated more closely with temperature than with any of the other environmental factors, the correlation coefficient with weekly mean temperature for the year 1875 being $r = \cdot 77$.

The Employment of the Growth-rates of Standard Plants as Indices for the Comparison of Different Climates.—Since 1916, when B. E. Livingston and F. T. McLean ("A Living Climatological Instrument," *Science, N.S.*, xliii, 1916, p. 362) proposed a method of employing the growth-rates of standard plants as indices for the comparison of different climates as these influence plant-growth in general, several papers have appeared which give the results of studies of plant-growth to which this method has been applied successfully.

Hildebrandt ("Leaf-product as an Index of Growth in Soy-bean," *Johns Hopkins Univ. Circ.*, March 1917, p. 202) has pointed out that the dry-weight and leaf-area of soy-beans four weeks old can be determined approximately from their leaflet measurements, and that this plant should be a very suitable one for use as a standard plant to indicate "climatic effectiveness" in the manner suggested by Livingston and McLean, since its growth-rate can be approximately determined from easily obtained leaf measurements.

In an attempt to test the above method for determining some of the quantitative relations between climatological conditions and the growth of plants, McLean ("A Preliminary Study of Climatological Conditions in Maryland, as related to Plant-growth," *Physiol. Res.*, ii, 1917, p. 129) investigated the growth of soy-beans during the first two weeks and during the first month from sowing at Oakland (in the mountains of Western Maryland) and at Easton (on the eastern shore of Chesapeake Bay) respectively. The following growth-measurements were made after about two weeks' growth—stem height, average number of leaves per plant, average length and width of mature leaves, and the "leaf-product," which is obtained by multiplying length by width for each leaf. After about four weeks' growth these measurements were repeated, whilst, in addition, the average leaf-area and the average dry-weight of tops per plant were determined. It was found that the rates of growth in terms of leaf-surface and in terms of dry-weight

varied in a similar manner with the same kind of variation in external conditions, whereas the growth-rates measured in terms of stem-elongation varied in another way with the same external differences. The seasonal marches of the growth-rates for Oakland were found to be markedly different from those for Easton. It appeared that temperature was clearly the limiting condition for growth during the first two weeks. During the second two weeks of growth, however, with exactly the same environmental conditions, the moisture relation (*i.e.* rainfall-evaporation ratio) appeared, in many cases, to have been the limiting condition for growth, this being especially true when the temperature was high. It thus appears that if two plants in different stages or phases of their development are exposed to the same fluctuations in environmental conditions, the limiting factor for one plant during a succeeding period may be of an entirely different nature from that for the other. The author points out that this result must be due to a difference between the " internal " conditions of the plants at different stages in development.

A rather more comprehensive contribution to the study of this problem has been made by F. M. Hildebrandt (" A Physiological Study of the Climatic Conditions of Maryland, as Measured by Plant-growth," *Physiol. Res.*, ii, 1921, p. 341), who has presented the results obtained from a study of a series of observations on the climatic complexes for nine different stations in Maryland for the summer of 1914, as the effectiveness of each complex was automatically integrated by soy-bean plants grown for a period of four weeks from sowing, new seeds being sown every two weeks. Measurements of stem-height, leaf-area, " leaf-product," and dry-weight were taken, the environmental factors studied being the air temperature, the evaporating power of the air, sunlight intensity and duration of sunlight. Indices for total seasonal climatic efficiency derived by multiplying the seasonal average growth-rate per day by the normal length (in days) of the growing season for the station in question, were found to have the following values for the several stations investigated, namely, Oakland, 9,009 ; Chewsville, 12,480 ; College, 16,867 ; Easton, 17,688 ; Princess Anne, 19,005 ; Coleman, 21,115 ; Darlington, 23,688 ; Baltimore, 25,422.

Adopting a similar procedure with seedlings of *Fagopyrum esculentum*, Moench., which were grown for a series of four-week exposure periods over a total time period of thirteen months, Earl S. Johnston (" Seasonal Variations in the Growth-rates of Buckwheat Plants under Greenhouse Conditions," *Johns Hopkins Univ. Circ.*, 1917, p. 211 ; also " Climatic Conditions in a Greenhouse as Measured by Plant-growth," *Monthly*

Weather Review, U.S. Dept. Agric., xlviii, 1920, p. 215), has investigated the climatic conditions obtaining in a greenhouse. These are expressed as rates of certain definite plant processes, such as dry-weight production, leaf-area increase, rate of stem-elongation, etc., the values thus obtained being expressed as averages per week for each of the four-week periods.

Amongst other recent papers dealing with plant-growth the following may be mentioned:

E. Reinau, " Die Horizonte der Wachstumsfaktoren als gestaltende Ursache für die Wuchsformen der Pflanzen über und unter der Erde "; *Angew. Bot.*, ii, 1920, p. 193; W. W. Garner and H. A. Allard, " Effect of the Relative Length of Day and Night and other Factors of the Environment on Growth and Reproduction in Plants," *Journ. Agric. Res.*, xviii, 1920, p. 553; F. G. Gregory, " Studies in the Energy Relations of Plants, I. The Increase in Area of Leaves and Leaf Surface in *Cucumis sativus*," *Ann. Bot.*, xxxv, 1921, p. 93; F. E. Lloyd, " Growth in *Eriogonum nudum* in Relation to Environmental Factors," *Trans. Roy. Can. Inst.*, xiii, 1921, p. 211.

ZOOLOGY. By Prof. CHAS. H. O'DONOGHUE, D.Sc., F.Z.S., Manitoba University, Winnipeg, Canada.

Protozoa.—The papers include:

Bhatia, " Notes on Fresh-water Ciliate Protozoa of India " (*Trans. Roy. Micro. Soc.*, pt. 3, 1920); Hegner, " Measurements of *Trypanosoma diemyctyli* from Different Hosts and their Relation to Specific Identification, Heredity and Environment " (*Journ. Parasit.*, vol. vii, March 1921); Kudo, " Microsporidia Parasitic in Copepods " (*ibid.*, March 1921); and " Studies on Microsporidia, with Special Reference to those Parasitic in Mosquitoes " (*Journ. Morph.*, vol. 35, No. 1, March 1921); and Schrader, " A Microsporidian occurring in the Smelt " (*Journ. Paras.*, vol. vii, March 1921.)

Invertebrata.—Stephenson has published a paper on " Contributions to the Morphology, Classification and Zoogeography of Indian Oligochæta " (*Proc. Zool. Soc.*, pt. 1, March 1921) which is subdivided as follows: I. The Affinities and Systematic Position of the Genus *Eudi chogaster* Mchlsn., and some Related Questions; II. On Polyphyly in the Oligochæta; and III. Some General Considerations on the Geographical Distribution of the Indian Oligochæta. The sub-titles indicate clearly the scope of the paper. The author concludes that certain genera of the Oligochætes are polyphyletic, and suggests that, while phylogeny should of course form the basis of a natural classification, until the former has been satisfactorily determined it becomes almost impossible to avoid setting up polyphyletic genera. In the last part the author goes into the question of land bridges that have been postulated at various times, i.e. an Indo-Australasian, an Indo-New-Zealand, and an Indo-African, and so far as the earthworms are concerned he is inclined to think that they are not necessary to explain the distribution.

Other papers include :

Dickey, " A New Amphibian Cestode " (*Journ. Parasit.*, vol. vii, March 1921); Schwarz, " A Microsporidian occurring in the Smelt " (*ibid.*, March 1921) ; Starr, " Effects of Variations in Oxygen Tension on the Toxity of Sodium Chloride " (*Biol. Bull.*, vol. v, pt. 40, Nos. 9, 3, March 1921) ; and Ward, " A New Blood Fluke from Turtles " (*Journ. Parasit.*, vol. vii, March 1921).

" Studies of the Development and Larval Forms of Echinoderms " (*Gad Copenhagen*, 1921) is the title of a paper by Mortesen. This volume of 261 pages and 33 plates constitutes a fairly full treatment of the larval echinoderms investigated when fresh as the result of two years' travelling and collecting in various parts of the world, particularly the Pacific region. While not belittling the work that has been done on the hybridisation of echinoderms, the author calls attention to the fact that with a few exceptions it has not been of an entirely satisfactory nature, partly because of our fragmentary knowledge of these forms and partly because until recently the hybrids have never been reared far enough. The present work has gone a long way towards providing a satisfactory book of reference of normal forms, the knowledge of which should undoubtedly precede work on abnormal ones, and it will no doubt form the basis of future work along these lines.

In addition to a well-illustrated and detailed description of about 80 species and their metamorphosis there is also an introduction, a discussion of their classification, morphology, phylogeny, biology, and geographical distribution.

Other papers include :

Lillie, " Studies of Fertilisation. VIII. On the Measure of Specificity in Fertilisation between two Associated Species of the Sea-urchin, Genus *Strongylocentrotus*; and IX. On the Question of Superposition of Fertilisation on Parthenogenesis in *Strongylocentrotus purpuratus* " (both in *Biol. Bull.*, vol. 40, No. 1, Jan. 1921) ; and Newman, " On the Development of the Spontaneously Parthenogenetic Eggs of *Asterina* (*Patiria*) *miniata*," and " On the Occurrence of Paired Madreporic Pores and Pore Canals in the Advanced Bipennaria Larvæ of *Asterina* (*Patiria*) *miniata*; together with the Discussion of the Significance of Similar Structures in other Echinoderm Larvæ " (both in *Biol. Bull.*, vol. 40, No. 2, Feb. 1921).

O'Donoghue has provided a list of the " Nudibranchiate Mollusca from the Vancouver Island Region " (*Trans. Roy. Canad. Inst.*, vol. xiii, No. 1, 1921). This contains an account of the diagnostic characters, structure of the radula and the distribution of 26 species, 8 of which are new. It is based on the examination and collection of a large series of specimens and helps to fill in the gap in our knowledge of the Nudibranchs of the Pacific coast of North America since the previous collecting had been done in Alaska or in California.

Other papers include:

Stebbing, "Some Crustacea of Natal" (*Ann. Durban Mus.*, vol. iii, Jan. 1921); Johansen, "The Larger Freshwater Crustacea from Canada and Alaska" (*Canad. Field Natur.*, vol. xxxv, Feb. 1921); Copeman, "Note on the Capture (in London) of a Rare Parasitic Fly, *Hammomyia* (*Hylephila*) *unilineata* Zett." (*Proc. Zool. Soc.*, pt. 1, March 1921); Hess, "Tracheation of the Light-organs of some Common Lampyridæ" (*Anat. Rec.*, vol. 20, No. 2, Jan. 1921); Ito, "On the Metamorphosis of the Malpighian Tubes of *Bombyx mori* L." (*Journ. Morph.*, vol. 35, No. 1, March 12, 1921); Metz, and Nonidez, "Spermatogenesis in the Fly *Asilus sericeus*, Say." (*Journ. Exper. Zool.*, vol. 32, No. 1, Jan. 1921); and Plough, "Further Studies on the Effect of Temperature on Crossing Over" (*ibid.*, No. 2, Feb. 1921); Burge and Burge, "An Explanation for the Variation in the Intensity of Oxidation in the Life Cycle" (*ibid.*, No. 2, Feb. 1921); Johannsen, "The First Instar of *Wohlfahrtia vigil*, Walker" (*Journ. Parasit.*, vol. vii, March 1921); Bodine, "Factors Influencing the Water Content and the Rate of Metabolism of Certain Orthoptera" (*Journ. Exper. Zool.*, vol. 32, No. 1, Jan. 1921).

Vertebrata.—Herrick has dealt fully with the question of "The Origin of the Cerebral Hemispheres" (*Journ. Comp. Neur.*, vol. 32, No. 4, Feb. 1921). The telencephalon is probably the front part of the ancestral vertebrata whose walls are not complicated by thickening. This, in certain low forms, has become thickened and specialised in two places; the first, as a thickened outgrowth in the region of the lamina terminalis, forms the olfactory lobe, and the second is thickened without evagination. Only the evaginated portions are to be looked on as the hemispheres, and in all vertebrates these two subdivisions of the telencephalon can be made out. Certain generalised fishes have thin-walled hemispheres, a form which may have been adapted to oxygenate the brain. With the transition to the Amphibia and its consequent oxygenated blood supply it was possible for the evaginated hemispheres to evolve progressively.

Other papers include:

Hyman, "The Metabolic Gradients of Vertebrate Embryos. Teleost Embryos" (*Biol. Bull.*, vol. 40, No. 1, Jan. 1921); Okkelberg, "The Early History of the Germ-cells in the Brook-lamprey, *Entosphenus wilderi* (Gage), up to and including the Period of Sex Differentiation" (*Journ. Morph.*, vol. 35, No. 1, March 1921); Regan, "Three New Fishes from South Africa, collected by Mr. H. W. Bell Marley" (*Ann. Durban Mus.*, vol. iii, Jan. 1921); and Stockard, "Developmental Rate and Structural Expression: An Experimental Study of Twins, 'Double Monsters,' and Single Deformities, and the Interaction among Embryonic Organs during their Origin and Development" (*Amer. Journ. Anat.*, vol. 28, No. 2, Jan. 1921).

Baitsell, "A Study of the Development of Connective Tissue in the Amphibia" (*Amer. Journ. Anat.*, vol. 28, No. 3, March 1921); Boulenger, "Experiments on Colour-changes of the Spotted Salamander (*Salamandra maculosa*) conducted in the Society's Gardens" (*Proc. Zool. Soc.*, pt. 1, March 1921); Harrison, "On Relations of Symmetry in Transplanted Limbs" (*Journ. Exp. Zool.*, vol. 32, No. 1, Jan. 1921); Jaesch, "Beobachtungen über das Auskriechen der Larven von *Rana arvalis* und *fusca* und die Function des Stirndrusenstreifens" (*Anat. Anz.*, bd. 53, March 1921);

Steiner, "Hand und Fuss der Amphibixen, ein Beitrag zur Extremitätenfrage" (*ibid.*, Feb. 1921); Swingle, "Germ-cells of Anurans. I. The Male Sexual Cycle of *Rana catesbeiana*" (*Journ. Exper. Zool.*, vol. 32, No. 2, Feb. 1921); and Uhlenhuth, "Observations on the Distribution and Habits of the Blind Texan Cave Salamander, *Typhlomolge rathbuni*" (*Biol. Bull.*, vol. 40, No. 2, Feb. 1921).

Watson discusses at some length "The Bases of Classification of the Theriodontia" (*Proc. Zool. Soc.*, pt. 1, March 1921). The use of the term Theriodontia was revived by the author in 1914 to include the Therocephalia, the Gorgonopsida, the Bauridæ, and the Cynodontia. Additions have been made to our knowledge of this group since that day, and these are utilised to reopen the subject. A full description of a mass of material, including a critical re-examination of previous descriptions, is given and the interrelations of the various types discussed. The author concludes that three orders—Deinocephalia, Dicynodontia, and Theriodontia—probably arose from a common stock, the nearest relatives of which are the Gorgonopsida and Dromasauria. He states that the classification of the group is still unsatisfactory, but the present consideration does establish a "series of evolutionary trends which persist throughout the history of the Anomodontia." The skulls now known point out the changes that have led from the Pelycosaurs of the Lower Permian (like *Seymouria*) to the Cynodonts of the Lower and Middle Trias."

Other papers include:

Broom, "On the Structure of the Reptilian Tarsus" (*Proc. Zool. Soc.*, pt. 1, March 1921); Hewitt, "On some Lizards and Arachnids of Natal" (*Ann. Durban Mus.*, vol. iii, Jan. 1921); Laurens and Detwiler, "Studies on the Retina. The Structure of the Retina of *Alligator mississippiensis* and its Photochemical changes" (*Journ. Exp. Zool.*, vol. 32, No. 2, Feb. 1921); Schmidt, "Zur Frage nach der Entstehung der Farbzellvereinigungen. Beobachtungen bei den Gekonen *Teratoscincus* und *Gekolepis maculata*" (*Anat. Anz.*, bd. 53, Jan. 1921); and Shufeldt, "Observations on the Cervical Region of the Spine in Chelonians" (*Journ. Morph.*, vol. 35, No. 1, March 1921).

Pohlman, "The Position and Functional Interpretation of the Elastic Ligaments in the Middle Ear Region of *Gallus*" (*ibid.*, March 1921); and Rowan, "Some Notes on the Belted Kingfisher" (*Canad. Field Nat.*, vol. xxxv, Feb. 1921).

Sonntag has written on "The Comparative Anatomy of the Tongues of the Mammalia 11 Family, 1 Simiidæ" (*Proc. Zool. Soc.*, pt. 1, March 1921). This paper gives a detailed and comparative account of the structure of the tongue in the chimpanzee, gorilla, orang-outan, siamang, and various species of gibbon, and the measurements were all made on fresh specimens. The author concludes *inter alia* that the tongue of the orang approaches most nearly to that of man, and with him is the only one to possess the apical gland of Nuhn. The apical notches

and dorsal sulci are not constant enough to form a diagnostic character of any value. The papillæ on the lateral borders are provided with papillæ of a similar form in all species, although in the chimpanzee and gibbons they include all the lateral organs while in the gorilla and orang they only include a part.

Other papers include :

Bremer, "Recurrent Branches of the Abducens Nerve in Human Embryos" (*Amer. Journ. Anat.*, vol. 28, No. 2, Jan. 1921); Gibson, "Note on a Persistent Left Duct of Cuvier" (*Anat. Rec.*, vol. 20, No. 4, March 1921); Hagström, "Die Entwicklung der Thymus beim Rind" (*Anat. Anz.*, Bd. 53, March 1921); Heuser, "The Early Establishment of the Intestinal Nutrition in the Opossum. The Digestive System just before and soon after Birth" (*Amer. Journ. Anat.*, vol. 28, No. 2, Jan. 1921); Jordan, "Further Evidences Concerning the Function of the Osteoclasts" (*Anat. Rec.*, vol. 20, No. 3, Feb. 1921); King, "A Comparative Study of the Birth Mortality in the Albino Rat and Man" (*ibid.*, No. 4, March 1921); Kudo, "Studies on the Effects of Thirst. I. Effects of Thirst on the Weights of the Various Organs and Systems of Adult Albino Rats" (*Amer. Journ. Anat.*, vol. 28, No. 2, Jan. 1921); Miller, "Demonstration of the Cartilaginous Skeleton in Mammalian Fetuses" (*Anat. Rec.*, vol. 20, No. 4, March 1921); Nanagas, "Two Cases of Monoventricular Heart with Atresia and Transposition of some of the Roots of the Great Vessels" (*ibid.*, No. 3, Feb. 1921); Nordkemper, "Zur Frage der Umschaltung der parasympathishen vagusanteile im Ggl. nodosum und Ggl. jugulare" (*Anat. Anz.*, Bd. 53, Jan. 1921); Peter, "Die Darstellung der Entwicklung der Knochen" (*ibid.*, Jan. 1921); Rasmussen, "The Hypophysis Cerebri of the Woodchuck (*Marmota monax*), with special reference to Hibernation and Inanition" (*Endocrin.*, vol. 5, No. 1, Jan. 1921); Siperstein, "The Effects of Acute and Chronic Inanition upon the Development and Structure of the Testis in the Albino Rat" (*Anat. Rec.*, vol. 20, No. 4, March 1921); Sonntag, "A Contribution to the Anatomy of the Three-toed Sloth, *Bradypus tridactylus*" (*Proc. Zool. Soc.*, pt. 1, March 1921); Thiel and Downey, "The Development of the Mammalian Spleen, with Special Reference to its Hematopoietic Activity" (*Amer. Journ. Anat.*, vol. 28, No. 2, Jan. 1921); and Vermooten, "A Study of the Fracture of the Epistropheus due to Hanging, with a Note on the Possible Causes of Death" (*Anat. Rec.*, vol. 20, No. 3, Feb. 1921).

General.—Darwin and Collins in "A Universal Microtome" (*Journ. Ray. Micro. Soc.*, Sept. 1921) give a description of a new type of microtome. It combines the good features of the old Cambridge rocking microtome and eliminates some of its disadvantages. The sections are cut flat instead of on the arc of a circle, and the knife is held rigid while the block itself is moved. It will cut objects that are frozen, embedded in paraffin or celloidin, and should prove a valuable instrument in the laboratory, since it also avoids some of the disadvantages of the rotary forms.

Hogben discusses "The Problem of Synapsis" (*ibid.*, Sept. 1920). The author points out that almost all workers agree that there is evidence to show that the chromatin elements conjugate, certain authors also claim that such elements are chromosomes *sensu stricto*; but the fusion is so complete in parasynapsis that

t is almost impossible to ascertain whether or not they lose their individuality. He claims that the urgent need to-day is a knowledge of the relation of chromosomes to the organisation of the resting nucleus and a detailed study of heterotypic chromosomes.

Other papers include :

Hsieh, " A Review of Ancient Chinese Anatomy " (*Anat. Rec.*, vol. 20, No. 2, Jan. 1921) ; Lewis, " The Effects of Potassium Permanganate on the Mesenchyma Cells of Tissue Cultures " (*Amer. Journ. Anat.*, vol. 28, No. 3, March 1921) ; Loeb, " Transplantation and Individuality " (*Biol. Bull.*, vol. 40, No. 3, March 1921) ; Patten and Philpot, " The Shrinkage of Embryos in the Processes Preparatory to Sectioning " (*Anat. Rec.*, vol. 20, No. 4, March 1921).

ANTHROPOLOGY. By A. G. Thacker, A.R.C.S.

In the *Proceedings of the American Philosophical Society*, vol. lix, No. 4, Dr. R. B. Dixon, of Harvard, contributes an interesting and important article on the Polynesians. The reader will remember that the South Sea Islanders have always been classed as a Mongolian race, though they were supposed to possess an admixture of other blood. There is of course no doubt that the Polynesians are mainly Mongolian, but Dr. Dixon has made a serious investigation into the other ingredients of the population. He thinks that the first substratum was Negrito, and of this there is to this day a distinct survival in Hawai. The second substratum consists of a Negroid race, akin to the Melanesians and Australians. This element has been recognised before, and survivals of this " Melanesian " race, as we may fairly call it, are to be found in New Zealand, and also in Easter Island. The third element is that of the Malay (Mongolian) stock, which is of course predominant to-day all over the South Seas. Lastly, Dr. Dixon finds traces of an ancient Caucasic population, which he believes to be as old as the Negroids. It is in regard to the first and last of these four elements that the views put forward are novel. The existence of a possibly wide-spread Negrito race, who preceded the supposedly " aboriginal " Melanesians, is certainly extremely interesting, as is also the presence of savage Caucasians in these longitudes. The whole discussion gives one another fleeting glimpse of those great migrations which took place long before the dawn of history.

The *Annales de Paléontologie* is an elaborate periodical ; the issue for the half-decade 1916–21 is now to hand, and it is an excellent and beautifully illustrated publication. There is one article in it which calls for special mention here. This is a contribution by Teilhard de Chardin which is entitled " Sur quelques Primates des Phosphorites du Quercy." It

will be remembered that great interest has recently been displayed in the living Tarsier of the East Indies, owing to its supposed special relationship to man. Even if one is critical—as I am disposed to be critical—of the new theory which divorces man and the higher apes, the interest of the Tarsier as a primitive member of the Primates remains very great. Now in the Eocene both of North America and Europe there are a number of lemuroids, which are placed in the family *Anaptomorphidæ*, which is considered to be nearer to the Tarsier than to the true lemurs of the family *Lemuridæ*. The type genus *Anaptomorphus*—which is also the oldest genus—is North American, but there are several others found in the later Eocene of France. It is with these genera that de Chardin deals. He discusses all the representatives of this interesting little group of fossils, such as *Pseudoloris*, *Necrolemur*, and *Microchærus*, and describes one new genus, to which the name *Anchomomys* is given. A feature of the paper is the good illustrations. The reader may perhaps be reminded that in addition to this Tarsier group, other Lemuroidea, more closely related to the true lemurs, are found in the Eocene.

Other papers to be noted are :

In the *Proceedings of the Royal Society*, Series B, vol. xcii., No. B. 645 (June, 1921) ; firstly, " A Remarkable Flint Implement from Selsey Bill," by Sir Ray Lankester ; and, secondly, " Preliminary Report of the Mackie Ethnological Expedition to Central Africa," by the Rev. J. Roscoe.

In *Biometrika*, vol. xiii., Pts. 2 and 3, " A First Study of the Burmese Skull," by Miss M. L. Tildesley. And in *Man* : (1) " A Series of Rostro-Carinete Implements not hitherto described," by J. Reid Moir (August) ; (2) " The Cornish Fisherman Type," by T. H. Andrew (September) ; and (3) " Egyptian Palæoliths," by Prof. Flinders Petrie (September).

MEDICINE. By R. M. Wilson, M.B., Ch.B.

Sir Thomas Lewis and his co-workers at University College have recently made an important contribution to the study of abnormal rhythms of the heart. Sir Thomas, some time ago, drew sharp distinction between ordinary abnormal rhythms, *e.g.* palpitation, and what he called " heterogeneous rhythms."

The latter include the extra-systole auricular fibrillation and auricular flutter. In thinking of these it is necessary to perceive a departure from the ordinary method of initiation of the heart-beat. Sir Thomas Lewis uses the illustration of a prairie fire. The fire can advance only over areas of the ground which have not yet been burnt out, where, in fact, there is a sufficiency of grass. If the grass has been burned in advance in any region the fire will be stayed when that region is reached. Nor can the fire return until new grass has grown up.

If now we see the muscle of which the heart is composed as the prairie we are able to understand the abnormal rhythm. Sir Thomas likens the fire to the increase of excitability of the muscle which occurs an instant of time before the muscle contracts, and which, for all ordinary purposes, may be looked on as a part of the contraction. That portion of the muscle which has just contracted is therefore the burnt-out area. This portion will not contract again until the muscle has rested—until, that is, the " grass has had time to grow."

Now, in the case of the fire, if a ring of grass is made and a light applied to it at any given point of its circumference, the flames will travel in both directions round the ring till they meet. If, however, the flame be allowed to travel only in one direction, and if the grass should be able to grow again almost at once after it has been burned, the flame would travel round and round the ring continuously. There would always be a gap of unburnt, new-grown grass in front of it. Thus we should have a " circus movement."

Such a circus movement can be induced experimentally in rings cut from the hearts of reptiles. Sir Thomas Lewis has applied this experimental finding to the heart of man, and has shown that when a heterogeneous rhythm occurs a " circus movement " has been set up in a portion of the heart muscle. The " flame " goes round and round, finding always in front of it a fresh gap of muscle ready to " burn " or contract.

His explanation led to the idea that if the " gap " of " unburnt " muscle could be got rid of the circus movement would, automatically, come to an end. That is to say, the abnormal rhythm would come to an end.

More recently he has published some results achieved with a drug called " quinidine," a quinine compound which appears to have the effect of closing the gap and stopping the circus movement. In some 50 per cent. of cases the drug has, when administered to patients with auricular fibrillation, brought the fibrillation of the auricle to an end, and set the normal rhythm going again.

Sir Thomas Lewis does not suggest that the time for a general exhibition of the drug in practice has arrived. It is still in the experimental stage. Moreover, Sir James Mackenzie has pointed out that clots are apt to form in the auricular appendages during fibrillation, and are thus apt to be dislodged when the normal rhythm returns. This very real danger has been emphasised by other workers. Yet the value of the research as a whole will not be disputed. It has been recognised throughout the whole medical world.

On November 1 three London hospitals, the London, St. Thomas's, the Royal Free, and the Hampstead General Hospital,

inaugurated a new provident scheme the basis of which is the payment by members of a small annual premium for surgical and medical treatment in case of need for themselves and families. The scheme, known as the "Sussex," was originated by Dr. Gordon Dill, of Hove, and has proved successful in Brighton and neighbourhood. It offers the hospitals—or so it is hoped—a chance of regaining their financial stability. More important still, it gives them a direct and immediate interest in the general health of the community.

This is equivalent to saying that it will be, more than ever before, the business of the hospital to see that early disease is diagnosed and treated. Thus, the general practitioner will be brought into closer relation to the hospital, and this body, in its capacity as a teaching school, will make special efforts to afford him training in early diagnosis.

These are healthy measures, and, if the new scheme succeeds, will go far to bring about a changed outlook in the medical world. Incidentally the foundations of what may well become a national, voluntary medical service, have been securely laid.

ARTICLES

THE LAW OF REFRACTION

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Every student of elementary physics knows the laws of the reflection and refraction of light, how the angle of reflection is equal to the angle of incidence, and the sine of the angle of refraction bears a constant ratio to the sine of the angle of incidence.

The law of reflection must have been known from very early times. It is proved in the first proposition of Euclid's *Catoptrics* by means of the accompanying figure. B is the position of the eye, AC the mirror, and D the object. It is assumed as an axiom, the third of the three axioms placed at the beginning

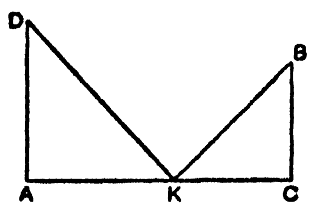

Fig. 1.

of the book, that BC is to DA as KC to KA ; the proof consists in showing that the triangles are similar, and consequently $< DKA = < BKC$. It is thus the angles which the rays make with the mirror which are proved equal, not, as nowadays, the angles which they make with the normal.

It is assumed by Euclid that the rays of light go from the eye to the object. This is made very clear by the first axiom of the *Optics*, which runs : " Let it be assumed that rays are emitted in straight lines from the eye, and are separated from one another by intervals." A justification of this axiom is given in the introduction to the *Optics*, which is of interest as dealing with physical ideas ; all the rest of the work is mathematical. This introduction first states clearly the usual arguments for the rectilinear propagation of light, *e.g.* the

phenomena attendant on the formation of shadows, the passage of the sun's rays through windows and chinks, etc. Then it deals with the necessity for the existence of gaps between the rays; we may look intently for a small object like a needle lying immediately before our nose and not see it. This is because it is lying in a gap between the rays. But if we direct one of the rays from the eye on to it, we see it at once. The same experiment proves that the rays must go from the eye to the object, because, if the needle itself were sending out rays, we should see it all the time.

Some authorities are of the opinion that the *Optics* and *Catoptrics* were not written by Euclid, the geometer, on account of the numerous errors they contain; certainly the notes to the Latin text by David Gregory, the Savilian professor at Oxford, are very critical—the books have not been translated into English. It is impossible to form a judgment on this point without a special knowledge of the period, but to the amateur

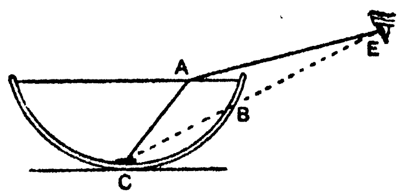

FIG. 2.

in these matters there is an apparent strong similarity in style between the geometry of the *Elements* and of the *Optics* and *Catoptrics*; possibly the latter were an account of Euclid's teaching by another hand. But there is no doubt that the *Optics* and *Catoptrics* give a good idea of the state of knowledge of the subject at the time when Euclid lived, 330–275 B.C., and that the subject of the refraction of light had not been studied then.

It is from the *Optics* of Claudius Ptolemy, the astronomer of Alexandria, who flourished at the end of the first century, that we obtain our first data on refraction. This work, which consists of five books translated from the Arabic into Latin, was not printed until 1885. It was known in the time of Roger Bacon, but escaped notice until 1816, when Delambre published an analysis of it from the manuscript in the Royal Library at Paris. A good account of it is given by Sir David Brewster in the article on Optics in the eighth edition of the *Encyclopedia Britannica*.

The fifth book deals with refraction. First of all, there is an explanation of the experiment with the coin and the basin of water, which attracted a considerable amount of attention

in ancient times. C is a coin resting on the bottom of an empty basin, E an eye which cannot see the coin owing to the edge of the basin at B coming between. But, when the basin is filled to the top with water, the rays from the coin to the eye are refracted and follow the path CAE ; hence the coin becomes visible.

In order to measure the refraction at different angles, Ptolemy employed a circle divided into 360°, the lower half of which was immersed in water up to the diameter. The centre of the circle was marked by a small coloured body, a similar body was fitted to one of the quadrants out of the water, and a third slid on the lower part which was immersed in the water. This last body was pushed with a rod, until an eye placed in air saw all three in a straight line. The results are given in the first two columns of the following table :

Air to Water.			Air to Glass.			Water to Glass.		
Angle of Incidence.	Angle of Refraction.	Refractive Index.	Angle of Incidence.	Angle of Refraction.	Refractive Index.	Angle of Incidence.	Angle of Refraction.	Refractive Index.
0°	0°		0°	0°		0°	0°	
10°	8°	1·248	10°	7°	1·425	10°	$9\frac{1}{2}$°	1·052
20°	$15\frac{1}{2}$°	1·280	20°	$13\frac{1}{2}$°	1·465	20°	$18\frac{1}{2}$°	1·078
30°	$22\frac{1}{2}$°	1·306	30°	$20\frac{1}{2}$°	1·428	30°	27°	1·102
40°	28°	1·369	40°	25°	1·522	40°	35°	1·120
50°	35°	1·336	50°	30°	1·532	50°	$42\frac{1}{2}$°	1·134
60°	$40\frac{1}{2}$°	1·334	60°	$34\frac{1}{2}$°	1·529	60°	$49\frac{1}{2}$°	1·139
70°	45°	1·329	70°	$38\frac{1}{2}$°	1·510	70°	56°	1·133
80°	50°	1·285	80°	42°	1·472	80°	62°	1·115
Mean	. .	1·311	Mean	. .	1·486	Mean	. .	1·109

In order to investigate the refraction from air to glass, Ptolemy used a semi-cylinder of glass, and arranged it with its plane surface horizontal. The graduated circle was then fixed with its diameter in the plane surface, and observations were made in the same way as before by getting three objects into line. Finally, the semi-cylinder of glass was placed above a water surface, and the refraction from water to glass investigated. The results from air to glass and water to glass are given in the fourth and fifth and seventh and eighth columns of the table.

The experiment is noteworthy as being one of the few achievements in physics which we owe to the Greeks. The other important ones, namely, the investigation of the laws of vibrating strings by Pythagoras and the discovery of frictional electricity by Thales, occurred some six centuries earlier. As is well known, the Greeks could observe and reason, but were singularly averse to experimental investigation. If we work out the numerical values of the indices of refraction from

Ptolemy's data, we obtain the results in the third, sixth, and ninth columns of the table, which give means of 1·311 for air to water, 1·485 for air to glass, and 1·109 for water to glass. The correct value for air to water is 1·333. The value for glass varies according to its composition; we do not know what Ptolemy's result should have been, but his value is probably low. On the basis of his first two results, his third result should have been $\frac{1\cdot486}{1\cdot311} = 1\cdot133$, so it is 2 per cent. out. But the agreement is good when we consider the nature of the apparatus by which the observations were made. Ptolemy, of course, did not discover the law of refraction, and knew nothing of the index of refraction. He merely left his results in the form of tables. But he applied them correctly to the explanation of astronomical refraction, *i.e.* the apparent displacement of a star towards the zenith by the refraction of its rays in its passage through the earth's atmosphere. He states that the height of the atmosphere is unknown, but that it must begin below the sphere of the moon.

Alhazen, who flourished in Arabia in the eleventh century, also made measurements on the angles of refraction and by a more elaborate arrangement than Ptolemy. He established the view that vision is performed by rays which proceed from the object to the eye. The next investigator of the law of refraction was the Polish philosopher Vitellio, whose book was published at Nuremberg in 1535. But neither Alhazen nor Vitellio discovered the law of refraction. There is some doubt also as to whether the values for the angles of refraction given by Vitellio were the result of independent measurements, or whether they were merely copied from a variant of Ptolemy's values.

Kepler attacks the question of the law of refraction in chap. iv of the *Paralipomena ad Vitellionem*, which was printed at Frankfort in 1604. The problem is to get a mathematical expression which will fit Vitellio's table of the refraction from air to water. He starts off with the idea that the solution is to be found in some of the geometrical properties of the conic sections; hyperbola, ellipse, and parabola are investigated tediously and laboriously with little result. Then Kepler breaks out into a characteristic exclamation: "O Deum immortalem quantum mihi temporis et operæ perdidit Gebri fiducia!" A few pages further on, however, we come upon a practical method for calculating the angle of refraction corresponding to any angle of incidence, which can be expressed by the formula:

$$i = \frac{\mu r}{\mu - (\mu - 1)\sec r}$$

where i is the angle of incidence and r the angle of refraction. Kepler, it is true, does not give the formula explicitly, partly because he is interested more in $i - r$ than in r, regarding the deviation as more important than the refraction; but it is easy to write the formula down from his procedure.

The formula, like the modern formula,

$$\sin i = \mu \sin r$$

is a one-constant formula. Like the latter, it reduces to $i = \mu r$, when i and r are small, for then $\sec r = 1$. The following table gives Kepler's verification of his formula:

i.	r. Vitellio's Observations.	Excess of r calculated by Kepler's Formula over Vitellio's Observations.	Excess of r calculated by Modern Formula over Vitellio's Observations.
10	7° 45′	− 11′	− 16′
20	15° 30′	− 29′	− 39′
30	22° 30′	− 19′	− 28′
40	29° 0′	+ 2′	− 10′
50	35° 0′	+ 14′	+ 4′
60	40° 30′	+ 22′	+ 1′
70	45° 30′	+ 19′	− 41′
80	50° 0′	+ 0′	− 2° 22′

The column headed i gives the angles of incidence and the column headed r the corresponding angles of refraction as given by Vitellio; it will be observed they differ from Ptolemy's values in three cases, but agree in the very bad observation at the end. The third column gives the excess of r as calculated by Kepler on the basis of $\mu = 1{\cdot}317$ over the observations. In the fourth column I have calculated the excess of r as given by $\sin i = \mu \sin r$ over the observations on the basis of $\mu = 1{\cdot}333$, the correct value for water.

It will be observed that Kepler's formula agrees much better with the observations than the modern one does, because the last experimental value, which is common to both Ptolemy and Vitellio, is very far out.

The fact that Kepler obtained a one-constant formula for the law of refraction seems to have escaped the notice of all modern writers. I have seen various statements to the effect that he used a two-constant formula and several rather patronising references to his failure to obtain the correct law. But, under the circumstances, his formula is a triumph; it is unfortunate that he did not check the observations before devoting so much labour to working them out.

As regards the differences between his calculated results and the observations, he states quite emphatically that the

observations are at fault, and have not been made with great accuracy. In calculating the value of r corresponding to a given value of i, he uses an interesting method of successive approximation which need not be given here. In his later work, the *Dioptrics*, which was published in 1611, he describes methods of measuring the refraction of glass, and shows that he fully understands total reflection, giving the limiting angle correctly; he describes and gives the correct theory of both the astronomical and Galilean telescopes, but does not arrive at the formula for the focal length of a thin lens.

Meantime, the telescope was discovered, and made in Holland in 1609 almost simultaneously by different workers; next year Galileo made his first telescope, and began his celebrated observations on the moon and planets. The construction and use of the telescope thus preceded the discovery of the law of refraction; thus often does practice precede theory. But it is clear that the discovery of the law could not now long be delayed. Willebrord Snellius, professor of mathematics at Leyden, devoted himself to its investigation, and discovered it about 1621, after many attempts. He died in 1626 at the age of thirty-five, leaving behind an unpublished manuscript on the subject. He stated that if a ray DC emitted by a point D in a dense medium were refracted at C into air, and the direction of the ray in air produced back to meet the perpendicular from D to the surface of the medium at E, then CD was always in the same ratio to CE, 4 to 3 in the case of water, no matter what angle DC made with the surface. This, it is easily shown, is the law, but in an unfamiliar form.

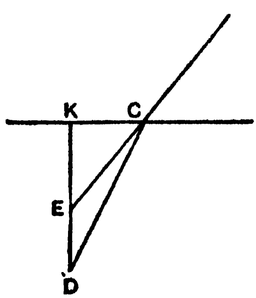

FIG. 3.

Eleven years after the death of Snell, Descartes published his *Dioptrics*, in which he announced the law of refraction in terms of sines as we have it now, without mentioning Snell's name. It is said that Descartes had access to Snell's manuscript; Huygens states that he himself had seen the whole manuscript volume of Snell, and that he believed that Descartes had also seen it. Huygens also states that Snell did not thoroughly comprehend his own discovery, and never imagined that the ratio of the two lines was the ratio of the sines. Brewster, however, thinks that it is incredible that Snell was not very familiar with the trigonometrical functions, and that he really preferred his own form of the law; there is much to be said for this standpoint, for Snell's diagram is much used by teachers at present in showing elementary classes

ignorant of trigonometry how to calculate the index of refraction.

Biot attempts to give the whole glory of the discovery to his countryman Descartes, but the preponderance of opinion is in favour of the view that Descartes' discovery was not independent. Playfair states that the question is not one on which we can ever attain certainty, but that we must be influenced by what we know of Descartes' general character and behaviour. Now, Descartes was not generous in speaking of the work of others. In dealing with the invention of the telescope, for example, he does not mention the name of Galileo, and in treating of the rainbow he makes no reference to Antonio de Dominis. It is only natural that this should produce an unfavourable impression, when we come to consider his claims in connection with the law of refraction.

So much for the statement of the law. When we consider its physical interpretation, we are confronted with the alternatives presented by the emission theory and the wave theory. According to the emission theory the ray consisted of a jet of particles. When these impinged on the surface of the denser medium some were reflected like billiard-balls from the cushion. The impact was perfectly elastic ; the component of velocity perpendicular to the reflecting surface was reversed, while the other component was unchanged. Hence the law of reflection. To explain the law of refraction it was necessary to assume that the particles were attracted, when they approached the denser medium, by the molecules of the latter. Hence the normal component of the velocity was increased. This gave the law of refraction correctly enough, but it made the velocity greater in the denser medium, a result which is contrary to experience. The case of total reflection is specially interesting on the emission theory ; the particles approach the surface of the glass from within in straight lines, pass out into the air, become subject to the attraction of the glass, describe parabolic orbits in the air, and return into the glass again. The totally reflected ray is analogous to particles shot up a tube through the earth into the air ; the latter describe parabolic orbits under the influence of the earth's gravitational field, and return to the earth again. Arguing on this basis, since the rays apparently emerged into the air at total reflection, Newton decided, that if a second glass surface were brought up parallel to the first, and the air-space between gradually diminished so as to make the second glass surface touch the top of the parabolic orbits, then the ray would enter the second piece of glass. He performed the experiment, and found his conclusion justified. At total reflection some light entered the second piece of glass, when the two glass surfaces were separated by a thin film of

air. He deduced some results about the parabolic orbits. The whole business is an interesting instance of how an incorrect theory may nevertheless lead to the discovery of a new effect.

The interpretation of the law of refraction on the wave theory by Huygens' principle is well known, and leads to the result that the velocity of light is less in the more dense medium, and that the index of refraction gives the ratio of the velocity of light in air to its velocity in the medium in question. All the different forms of the wave theory lead to the law of refraction; it must hold if there are to be boundary conditions at all, quite apart from the particular form of these boundary conditions. But it is not generally known how extensive is the application of the law of refraction. It and the whole theory of reflection and refraction for transparent media apply to metals, if the index of refraction is made complex; for mercury, for example, the index of refraction has the value $1{\cdot}73 + i\,4{\cdot}96$ where i is $\sqrt{-1}$. If $\cos\frac{2\pi}{\tau}\left(t-\frac{\mu x}{v}\right)$ represents the light-wave, where τ is the period, μ the index of refraction, and v the velocity of light *in vacuo*, the cosine can be written as

$$\text{real part of } e^{i\frac{2\pi}{\tau}\left(t-\frac{\mu x}{v}\right)}.$$

Now, suppose that μ is made complex and $= \nu - i\kappa$. Then

$$\text{real part of } e^{i\frac{2\pi}{\tau}\left(t-\frac{(\nu-i\kappa)x}{v}\right)}$$

$$= \text{real part } e^{-\frac{2\pi\kappa x}{\tau v}}\, e^{i\frac{2\pi}{\tau}\left(t-\frac{\nu x}{v}\right)}$$

$$= e^{-\frac{2\pi\kappa x}{\tau v}} \cos\frac{2\pi}{\tau}\left(t-\frac{\nu x}{v}\right),$$

a wave which is being absorbed as it progresses. It is rather curious that we can use the complex quantities in this way, and it is, of course, a great simplification; they were used thus first by Fresnel in connection with total reflection. He showed that in this case the cosine of the angle of refraction has still a meaning when the sine becomes greater than unity.

Is the law of refraction absolutely true, or is it only the approximate form of a more exact law, which we shall discover in the future, when our observations become more refined? Of course, when we attempt to verify the law in a particular instance we may find a discrepancy, owing perhaps to the temperature of the glass having altered, or to the glass itself not being sufficiently homogeneous. But we can allow for such sources of error. The question arises as to whether, after we

have made all possible allowances, there may not be a remainder, a systematic deviation between theory and observation. All that we can say at present is, that there is no trace of such a deviation, that there is nothing whatever to suggest that the law, as we have it now, is not absolutely true. It would be an almost impossible problem to calculate the probability that it is absolutely true, since it has been verified so repeatedly and in so many different ways. But when we deal with an isolated series of observations we are on surer ground. Prof. Karl Pearson states that he looks forward to the time when no physical paper will contain a curve fitting a series of observations without some estimate of the probability that the law is absolutely true, without some estimate of the goodness of fit, *i.e.* of the percentage of trials in which we should get in random sampling a fit as bad or worse. He has provided the necessary formulæ [1] and tables by which the estimate can be made.

Prof. Karl Pearson's methods are as yet quite unknown to physicists, partly because there is no good textbook explaining them, and partly because statistics is not yet an "examination subject." But on account of their great importance it is desirable to illustrate their bearing on the law of refraction here. Considerations of space make it impossible to go into them fully.

Suppose that for a series of angles of incidence we determine the corresponding angles of refraction with the utmost refinement that modern instruments are capable of, making several determinations of the angle of refraction for each angle of incidence, then there will be differences between each mean experimental result and the corresponding theoretical value. Differences are naturally to be expected owing to human fallibility and imperfections in the instrument, owing, for example, to looking at the vernier obliquely, not setting the cross-wire exactly on the middle of the image, or to errors in the graduation of the scale. The question arises as to whether the differences observed are satisfactorily accounted for by such accidental variations, or whether they indicate a real deviation from the law. The physicist usually answers this question in a rough manner by drawing in the theoretical curve, plotting the observed values, and seeing how close they lie to the curve. It is possible to be misled by the scale of the curve. Prof. Pearson answers it more accurately by stating the probability or percentage of cases in which, if the theoretical value were absolutely true, the agreement would be as bad or worse. For example, if the probability P comes out ·60, then, if we repeated the experiment 100 times under the same conditions, in 60 per cent. of these cases the agreement would be as bad or worse.

[1] *Biometrika*, p. 239, 11, 1917.

If P comes out high, *e.g.* ·80, then our formula is probably the correct one ; if P comes out low, *e.g.* ·05, the formula is a very bad fit. It is not possible to state where a good fit ends and a bad one begins.

As an illustration of the method, I have made a special set of observations on a glass slab, determining the angle of refraction three times for each of the eight angles of incidence 10°, 20°, 30°, 40°, 50°, 60°, 70°, and 80°. The apparatus employed was exceedingly simple, one of the arrangements used in schools under the name of " Pin Optics." The following table shows the calculation :

Mean Sine of Angle of Refraction.	Theoretical Value.	Difference.	(Difference)².	Sum of Squares of Differences of Observations from Mean.
·1059	·1119	− ·0060	·00003600	·00000241
·2184	·2206	− 22	484	6260
·3244	·3224	+ 20	400	7690
·4171	·4145	+ 26	676	728
·4969	·4940	+ 29	841	2681
·5634	·5584	+ 50	2500	632
·6020	·6059	− 39	1521	2393
·6328	·6351	− 23	529	3358
Refractive index .	.	1·551	Sum ·00010551	Mean ·0000299

In the table the first column gives the mean of the three observed values of the sine of the angle of refraction, the second the theoretical value, the third the difference of these two quantities, the fourth this difference squared, and the fifth the sum of the squares of the differences of the three observed values from their mean. Then by means of the formula

$$\chi^2 = \Sigma\left\{\frac{n(m-\bar{m})^2}{\sigma^2}\right\},$$

where m is the mean experimental value, $\bar{m}$ the theoretical value, n the number of observations of sin r to each value of i, and σ^2 the value which the square of the standard deviation would take for an infinite number of observations at the value of i in question, a quantity χ^2 is calculated. σ^2 is the weak point in the calculation, since the value of n is so small. I have simply taken $\sigma^2 = \frac{3}{2}$ (average of last column). For a full discussion of the method, reference should be made to the original paper. I find

$$\chi^2 = \frac{2 \times \cdot 0001055}{\cdot 0000299} = 7{\cdot}06,$$

and on reference to the tables P = ·53, *i.e.* in 53 out of 100 cases we should get in random sampling a fit as bad or worse. Or, in other words, the theoretical formula fits the observations quite well. We should not, however, expect a difference between theory and experiment to show itself, unless the observations were made with the utmost refinement possible.

Truth in physics is only a measure of probability. This we have known for long. But it is only now that methods are opening up by which we are able to estimate numerically the truth of physical laws, methods which will doubtless afford profitable research to many workers.

SOIL REACTION

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The Nature of Soil Fertility

The problem of maintaining the fertility of our richer agricultural soils and of improving that of the poorer ones is one of ever-increasing importance. Soil fertility, however, is often spoken of as though it were an absolute property of a soil : it is not always sufficiently emphasised that fertility is a particular relationship subsisting between *soil conditions* on the one hand and *crop growth* on the other. The particular conditions that conduce to soil fertility are the resultant of two main groups of factors : the *intrinsic* properties of the soil, which are dependent on the actual chemical and physical and biological nature of the soil complex and those *extrinsic* properties which are impressed on the soil by topographical and climatic factors. These two groups cannot be sharply distinguished from each other, nor can any hard-and-fast line be drawn between the various chemical, physical, and biological factors comprised in them because few are at present susceptible of any exact measurement. In other words, that particular group of conditions that make up the fertility of any particular soil is an equilibrium caused by the interactions of numerous factors, some of which can be varied by the agriculturist by manurial and cultivation operations. It is by these operations (knowledge of which has been evolved empirically throughout the ages) that the farmer is enabled to maintain or to change, to regulate and adjust the relationships between soil conditions and crop growth.[1]

Among those soil factors that are most readily susceptible of regulation and adjustment are many that cannot be allowed to vary beyond comparatively narrow limits without becoming limiting or controlling factors in crop production. The capacity of the soil to supply the necessary nutrients for plant growth is of fundamental and obvious importance : the original reserves of foodstuffs, supplemented by biological activity of the soil

[1] For an interesting discussion of fertility from this point of view see E. J. Russell, *Soil Conditions and Plant Growth* (Longmans), 4th ed., 1921, especially chap. viii.

organisms, if insufficient, must be aided by the addition of natural or artificial fertilisers. The complexity of even a single factor such as this is evident when it is remembered that the very poorest of soils apparently contains sufficient nutrient material for many hundreds of crops, and the rate at which the potential foodstuff is made available for the plant is apparently of more importance than the actual amount of nutrient material present. The micro-organic population of the soil is also of supreme importance, and not merely as food producers, although the conditions governing the equilibrium between the various genera and species form an almost untouched field of work. The relation of the soil to air, water, and temperature form another group of limiting factors of no less interest and importance than the others. All these and many more are intimately related and mutually dependent—the simple addition of a few cwts. of soluble fertiliser means far more than a mere trifling addition to the store of plant food in the soil, for such an addition is followed by an alteration in many physical and biological properties of the soil.

Importance of Soil Reaction as a Factor in Soil Fertility

Anything that produces an alteration in any of these numerous and mutually dependent factors will have its effect also on many of the others, and the complex system of equilibria existing or tending to exist in the soil will be disturbed. In particular the growth of soil organisms, as well as that of the plant itself, is very sensitive to the reaction of the medium, and it frequently happens that the presence or absence of a base will act as a limiting factor in crop production not merely through the effect of acidity or alkalinity on the plant itself, or on the soil organisms, but on account also of the varying displacements produced on all the factors that go to make up the complex chain of soil equilibria.

The question of soil reaction cannot therefore be over-emphasised, and one aspect of it, viz. soil acidity or " sourness," to use the farmer's phrase, has attracted attention from very early times. The use of lime as oxide, hydroxide, or carbonate, the last-named as chalk, limestone or marl, in correcting soil acidity, or adjusting soil reaction, is among the oldest of agricultural operations ; while of late years the study of soil acidity has occupied an important place in agricultural research. It cannot be said, however, that the enormous amount of work done has either solved the practical problem involved or clarified our ideas as to what exactly soil acidity is. This is illustrated by the almost indiscriminate use of such terms as " soil acidity,"

"apparent acidity," "real acidity," "potential acidity," "absorption acidity," "lime requirements" of soils and of plants, "immediate" and "continuous" lime requirements, "active" and "latent" lime requirements, etc.; such a multiplicity of terms tends only towards confusion and obscurity.

Theories of Soil Acidity

Some soils are apparently so acid that when moist they will redden litmus paper almost immediately, but their aqueous extracts seldom redden litmus after boiling off the CO_2. The comparatively few cases reported in which the CO_2-free aqueous extracts were acid to litmus have invariably been the result of highly abnormal conditions, and in such cases the immediate cause of the reaction is obvious. Most acid soils, however, yield so little soluble acid on extraction with water alone that it cannot usually be detected by litmus paper after boiling off CO_2.

(A) *Humic Acid Theory*

Various theories have been put forward at different times to explain such acidity as this. Sprengel in 1826 attributed the acidity to the accumulation of insoluble complex organic acids—the so-called humic acids—produced by the decomposition of plant residues left over from the crop. The dark alkaline solution obtained on treating an acid soil with ammonia was supposed to contain the soluble ammonium salts of these acids and the acids themselves could be precipitated on acidifying. Such acids were also supposed to occur in neutral and alkaline soils combined with calcium or magnesium, and are then practically insoluble in alkalies without a previous extraction with acid. This hypothesis long held the field. The compounds obtained from soils, however, were very indefinite and variable in composition and always contained mineral impurities that could not be eliminated. As analytical methods improved it was realised that these differences were real differences in composition and were not due merely to imperfections in the analytical methods, and doubt arose as to whether these so-called humic acids really were definite chemical compounds.

(B) *Selective Adsorption Theory*

The first real advance was due to van Bemmelen[1] in 1888, who considered that these bodies were not definite chemical compounds but absorption complexes, *i.e.* mixtures of a base and of various colloidal substances held together by some sort of surface attraction. These ideas were further developed into a general theory of soil acidity by Baumann and Gully about 1910. The acid reaction of peat moss and of peat soils was

[1] For detailed references to literature, see *Journ. Agric. Sci.*, xi (1921), p. 42.

attributed by these authors to the colloidal matter in the coverings of the hyaline sphagnum cells. The original sphagnum was almost as " acid " as the peat, hence it is unnecessary to assume that the " acid " is a decomposition product. Moreover, if an acid is really present it is very insoluble, as an aqueous extract of peat is practically neutral to litmus ; and this is generally true of most acid soils. These views were based on the purely chemical work of Linder and Picton, who showed that when As_2S_3 is precipitated from colloidal solution by $BaCl_2$ a small amount of Ba is carried down with it and a corresponding amount of HCl is set free. If Ca, Sr, or K. chloride is used the same amount of HCl is left behind, showing that equivalent amounts of the base are absorbed in each case. The Ba thus carried down is held very firmly and cannot be removed by washing, although it is easily replaced by other bases by digesting with an appropriate salt in solution. The work of Whitney and Ober concerning the electrical properties of colloidal solutions gave some theoretical basis to Baumann and Gully's views. and further support was accorded by the fact that acidity is developed on shaking an acid or a neutral soil with neutral salt solutions. This phenomenon was first noticed by Thompson in 1845, and was explained by Way about 1850 as due to an interchange of bases between the neutral salt and a constituent of clay—a hydrated aluminosilicate of an alkali or an alkaline earth. Later, when the general theory of surface adsorption had been put forward and developed by Willard Gibbs, J. J. Thomson, Freundlich, Wo. Ostwald and others it was extended and applied in a somewhat modified form and under the name of " selective adsorption " to soil interchanges by Cameron and later by E. G. Parker and by Harris. Parker found that when an acid soil is shaken with a solution of KCl the K was displaced by Al and other bases in exactly equivalent amounts, the Cl remaining unchanged. When NaOH was also present with the KCl just as much K was absorbed by the soil, but no bases from the soil replaced it. Parker concluded that the base is *ad*sorbed by the soil and a real acidity developed, which then dissolves from the soil the bases found in the solution. This conclusion was supported by washing out the soluble bases from the soil with HCl, washing the soil free from HCl, and treating with KNO_3 solution : a considerable amount of free HNO_3 was found in the solution besides the usual $Al(NO_3)_3$, etc.

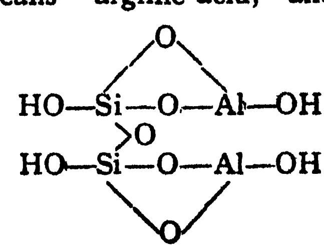
(C) *Basic Exchange Theory*

This adsorption view is, however, not universally held and many still hold that a real interchange of bases occurs, not however with the insoluble organic acids of the soil, but with the complex aluminosilicates of the clay fraction. This modern

development of the classical work of Way and that of Lemberg on the transformation of minerals by contact with salt solutions has much to support it. Armsby as long ago as 1877 pointed out that $CaCl_2$ and an artificial Na-aluminosilicate may interact to form two slightly soluble substances—Na- and Ca-alumino-silicates. In such a case the reaction does not proceed to the formation of one of these two substances exclusively, but an equilibrium is reached in which both slightly soluble compounds are present; a definite relationship then prevails between the concentrations of the reacting substances in the solution: the amounts of different bases absorbed will vary with the solubilities of their respective alumino-silicates, while change in temperature, by altering the relative solubilities, may lead to readjustments in the equilibrium. Such an absorption is in general an exponential function of the concentration, as also is the solubility of alkaline earth sulphates in acids of varying concentration (Ostwald) and the action of K_2CO_3 on $BaSO_4$ (Meyerhoffer). This form of curve is not therefore necessarily proof of adsorption, as is frequently supposed by agricultural workers. Such inter-changes were found to occur by Sullivan with many other silicates and alumino-silicates and some soils. Al is replaced by the base of a neutral salt: and this accords with Veitch's conclusion that an interchange of bases occurs between the neutral salt and the hydrated neutral silicates or alumino-silicates of the soil by which Al is brought into solution, the acidity developed being due to the hydrolysis of the Al salt. Daikuhara, Rice, and more recently Knight came to somewhat similar conclusions, and this aspect has been further developed by the work of Spurway on the "hydrolytic ratio" of soils and that of Hartwell and Pember on the part played by the Al ion in the toxicity of acid soils. This, however, is probably not the whole story, and the very complexity of the soil militates against any single factor being the sole cause of soil acidity.

(D) *Mineral Acid Theory*

A fourth theory proposed by O. Loew, and later strongly supported by Truog, attributes the acidity of certain Porto Rican clay soils deficient in humus to an alumino-silicic acid in the clay which he calls "argillic acid," and to which he gives the formula

(*a*)  (*b*)

By the neutralisation of the acid OH groups at (*a*) the acid would became neutral, while absorption of P_2O_5 is attributed to the basic OH groups at (*b*). By prolonged treatment of neutral clay, *i.e.* salts of argillic acid, with large quantities of water charged with CO_2 an acid clay may be produced. The reverse action should also take place, *i.e.* neutral salts should be decomposed by acid clay, the base being absorbed and the acid set free; and this was actually observed by Daikuhara. The work of Ashley, of Mellor, and of F. W. Clarke on the constitution of pure clays supports at any rate the plausibility of the theory, although the phenomena can apparently be explained as satisfactorily by the interchange of bases.

Methods of determining Soil Acidity

Whatever the cause or causes of "soil acidity," it is difficult to avoid the conclusion that a certain degree of real acidity is present which should be capable of measurement. Many methods have been proposed at various times, among them:

1. Inversion of cane sugar.
2. Saponification of ethyl acetate.
3. Liberation of iodine from a mixture of
   (*a*) Potassium iodide and potassium iodate.
   (*b*) Potassium iodide and potassium nitrate.
   (*c*) Potassium iodide and potassium nitrite.
4. Basic exchange with
   (*a*) Neutral salts, and
   (*b*) Salts of weak acids.
5. Absorption of base from a solution of
   (*a*) Basic hydroxide, and
   (*b*) Dye.
6. Decomposition of
   (*a*) Insoluble, and
   (*b*) Soluble carbonates.
7. Growth of *Argotobacter* in mannite and of *B. mycoides* and *B. subtilis* in bouillon.

All these various methods give different results: the degree of acidity measured by some may be anything up to ten or twenty times that obtained by others.

The conditions under which the various methods for estimating soil acidity are carried out vary so much among themselves and differ so enormously from the natural conditions in the field that the equilibrium approximately attained prior to experiment would certainly be shifted in one direction or another during the experiment. Thus Sharp and Hoagland and independently Christensen showed that the extraction of a soil with neutral salt solution, *e.g.* KCl or Ca-acetate, may actually

change the reaction of the soil solution from distinctly alkaline to strongly acid, while in practically all cases extracts prepared with neutral salt solutions showed a higher H-ion concentration than extracts prepared with water alone. Moreover, if a soil is extracted with Na-acetate solutions of varying concentration it has been found that the amount of acetic acid liberated increases, while the actual hydrogen ion concentration in the resulting solution diminishes as the concentration of the Na-acetate increases. If the extraction is carried out with Na-acetate and NaCl solutions of equivalent strengths then more titratable acid is liberated in the former case, but a higher hydrogen-ion concentration is attained in the latter.

Distinction between Acidity and Lime Requirements of a Soil

Evidently more factors than mere acidity are involved here; the absence or deficiency of certain bases from the soil brings about a condition of infertility that cannot be accounted for as being due directly only to a definite degree of acidity of the nutrient soil solution. Such a condition of infertility is remedied by a dressing of lime. But for the complete removal of this condition an amount of lime is added in excess of that required for neutralisation purposes, and it is generally assumed that this excess of lime is necessary in order to neutralise any acidity that may develop subsequently. It must be emphasised, however, that this excess of free lime probably has certain beneficial effects on the soil in addition to its effect on the reaction, and it would appear that the presence of a free base—or rather an available base, for lime quickly reverts to carbonate in the soil, in which form it is certainly not a free base although it is as available as though it were actually free—is of considerable importance in maintaining soil fertility.

Effects of Acidity and of Liming on the General Fertility of the Soil

Thus the presence of an excess of free lime appears to have a good effect on the physical condition of heavy soil due to the flocculating action of $CaH_2(CO_3)_2$ or of $Ca(OH)_2$. This effect indirectly influences the regulation of the air and moisture supplies, which in their turn affect the conditions underlying the biological and chemical relationships: both nitrification and nitrogen-fixation are promoted, the rate of oxidation of organic matter is increased,[1] and it is supposed by some that

[1] This is denied by MacIntire as a general proposition, although it may be true in some cases.

possibly a precipitation of harmful toxic substances may occur, especially salts of Cu, Zn, and the heavy metals when these are present. Truog, in fact, generalises thus: "Nearly all the chemical reactions which take place in soils are affected unfavourably in regard to fertility by an acid condition." Thus, it has been stated, an acid condition will bring about a decreased availability of phosphates due to (*a*) the formation of the less soluble ferric and aluminium phosphates from the more soluble calcium phosphates, and (*b*) the formation of complex phosphorus compounds with acidic organic matter which have a very slow rate of decomposition under acid conditions. In general, and from a chemical point of view, the elements present in soils may be divided into two groups according to whether their solubility or availability is increased or diminished by an acid condition of the soil: (1) Ca., Mg., Na., and K tend to become less soluble, while (2) Fe, Al, Mn, Cu, and the heavy metals tend to become more soluble. As the metals of the first group become less soluble and available those of the second group become more so because the carbonates or bi-carbonates of the first group act as precipitating agents for the second group, which might otherwise be in solution as other salts. These relationships are important not merely because of the increased availability of the phosphates, but on account also of the toxic action of most trivalent metal ions, *e.g.* Al and Mn. Moreover, the metals of the former group, together with P_2O_5, form the great chemical[1] "reaction regulators" of the soil solution, to which reference will be made later.

Effect of Acidity on the General Soil-Plant Economy

These considerations bring out more or less clearly how the *general fertility of the soil* is affected by an acid condition on the one hand and by an excess of lime on the other. There are, however, many other far-reaching effects of soil acidity on the general interrelationships comprised within the soil-plant economy. These have been discussed at some length by Truog,[2] who classifies the various effects under the following heads[3]:

A. The general and indirect effects of soil acidity on plant growth:
   1. Effect on the general fertility of the soil;

[1] Chemical, because soil colloids are also very efficient reaction regulators.
[2] *Soil Sci.*, 5 (1918), p. 169.
[3] This scheme does not represent the actual known effects of soil reaction on the plant-soil economy, but rather the possible effects that would have to be considered in any really comprehensive study of the subject.

2. Effect on the prevalence of plant diseases [1];
3. Effect on the competitive powers of different species of plants.[2]

B. The direct and specific influence of soil acidity on plant growth:
1. Effect on the supply of available calcium needed by plants as direct plant food material; *i.e.* the effect on the "lime requirements of the plant," which are determined by:
   (*a*) Lime content of the plant;
   (*b*) Rate of growth of the plant;
   (*c*) Feeding power of the plant for lime, which depends on:
      (i) Extent of the root system;
      (ii) Character of the root system;
      (iii) Internal acidity of the roots;
      (iv) Excretion of carbonic acid;
2. Effect on the symbiotic nitrogen-fixing bacteria of legumes;
3. Effect on the root tissues of plants.

Considerations such as these bring out the importance of distinguishing clearly between the "acidity" of a soil and its lime requirements. The latter is a more comprehensive term than the former, and has a larger value in terms of lime per acre. It is also what the farmer has in mind when he calls his soil "sour." The usefulness of the ordinary laboratory methods of determining "soil acidity"—as typified by the Hutchinson-MacLennan method in general use in this country—is due to the fact that the results obtained are always very much too high when regarded as strict neutralisation values, so that a farmer who limes his land according to the indications of such a method will run no risk from underliming, although there may in certain cases be danger of over-liming when calcium oxide or hydroxide is the form employed.

Possible Dangers of Over-liming

Cases have frequently been recorded in the literature of the addition of lime to acid soils retarding the growth of such

[1] *Cf.* soft scab in potatoes, the prevalence of which appears to depend on the reaction of the soil.

[2] This is possibly a factor in what is known as the calcifuge habit of many plants. Another instance is the insensitiveness of rye as compared with barley to an acid reaction. This difference has been traced by Hartwell and Pember to the toxicity of Al-ion, brought into solution by the high hydrogen-ion concentration.

crops as oats or wheat when grown *immediately* after the liming. This is apparently due to temporary conditions which soon disappear, when the lime will benefit these crops as well as other subsequent ones. A similar temporary condition has been noticed in that the addition of lime will sometimes apparently retard the immediate effectiveness of insoluble phosphatic fertilisers. Such a temporary condition may possibly be connected with the time required for the reversion of oxide or hydroxide into carbonate under the conditions prevalent in soils [1]; until such conversion is completed it may well happen that the OH-ion-concentration of the soil solution due to the solution and ionisation of $Ca(OH)_2$ may be too high to be consistent with fertility and it is not until the OH-ion concentration is diminished by the conversion of $Ca(OH)_2$ in $CaCO_3$ or $CaH_2(CO_3)_2$ that suitable soil conditions are obtained. This point of view seems to be supported by the work of Morse and Curry, who showed that the concentration of several elements in the soil solution of certain acid soils was diminished by the addition of lime. Iron is a conspicuous and interesting example of this: depressing the concentration of iron in the soil solution is advantageous, as it decreases the likelihood of formation of ferric phosphate to replace the calcium phosphate of normal soils; if carried too far, however, it may result in injury to the crop, as the ferric-ion concentration may, through over-liming, be reduced beyond the limit of availability to the plant, which becomes chlorotic in consequence. Instances of this have been recorded in the case of certain pine-apple soils and with some kinds of lupins.[2] This behaviour, however, is somewhat exceptional, and other plants are not known definitely to be affected, although one or two instances in this country have been brought to the writer's notice.

Soil Acidity a Phase of the Broader Question of Soil Reaction

In view of the above considerations, it is permissible to doubt the value of the usual laboratory methods as means of measuring soil acidity. At the best they can give some rough empirical information to the effect that certain soils examined will or will not be benefited by liming. They give—and can give—no quantitative information as to the degree of acidity in its strict,

[1] For a comprehensive treatment of the conditions governing the carbonation of burnt lime in soils see MacIntire, *Soil Sci.*, 7 (1919), pp. 325–446; *per contra*, see Hager, *Journ. Landw.* 65 (1917), p. 245.

[2] For a somewhat different interpretation of such chlorosis, see E. J. Russell, *Soil Conditions and Plant Growth* (4th ed.), pp. 86 and 304.

scientific, and only possible meaning, *i.e.* the hydrogen-ion concentration of the nutrient solution bathing the soil particles. And yet the hydrogen-ion concentration of the soil solution, which is in equilibrium with the reserve of acidity bound up with the soil mass, is one of the few soil factors susceptible of investigation by really accurate methods. Until comparatively recently soil acidity has been studied as though it were entirely unrelated to the ordinary physico-chemical concept of acidity; the reason being that the practical agricultural problem involved is a complex one requiring for its complete solution considerations based on colloid chemistry and physics as well as considerations of acidity in the physico-chemical sense. But a complex question such as this cannot be solved by any general method, and the attempt to do so has been the main defect in most of the earlier work on this subject. The only rational way of attacking such a problem would appear to be to resolve it into its various factors and study each factor separately—in so far as our present experimental methods permit. One of these factors is the true acidity, in the physico-chemical sense, of the soil solution—that is, acidity regarded as a function of the hydrogen-ion concentration. This, of course, is not the whole story—it is only one chapter of it—but it is known to be a fundamental one, and is besides one of the few soil properties capable of study by exact quantitative methods. Soil acidity, in this narrower sense, thus becomes a phase of the broader question of soil reaction in general, and its study should be merged in that of the more comprehensive question. This broadening of the point of view is also justified from agricultural considerations. Soil acidity is generally regarded as a pathological condition of the soil which may, and should, be removed by liming. This is not universally the case, however. In potato growing in particular an acid condition of the soil is beneficial, and is, in fact, generally preferred, because a certain degree of acidity [1] is not only not injurious to the potato crop, but is inimical to the organism causing soft scab—*Actinomyces chromogenus*—one of the worst of potato pests. This disease, however, never appears on a soil of a certain degree of acidity. This has met with some response in agricultural practice, and in this case obviously acidity is a desirable condition.

Moreover, large acid areas occur—particularly in America—where, owing to altitude, transport difficulties, or other causes, liming is a difficult and expensive and often an impossible operation, and in such cases attempts have been made to develop a definite system of acid land agriculture that appears to have met with some success.

[1] Expressed by $P_H^{\bullet} = 5$ or less.

Soil Acidity correlated with Physico-Chemical Conceptions of Acidity: Acidity as a Function of [H·]

At the outset, soil acidity should be correlated with physico-chemical conceptions of acidity; all phenomena depending on acidity—whether in the soil or in any other medium, homogeneous or heterogeneous—are determined by the concentration of hydrogen-ions in the continuous liquid phase. "Acidity," "alkalinity," and "neutrality" have a definite and quantitative meaning, and refer to the ratio of the hydrogen-ion and hydroxyl-ion concentrations in the solution.

Neutrality is the condition when the two concentrations are equal, as in pure water, and in such a case [H˙][1] = [OH'] = $10^{-7.07}$ g.-ions per litre at 18° C. Acidity corresponds to a higher hydrogen-ion concentration than this, and "alkalinity" to a lower.

A simplification can be effected by regarding intensity of acidity as a function, not of the hydrogen-ion concentration, but of the reciprocal of the logarithm of the hydrogen-ion concentration. It is not easy at first sight to compare such a series of hydrogen-ion concentrations as

$$1 \times 10^{-5};\quad 4{\cdot}0 = 10^{-6};\quad 1{\cdot}6 \times 10^{-6};\quad 6{\cdot}3 \times 10^{-7};$$

by a simple conversion, however, we get the following series:

$$\begin{array}{ll} 1 \quad \times 10^{-5} = 10^{-5} & 1{\cdot}6 \times 10^{-6} = 10^{-5{\cdot}8} \\ 4{\cdot}0 \times 10^{-6} = 10^{-5{\cdot}4} & 6{\cdot}3 \times 10^{-7} = 10^{-6{\cdot}2} \end{array}$$

and the order of the acidities is at once apparent. It thus becomes simpler to omit the 10, which is common to all, and to express the [H˙] by means of *minus* logarithms. These are merely the reciprocals of the logarithms of the hydrogen-ion concentrations and are generally written $-\log$ [H˙] or P_H. A further advantage of this mode of expression is found in expressing graphically on squared paper large variations in [H˙]. It is impossible, for example, to compare graphically on the same sheet of paper [H˙] = 10^{-0} and [H˙] = 10^{-14} g. per litre or any really large variations between these limits; but it is perfectly easy to compare the minus logarithms of these values, viz. 0 and 14 and any variation of any magnitude between these values.

Distinction between Degree of Acidity and Titratable Acidity

The degree of acidity as expressed by the hydrogen-ion concentration is, of course, not the same as that measured by

[1] The square bracket signifies concentration.

titration. In the latter case hydrogen ions are continuously removed by the alkali used in the titration, thus

$$\left.\begin{array}{l} HA \rightleftharpoons H^{\cdot} + A' \\ BOH \rightleftharpoons B^{\cdot} + OH' \end{array}\right\} H^{\cdot} + OH' \rightleftharpoons H_2O$$

The equilibrium is continually being shifted and as continuously readjusted by dissociation of more acid until all the acid has been dissociated and the hydrogen-ions removed by the alkali until $[H^{\cdot}] = 1 \times 10^{-7.07}$, which concentration corresponds roughly with the end-point of the titration. The "acidity" measured by titration methods refers to the total quantity of hydrogen-ions that can be produced from the acid when the ionic equilibrium is continually shifted by the introduction of hydroxyl-ions, and is often called *true* or *total* acidity, but would be more accurately described as *potential* or *titratable* acidity. The potential acidity of such a system as an acid soil may consist partly of undissociated acid dissolved in the soil water, or of acid derived from partly hydrolised soluble salts such as $Al_2(SO_4)_3$, or slightly soluble mineral acid or organic acid or even of "adsorbed" acid, such as phosphoric. The presence of "adsorptively unsaturated" compounds, however, such as colloids, in soils affects the hydrogen-ion concentration of the soil solution without necessarily affecting the titratable acidity and the apparently anomalous behaviour of soil extracts made with neutral salt solutions is often attributed to "selective adsorption."

Nature of Buffer Action

All properties of a solution due to acidity depend really on the particular $[H^{\cdot}]$ at the moment, which, however, may be influenced by various factors. Thus, the colloids in the soil act as *buffers*, or *reaction regulators* in presence of which the soil solution has a strong tendency to maintain its $[H^{\cdot}]$ unchanged, so that a given amount of acid or base would produce much less alteration in the $[H^{\cdot}]$ in presence of a soil than it would in pure aqueous solution. Thus 1 c.c. of $\frac{N}{100}$ HCl added to one litre of water will decrease the P_H from 7 to 5, the resulting solution being strongly toxic to many bacteria. If, on the other hand, the same amount of acid be added to a litre of water containing 50 or 100 g. of soil of $P_H = 7$ the resulting change in P_H is hardly appreciable. This effect is known as *buffer action*, the substances causing the action being known as *buffers*, and the solutions themselves as *buffer solutions*. In the case of pure aqueous solutions, the mechanism of buffer

action is clear. Thus, suppose there is present a mixture of a weak acid, *e.g.* acetic acid, and one of its salts, *e.g.* sodium acetate. The acetic acid is only slightly ionised, so that

$$\frac{[H^{\cdot}] \times [Ac']}{[HAc]} = K_A = 18{\cdot}0 \times 10^{-6}$$

The sodium acetate, on the other hand, is a strong electrolyte, and a very large proportion of it is ionised. This involves an enormous increase in [Ac'], and in order to keep K_A of the acetic acid constant [H˙] must be decreased, the result being that such a solution is by no means so sensitive to additions of acid or alkali as is pure water or pure aqueous acetic acid in the absence of the salt. This is seen very clearly in Fig. 1,[1]

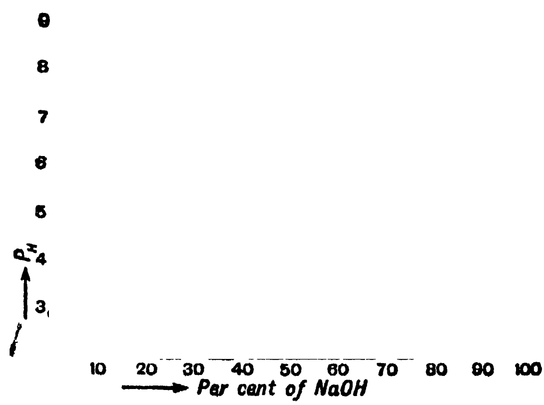


FIG. 1.—Neutralisation curve of $\frac{N}{10}$ acetic acid.

which represents the change in [H˙] of $\frac{N}{10}$ acetic acid when it is titrated with $\frac{N}{10}$ alkali. At either end of the curve when only acetic acid *or* NaAc is present the addition of 10 per cent. of NaOH will cause a large alteration in P_H. At the point X, however, where the acetate content of the solution consists of 50 per cent. acetic acid and 50 per cent. Na-acetate, the addition of 10 per cent. of NaOH or of acetic acid will produce only an insignificant effect on P_H.

Similar considerations apply to a weak base in the presence of one of its salts.

[1] Reproduced from Prideaux's *Theory and Use of Indicators* (Constable), 1917, p. 228.

The curves shown in Fig. 2 [1] bring out some features of "buffer action" that are shown even more strikingly by soil-water mixtures. The curves represent the changes in P_H when 1 per cent. and 5 per cent. solutions of Witte's peptone are titrated with $\frac{N}{10}$ lactic acid and $\frac{N}{10}$ NaOH respectively. An examination of the two curves will show that the amount of buffer action is dependent upon a number of factors, among which are: (1) The nature of the constituents as indicated by the comparison of the slopes of the curves in Fig. 2 with the slope of that in Fig. 1; (2) the concentration of the constituents, the 5 per cent. solution being more resistant to change in P_H, *i.e.* has greater buffer effect, than the 1 per cent. solution; (3) the buffer effect is not the same at all points of the curve,

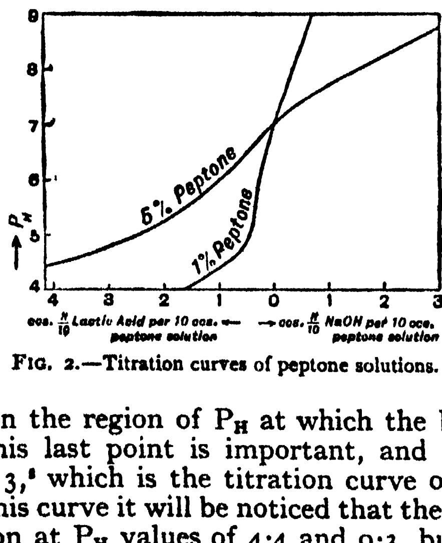


FIG. 2.—Titration curves of peptone solutions.

but depends on the region of P_H at which the buffer action is measured. This last point is important, and is brought out better in Fig. 3,[2] which is the titration curve of O-phosphoric acid. From this curve it will be noticed that there is practically no buffer action at P_H values of 4·4 and 9·3, but that there is very considerable buffer action in the region of P_H 7·4 to 6·2. This is the region between which the reaction of normally fertile soils varies and the possibility that a part may be taken by acid phosphates as reaction regulators in the lighter soils has been very largely overlooked. Acid phosphates, of course, cannot be the only buffers in a soil—salts of lime may also act as such, although not, perhaps, in the same region of P_H. The soil colloids also are powerful buffers, but nothing is known as to the region of P_H in which they act best. Fig. 2 indicates that

[1] Reproduced from Clark and Lubs' paper, *Journ. Bact.*, 2 (1917), pp. 1, 109, 191.

[2] *Ibid.*

Witte's peptone exerts its greatest effect when P_H is on the acid side of 5; but the soil colloids, from their very number and diversity, may exert considerable effect in all regions of P_H. Thus 1·67 c.c. $\frac{N}{10}$ H_2SO_4 added to a mixture of 40 g. of a slightly acid soil and 80 c.c. of water lowered the P_H from 6·6 to 5·6; the same amount of acid added to 80 c.c. pure water lowered the P_H from 6·8 to 2·5. It would appear, therefore, that the soil complex is well supplied with "buffers" which operate at all regions of P_H. In view of these facts, it is not surprising that although the lime requirements of different soils vary enormously yet the variations of P_H of the same soils are confined within

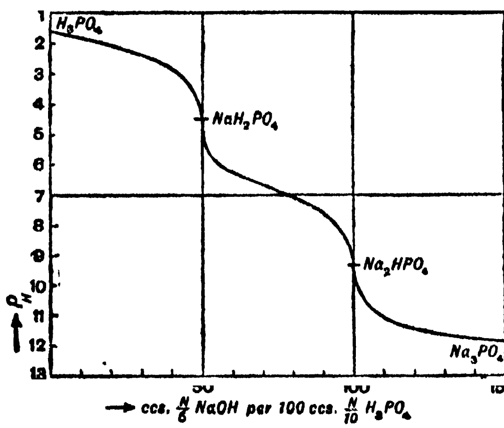


FIG. 3.—Titration curves of O-phosphoric acid.

much narrower limits. The largest variations in P_H of aqueous soil extracts are those given by Sharp and Hoagland—3·7 to 9·7. These are relatively considerable, and include extreme cases of acidity and of alkalinity. These workers found, however, that the definitely fertile soils showed strikingly similar reactions: slightly alkaline as indicated by P_H values of 7·04 to 7·52. Such wide variations in P_H as 3·7 to 9·7 must correspond to abnormally large differences in titratable acidity; on the other hand, the writer has shown that identical or nearly identical values for P_H may exist in different soil samples along with widely different amounts of titratable acidity as found either by the ordinary lime requirements methods or by the measurement of P_H. It is impossible at present to account completely for the buffer actions in soils, and it can be expressed only by the slopes of titration curves.

Methods of measuring Soil Reaction

The older titrimetric methods fail to investigate completely soil reaction, partly because they measure only titratable acidity, and partly because of " adsorptive " and other buffer effects. Soils have frequently been arranged in the order of their apparent acidities by the use of litmus paper and Harris attempted to classify soils as " truly acid " and as what Ramann called " adsorptively insaturated," according to whether the extract alone or the moist soil itself were necessary to colour blue litmus paper red. Walpole and, later, Gillespie and Wise showed that both buffer action and time of contact were potent factors in the behaviour of buffered solutions towards litmus paper: $\frac{N}{10,000}$ HCl in pure water has $P_H = 4 \cdot 8$, but will produce practically no effect on litmus paper apart from a certain amount of leaching of dye from the paper. A standard buffer phosphate solution, however, of P_H as great as 6·8 reddens blue litmus paper almost instantaneously. The fact that moist soil itself reddens blue litmus paper when its aqueous extract does not implies no necessary difference between " truly acid " and " adsorptively insaturated " soils, but merely that the buffer action in aqueous soil extracts in the absence of the solid soil is not sufficient to maintain the P_H unchanged while the reaction of the paper itself was altering. The buffer action of soils is bound up with the solid phase, and the reaction of the soil solution is maintained constant only when in contact with the solid phase. It follows, therefore, that the reaction of soil extracts bears no relation at all to the litmus test when litmus paper is used and both time and buffer factors are left out of account. No difficulty, on the other hand, is experienced in testing the reaction of an aqueous soil extract when suitable precautions are taken and the much more brilliant and sensitive sulphone-phthalein dyes are used in place of litmus.

By using a number of different indicators in combination with a series of standard solutions of known P_H, Sörensen in 1909 introduced the so-called colorimetric method of measuring P_H. The method depends on the fact that for every indicator there is a particular range of P_H within which its colour changes but gradually. A large number of indicators are known, each having its own particular zone of change, which differs from that of most other indicators. Thus, methyl red changes its colour gradually from yellow through brown to red within the zone of P_H of 6·0 to 4·4; phenol phthalein changes colour between P_H 10 to 8·5; litmus between 8 and 5; methyl orange, 4·0 to 3·0. Moreover, many of these changes overlap, so that the tint produced on a particular indicator by a particular solution,

A, allows of direct comparison of its P_H with that of a standard solution, B. At certain points where the ranges of two or more indicators overlap, the results can be checked by using more than one indicator for the determination of the same P_H. The method is not absolute ; it does not really measure the P_H of a solution, but only shows that this function is identical with that of a particular standard. Ultimately the P_H of the standards must be determined by the more fundamental electrometric method. If the standard solutions are strongly buffered by the presence of reaction regulators they will maintain their P_H unchanged for considerable periods, since small quantities of impurities from the air, glass, or slight mould growth, etc., have but little effect. Further, the standard buffer solutions are generally easily prepared, and, once made up and their P_H measured electrometrically, they can easily be renewed without making fresh electrometric determinations. The method has been applied with much success to biological fluids by Sörensen, Palitzsch, Walpole, Clark and Lubs and others, and their papers must be consulted for details of the method. Gillespie and his co-workers in America applied both the electrometric method and the colorimetric method to soil with excellent agreement, in spite of the fact that they employed aqueous soil extracts in the latter method, but mixtures of soil and water in the former, soil extracts alone being unsuitable for use with the electrometric method, as they are poor in buffer action. Further, the NO_3-ions were reduced to NH_3 by the hydrogen of the electrode, thus rendering a constant potential impossible of attainment, and in some cases even changing the reaction of the whole fluid to indicators. This is the only systematic comparison so far made between the two methods, as applied to soils, and it deserves repetition on account of its fundamental importance. The method certainly seems to present difficulties in some cases and preliminary work by the writer[1] indicated that the conditions of its application in soil investigation have not yet been completely worked out. When the method has been properly standardised it should, however, prove of the greatest usefulness in soil investigation, and it is along such lines as those indicated that soil reaction—and related problems, such as lime requirements and the relation of hydrogen-ion concentration of soils to plant distribution—can best be studied.

[1] *Journ. Agric. Sci.*, xi (1921), p. 45.

POPULAR SCIENCE

THE STORY OF TRANSITS

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One of the most interesting predictions in the history of astronomy was that of 1629, when Kepler announced that both Mercury and Venus would cross the sun two years later. It was the first prediction of a transit, and a remarkable achievement for the science of that day.

The popular ideas of transits are somewhat vague. A man recently made the statement to the writer, " I can always remember the last date that Venus went round, for it occurred a few weeks after I was born, in 1882." He had a hazy notion that something unusual had happened in connection with the planet.

Another man proposed the question in a different way. The writer was doing the astronomical work for one of the daily newspapers. Those who have had much to do with journalism know that not infrequently the readers of a paper write letters to the editor to air all kinds of views and to ask for all kinds of information. This particular newspaper prints a column or so of answers to correspondents. Some funny questions come sometimes. Venus gives rise to many. One day there came this one: " I have seen it stated in your columns recently that the star now visible in the western sky is the planet Venus. Is this correct, for I seem to remember having seen it stated in a book that this planet would not be seen again until the year 2004 ? "

It is difficult sometimes to find out what your correspondents mean or what they want to know. That would certainly have been the case with this question had not the writer mentioned 2004. This date was the clue to the whole matter, for as soon as an astronomer sees 2004 he thinks at once of the next transit of our brilliant sister world ; the confusion in the correspondent's mind was due to his ignorance of the nature of this rare phenomenon. Let us, therefore, get a clear idea of what we mean by the transit of a planet.

We may regard the sun as being at the centre of the solar system, and the planets as going round it in paths that are very nearly circles. The nearest to the sun is Mercury, then comes Venus, and then our earth. The paths of Mercury and Venus, therefore, lie between us and the sun; at some points these paths will cross the plane that contains the earth and the sun, and if either of the planets happens to be at one of these points at the right time, the earth, the planet, and the sun will be in the same straight line, with the earth and the sun at either end. At such a time the planet will be seen from the earth as a black dot against the brilliant background of the sun. To this sight we give the name of a transit of Mercury, or of Venus, as the case may be.

Whenever such a thing occurs the planet seems to cross the sun from left to right, or, as we say in the language of science, from east to west. The planets go round the sun in a counter-clockwise sense; that is a fundamental fact that cannot be stated too often; on a diagram the planets always move in the direction opposite to that in which the hands of a clock go round. Venus goes along its path at about twenty-two miles a second; we move more slowly, but still very fast, for we go along our path at something more than eighteen miles a second. So, just before the positions for a transit are brought about, Venus is always overtaking the earth, and as this goes on in the direction already stated, the planet moves across the sun from left to right. This may appear to contradict the previous statement that the movement is from east to west, for on the maps of all countries the west is found on the left, the east on the right. Exactly the opposite is the case with maps of the sky and charts of the heavenly bodies; the east is on the left, the west on the right, so that if the south be faced and the map held against the sky the cardinal points are seen just where they would be expected.

Such is the fundamental conception of a transit. In practice, however, a slight modification becomes necessary. We have tacitly made but little reference to the planes which contain the orbits of the planets. We will first of all confine ourselves to the case of Venus, and to make the points quite clear let us take a simple illustration.

Let us imagine that a big clock face is painted on a horizontal table. The edge of its circle may represent the path of the earth round the sun, the latter being regarded as a small globe placed at the centre. The earth travels round this circle, this clock face, once a year, so that it will be at some definite point on any day we care to select. The most important day of the year from the mathematical point of view is the vernal equinox, which comes on or about March 22. This is the day on which the sun rises due east and sets due west, the time

when, in the old familiar words, "day and night are of equal length." We may imagine that on this most important date, March 22, the earth is at the most important mark on the face of the clock, the XII hour-mark; then it will be at the IX hour-mark on June 22, at the VI hour-mark on September 22, and at the III hour-mark on December 22, for, as was said just now, the earth goes round the circle in the opposite way to that in which the hands of a clock rotate. The VI hour-mark will now represent the autumn equinox, whilst the IX and III hour-marks will be respectively the summer and winter solstices. We can go a step further and calculate the position of the earth on any day we choose. Thus, for example, it will be on the 57 minute-mark on April 11, on the 37 minute-mark on August 10, and so on.

The path of Venus on the diagram will be a circle of radius a little more than two-thirds that of the clock face, and so will be about one-third of the distance from the circumference to the centre. It will not, however, lie in the plane of the clock face, but will be inclined to it at an angle of about 3½ degrees, and will be placed so that half the circle lies above and half below the surface of the table. It may be helpful to think of this path of Venus as being a ring of wire round which a bead can slide. Since the ring lies partly above and partly below the table, the wire will pass through the table at two points, to each of which we give the name "node." Or, in more scientific language, the nodes are the points where the orbit of a celestial body cuts the plane of the ecliptic, or the orbit of the earth. The point at which the body passes from south to north is called the ascending node, whilst the opposite one is called the descending node. Naturally enough, the line that joins the two is called the line of nodes. In the case we are considering, in which the wire hoop representing the path of Venus is inclined to the table, the line of nodes will coincide with that joining the 17½ minute-mark to the 47½ minute-mark on the face of the clock, and the diagram will become somewhat as shown.

It will sometimes happen that the sun, Venus, and the earth will be in the same direction, as in the figure, where S is the sun, V Venus, and E the earth. In such a case Venus will not in general be projected against the sun when looked at from the earth, for it will be out of the table altogether, either above it or below. This argument applies to all parts of the wire except to those very near the two points marked M and N, the two nodes. If Venus is at M or N, it will be on the table, and if the earth is at P or Q at this time, Venus will be seen against the sun and a transit will be observed. This will also be true if Venus is very near M or N when the earth is very near P or Q.

It is not difficult to see that if the planets are exactly at M and P, or at N and Q, then Venus will appear to travel right across the centre of the solar disc along a diameter, a very rare event indeed. In such a case the transit would occupy about eight hours. More frequently, however, the path is displaced towards the edge of the sun, the "limb," as it is called technically, and the duration of the phenomenon is correspondingly reduced. We see, therefore, that a transit can occur only when the earth is near P or Q, and only then if Venus happens to be near M or N at the same time.

A calculation will show that the earth is at P on June 5, and at Q on December 7, so that whenever a transit of Venus occurs at all, it must take place on or about those dates.

One of the most interesting questions in connection with transits is the frequency with which they occur. Venus goes round the sun once in about $224\frac{3}{4}$ of our days, whilst our own year contains about $365\frac{1}{4}$ on the average. Now, thirteen times

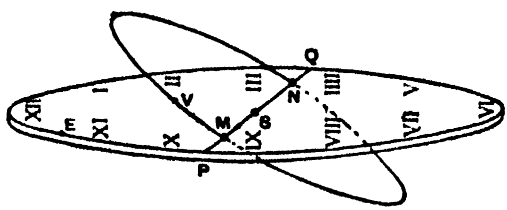


Fig. 1.

$224\frac{3}{4}$ is $2{,}921\frac{3}{4}$ days and eight times $365\frac{1}{4}$ is 2,922, results which show that thirteen Venusian years are very nearly equivalent to eight of our own. There have been many occasions when the earth has been near P and Venus near M on the same day. A transit may have taken place. Twelve months later the earth is back at P, but Venus is not at M, for it passed that point 140 days previously. In two years' time the earth comes back to P again, but again Venus is not at M. This goes on for seven years, but when the earth comes back to P in the eighth year, it finds that Venus has made thirteen revolutions and has now come close to M ; it is not quite in the same place as it was eight years before, owing to the small inequality discussed above. A transit is observed. By the time the next eight years come round the effect of this inequality has been magnified sufficiently to prevent another taking place. It is, indeed, impossible to have more than two successive transits in these short eight-year periods, for, whilst two may be separated by such an interval, yet under no circumstances can three take place in sixteen years.

Even two do not always come. If Venus is almost exactly

at M when the earth is at P we have a transit across the centre of the sun ; this is a unique event, for Venus is too far from M eight years earlier and eight years later for the passage to take place. The condition for the double occurrence is that Venus is not too close to M. The paths are then displaced from the centre of the sun and are seen near the upper and lower edges in turn. This is the case with the transits of our own time.

Although the connection between the thirteen Venusian and the eight terrestrial year periods, to which reference has been made, is accurate enough to predict a pair of transits or at least to warn us when to look for them, yet a much more exact relation can be found. A more accurate value of the Venusian year is 224·7 days. Multiplying this by 395 we get 88756·5. A more exact value of our own year is 365·256 days. Multiplying this by 243 we have 88757·2. Hence 243 of our years are almost exactly equal to 395 years of Venus, the difference being less than ·008 per cent. We may, therefore, look with confidence for transits separated by this interval of 243 years.

The following table gives the dates of transits of Venus, and also shows the two facts that have already been emphasised, that the phenomenon occurs early in June or early in December, and that successive passages at the same node are separated by either 8 or 243 years. Fortunately for observers the transits at the other node occur near the middle of this 243 year period.

A winter transit occurred on .		1631	December 7
	Add	8	years
The next transit occurred on .		1639	December 4
To the former date . .	Add	243	years
The next winter transit was on .		1874	December 9
	Add	8	years
The next occurred on . .		1882	December 6
To the former date . .	Add	243	years
The next winter transit will occur on		2117	December 11
	Add	8	years
The following one will be on .		2125	December 8
A summer transit occurred on .		1761	June 5
	Add	8	years
The next occurred on . .		1769	June 3
To the former date . .	Add	243	years
The next will occur on . .		2004	June 8
	Add	8	years
And the next on . . .		2012	June 6

Combining these, we see that transit years are 1631, 1639, 1761, 1769, 1874, 1882, 2004, 2012, 2117, 2125.

These transits have come in pairs for the last two thousand years, and will continue to do so for about a thousand years more. At the end of that time they will occur singly and Venus will cross the central part of the solar disc.

The table shows us that if we failed to see the transit of 1882, there is very little likelihood of ever witnessing one of the most interesting sights in the whole of naked-eye astronomy.

The year 1631, with which we have begun the table, was the first in which astronomers began to look for the new appearance of Venus as a black spot against the brilliant background of the sun. Two years previously Kepler had predicted the phenomenon for the early part of December, and Gassendi searched carefully for it on the 4th, 5th, 6th, and 7th; but he failed to witness it, and we now know that the transit actually took place during the night of the 6th and 7th. The first to be observed was, therefore, that of 1639, and, as far as we know, this was seen by two people only. A Lancashire clergyman named Horrocks had been making some calculations in connection with the event, and these pointed to its occurrence on December 4. This was a Sunday and the young vicar had to arrange his day as best he could so that he could watch the skies in the intervals between the services in his church. After an anxious day his labours were rewarded just before sunset, when the clouds cleared away and revealed the planet on the brilliant solar disc. Horrocks had previously informed his friend Crabtree of his prediction, and he, too, saw the transit take place.

Transits of Mercury are far more frequent than those of its sister world, for Mercury is so much nearer the sun that there is much less likelihood of it passing over or beneath the solar disc.

Precisely the same general ideas apply to transits of Mercury as apply to those of Venus. Let us take another hoop of wire and try and represent the path of this smaller planet on the clock face we used just now. We shall find that the line of nodes will be the one that joins the 22 and the 52 minute marks, the nodes themselves being at a distance, from the centre, of about one-third of the radius. The earth will be at the extremities of this new line of nodes on May 7 and November 9, so that, when transits of Mercury occur at all, they must take place on or about one or other of these dates. The ascending node is the November one and is considerably nearer the sun than is the May node, since the orbit of Mercury departs from the circular form to quite an appreciable extent. We should,

therefore, expect to have more transits in November than in May; as a matter of fact, there are more than twice as many.

The intervals between successive transits of Mercury are much shorter than those between transits of Venus. At present a transit of Venus occurs, on the average, once in sixty years. This can be deduced from the table. Those of Mercury, however, are occurring, on the average, at the rate of one in eight or nine years, seven times more frequently.

There are three fundamental periods for transits of Mercury derived in precisely the same way as the two for Venus. The first contains seven years, and it is quite possible to have November transits at the ends of this period; May transits, however, occur less frequently, and the seven-year period does not hold for them. The second period is one of thirteen years and is much more exact, so that transits may be regarded as probable when separated by this interval. It holds both for November and for May. But the third period is so much more accurate that it can be used with great confidence to predict the transits of the future. It contains forty-six years, and rarely fails.

Twelve transits of Mercury occur during the present century, nine of them in November and three in May. If we tabulate each set we shall see how the interval-periods work out.

The first transit of the century took place on . . .	1907	November 14
Add	7	years
The next occurred on . .	1914	November 6
Add	13	years
Another will occur on . .	1927	November 8
Add	13	years
Another will occur on . .	1940	November 12
Add	13	years
Another will occur on . .	1953	November 13
Add	7	years
Another will occur on . .	1960	November 6
Add	13	years
Another will occur on . .	1973	November 9
Add	13	years
Another will occur on . .	1986	November 12
Add	13	years
Another will occur on . .	1999	November 14

The thirteen-year period is very well defined, but the seven-year period holds twice only in the century. The forty-six year period is also well defined, for if we start from 1907 and add 46 we have 1953, and adding 46 again we have 1999. The periods seem to fall in the order three thirteens and a seven, giving rise to the forty-six.

There are only three May transits in the century.

The first is on	1924 May 7	
Add	13 years	
The next is on	1937 May 10	

If now we add 46 to 1924 we have 1970, in which year there is a transit on May 9.

The first transit of Mercury ever seen was observed by Gassendi on November 7, 1631, the year in which he had looked in vain for the like event for Venus.

The only importance of transits of Mercury from the astronomical point of view is that they give us very accurate determinations of the planet's place, and from these data we can discuss its orbit. The great American astronomer, Simon Newcomb, at one time made an investigation of all recorded transits in order to test the regularity of the earth's rotation. He concluded that it was not quite uniform.

Transits of Venus, however, are important, because they give us one of the famous historical methods for determining the distance of the sun. In giving this distance for astronomical purposes we do not usually express it in miles but as the magnitude of a certain angle which we call the solar parallax. This is a term we are always using; two or three words will make it perfectly clear.

Let us suppose we are looking at two vertical rods. When we place our eye in line with them they appear to be together, if neither is too near. But when the eye is moved, the rods appear to separate and are no longer seen together. Their apparent movement relatively to one another is called their parallax. Now apply this to the heavens. Suppose we are standing at a certain point on the surface of the earth. We see a star in a certain direction. If we were able to get down to the centre of the earth and look at the same star, we should see it in a different direction. The difference between these two directions is called the parallax of the star. In the diagram (Fig. 2) let S be the star, C the centre of the earth, and O the observer on the surface of the earth. The observer O sees the star in the direction OS, but if he went to the centre of the earth he would see it in the direction CS. The difference between these

two directions is the angle at S, and this is the parallax of the star for the observer. It is quite clear that, as the observer moves about, the parallax will vary. If he goes down to the line CS there will be no parallax, whilst if SO becomes a tangent to the circle, the parallax is the maximum. This shows us clearly that the star's parallax may be said to be the angle between the observer and the centre of the earth *when looked at from the star*. It would obviously be most inconvenient to have to deal with a continually varying parallax, and one that depended so much on every individual observer, and so it has been agreed that when we speak of *the* parallax of a star we always refer to this greatest parallax, which is made by the equatorial radius of the earth. So the parallax of the sun is the angle the radius of the earth makes at the sun, or, what is the same thing, the very small portion of the circle of the heavens that the radius of the earth would occupy if looked at from the sun. This is a definite quantity, but is very small indeed, less than one four-hundredth part of one degree, being, in fact, about 8·8 seconds of arc. All our calculations are directed to the determination of the exact value of this angle. The smallest alteration

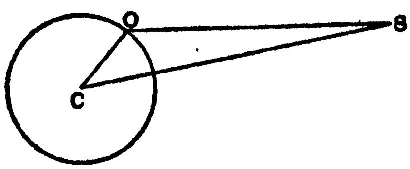


Fig. 2.

in the number of seconds means a big variation in the number of miles that gives the distance of the sun. Thus, for example:

8·75	seconds	correspond to a	distance of	93,428,000	miles
8·80	,,	,,	,,	92,897,000	,,
8·85	,,	,,	,,	92,372,000	,,
8·90	,,	,,	,,	91,852,000	,,

There are a great many different ways of determining this parallax, but the only ones that concern us at present are those based on the transit of Venus.

The first method was proposed in 1677 by Halley. Two observers go to two stations separated as widely as possible *in latitude*, one as near the North Pole as convenient, the other as near the South. These two men look at the transit and time its duration. It is not necessary to know the exact times at which the transit begins or ends, the clocks may be fast or slow, it does not much matter, for the duration is the one thing to be observed.

But this is not quite so simple a process as it seems, for the planet is not a point and does not appear upon the solar disc suddenly. When it just touches the outside edge we have what we call the " external contacts," whilst when the planet is just completely on the sun we have " internal contacts." There is a very great difficulty in observing these contacts exactly, and the actual instant of contact is doubtful by about ten or fifteen seconds, a very big error in work of this kind. The difficulty is, in part, due to optical defects both in the eye and in the telescope, for when two bodies approach very close to each other their surfaces seem to run together.

A simple experiment illustrates the idea. Place the thumb in contact with a finger and hold them two or three inches from the eye. If they now be separated slowly, it will be seen that they tend to cling together and a kind of ligament, or " black drop," is observed between them.

We have precisely the same kind of effect when Venus is entering or leaving the solar disc, so that it is quite impossible to see the actual instant of contact, although previous practice with an artificial " Venus " enables some kind of allowance to be made for it.

A very similar difficulty is produced by the planet's atmosphere, which appears as a luminous ring round Venus. There seems to be no way of getting over this trouble, which itself makes the moment of contact uncertain to the extent of five or six seconds.

There are two great objections to this method proposed by Halley. The one is that the observers must be near the poles of the earth in places that are necessarily difficult to reach and unpleasant for work. The other is that both the beginning and the end of the transit must be seen, so that the weather must be favourable for several consecutive hours at the right time—a very big demand in a polar region.

Both these objections are avoided in the method proposed by Delisle. In the first place, the two observing stations are chosen near the Equator on a line more or less parallel to that in which Venus is moving; in the second place, *one* contact only need be seen, and this may be either the beginning or the end of the transit. It must also be remembered that the weather is much more likely to be fine near the Equator than it is near the poles.

The principle of the method is simple. Let A and B be two stations on the Equator, so placed that the line joining them is a diameter of the earth. When Venus is in some position V, the observer at A will see it make its first contact; he notes the exact time, though, of course, he is faced with the " black-drop " difficulty. The observer at B also notes the

exact moment when he sees first contact; by this time Venus will have reached another position, V^1. Thus the times when Venus is at V and V^1 are both known, and hence their difference is also known. This difference is the time that Venus has taken to go from V to V^1, and from it we can calculate the solar parallax.

One further word may not be inappropriate in this article. The movements of Mercury have always given astronomers much more trouble than have those of any other planet, and for a long time seemed merely to throw discredit on the calculations. In 1869, however, Leverrier pointed out that the discrepancies could be accounted for by the disturbing effects of another planet between Mercury and the sun. Such a planet would undoubtedly be too close to the sun to be visible as an evening or a morning star, and the only way in which its existence could be demonstrated visually would be by seeing it make a transit across the solar disc. In 1859 a physician named Lescarbault thought he saw such a phenomenon, but he said nothing about it until he heard of Leverrier's announcement. He communicated with the astronomer, and the latter, after many questions and a personal inspection of the apparatus, was convinced of the genuine nature of the observation and named the planet Vulcan. It has, however, not been seen again, and it is not improbable that what the doctor actually saw was a sun-spot without penumbra. There is no doubt at all that if such a planet really exists it must sometimes make these transits across the sun, and, as the distance between the two bodies must be comparatively small, these occurrences must be fairly frequent. Close observations of the sun have been maintained for a great many years, but Vulcan has not yet been found. It is most improbable that it exists.

NOTES

Scientific Politics.—I. Self-Determination.

POLITICS, according to a celebrated definition, is an art, not a science. The present condition of the world is an eloquent witness to the truth of the formula; for those who are accustomed to the precision and exactitude of scientific method can only stand amazed at the chaos and disorder of contemporary affairs. If statecraft be an art, it is evidently one that is sometimes practised by others than great masters.

But if politics is not, and perhaps never can be, one of the exact sciences, there is no apparent reason why it should be practised with such utter disregard for all scientific theory. Man is a political animal, and he lives in a society. There are certain rules of physical health which the individual disobeys at his peril; obviously there must also be rules of social and political health which States disobey at their peril. On the moral or ethical side of the State this is universally admitted by the institution of law; but on the political side it is ignored, and a popular catchword substituted for a principle.

There have been many recent instances of the kind: a "war to end war"; "too proud to fight"; "making the world safe for democracy"; "no annexations, no indemnities"; "self-determination"; and many others. The first four are already relegated to the dictionaries of phrase and fable, for the catchword has a short life and its very meaning is quickly forgotten; and the last is in process of being discredited. But, before its actual dissolution, we propose to examine this curious specimen of political craftsmanship.

Self-determination was coined during the war as a propogandist weapon to hasten the dissolution of the Austrian Empire. It is short, it is simple, and it seems to express a political maxim in tabloid form. It caught on in the British Empire, which was neither founded nor maintained onself-determination; and in the United States, whose people fought a civil war sixty years ago to prevent the Southern States from exercising self-determination. But at the Congress of Versailles, which dealt with realities and not with phrases, its sponsors treated it as an illegitimate child, and the maxim was quietly ignored. German Austria proposed to exercise the right of self-determination

by joining Prussia; it was forbidden. Hungary has since shown some partiality for a Hapsburg; she has been refused. These policies were not in accord with Allied interests, and therefore self-determination did not operate.

But the phrase could not be so easily slain, and it has lived to work mischief in Egypt and India, and nearer home in Silesia and Ireland. It is probable that these two latter examples will finally expose its absurdity.

In Silesia and Ireland the situation is very similar. There is a German majority and a Polish minority in the former; a Catholic majority and a Protestant minority in the latter. But in both cases the population is mixed. In Upper Silesia there are preponderantly German towns in preponderantly Polish rural districts, and there are some preponderantly Polish districts among German enclaves, or vice versa. If the right of self-determination is conceded, to what unit is it to be conceded? If to the whole, then Upper Silesia is German; if to the districts, then chaos results, for one district is Polish with a German admixture, and another German with a Polish admixture.

The thing, therefore, becomes an absurdity of statecraft; for an economic unit is substituted a series of piebald political units, each jealous of its neighbours, each with a discontented minority, and each unit so small in size as to be weak and unstable. The result of applying self-determination would not be far from anarchy. The nationalist policy of the nineteenth century recognised that small national States might be better political units than great Empires, like those of the Bourbons and the Hapsburgs. But the statesmen of the nationalist movements did not commit the absurdity of suggesting that every village should determine its own allegiance; they recognised that boundaries must be drawn with some regard to the interest of the whole, that economic unity and national defence were relevant considerations, and that these might sometimes run counter to the local patriotism of a commune, or the desire of a perhaps isolated colony.

Self-determination in Ireland furnishes an equally glaring example of the folly of the phrase. If the political unit be taken as the United Kingdom, then the Act of 1801 would probably stand by a popular vote of Great Britain and Ireland. If the political unit be taken as Ireland, then the Act of 1801 would be abrogated, Ireland would proclaim a political independence which she cannot economically exercise, and another weak State would be added to those which already exist in and menace the peace of Europe.

But Ireland is manifestly not a political unit at all; the three southern provinces are distinct in population, religion, education, occupation, and outlook, from Ulster. But to

treat Ulster as a separate unit would not suit the Sinn Feiners, for there is a loyal majority in Ulster. The suggestion is therefore again made, as in Silesia, that Ulster should poll by districts, and a piebald map has been published in the London newspapers showing the probable results of such a poll. In most parts of Ulster the Protestants are in a majority ; but in two counties certainly, and in two more possibly, a careful dissection would show certain districts that are predominantly Catholic, and others that are predominantly Protestant. A division on these lines would achieve the Sinn Fein purpose—the disappearance of Ulster as a unit ; but it would put nothing in its place save a series of weak and discordant districts, nominally independent, but, in fact, subject to pressure from a greater neighbour.

In no sense of the word can this be called statecraft ; nor is it intended to be. It is a means of insinuating anarchy into an ordered State. If that is the art of politics, the onlooker can only regret that it is a bastard art, and not a science.

Scientific thought, we believe, would approach this subject from an altogether different angle. An individual being is an organism whose purposive actions are directed to the advantage of that organism. A family, a society, and a State are aggregations of individuals united by the ties of race, interest, and geography ; but together they form a more complex organism. A family may sometimes quarrel, but the tie of blood remains ; a society may sometimes be shaken by the conflict of parties, but its underlying unity must remain. If it does not, the State disappears, and the society becomes a mere mob. The more highly organised unit is dissolved into a series of disorganised individuals, and politics destroys itself by the application of its own false maxims.

It is not by such means that civilisation has been built up in the past. Man has reached his present status because he has understood how to subordinate the individual to the society, and the smaller society to the greater one. The Roman Republic began on the Tiber, but it conquered or absorbed its neighbours because its political sense was more advanced than that of the Etruscans or the Celts who surrounded it. Had self-determination controlled the policy of the Roman Senate, there would have been no need for Gibbon to write the *Decline and Fall of the Roman Empire*, for the Roman Empire would never have come into being at all. Its decline began when that great monument of human organisation relaxed its hold on the central sovereignty, and two associated governments divided the control of East and West.

The last seven years have shown on how fragile a base European civilisation stands. The nineteenth century was

one of unparalleled progress in natural science, in industry, in invention—in other words, in all those departments of life where clarity of thought and precision of method are essential. All these achievements are endangered, and some have actually been destroyed, because there was no similar advance in political thought, which remained empirical and emotional, unstable in practice and contradictory in theory.

Yet there is a sense in which politics should be the crown of all the sciences, and a knowledge of its principles a part of the common education of every citizen. Biology is the study of the development of life from the simple to the complex physical organism, psychology of the development of mentality in those organisms. It should be the business of politics to build on those foundations, to elucidate the principles underlying the progress and decay of previous societies, and to apply them to the study of the contemporary State.

The primary business of the living organism is to go on living; it seeks stability and permanence for itself and its kind in a world of changing circumstance. The primary business of the State, which is a combination of living organisms, is also to go on living; it, too, seeks stability and permanence. History shows that every society has unconsciously sought to attain that condition; and that the essentials are the maintenance of law within, and of defence from attack without. On those foundations may be built a great superstructure of many varying types, conditioned partly by the heredity and partly by the environment of the society concerned; but, unless those foundations are secure, the State will be the prey of internal disorders or external aggression, and sooner or later its doom is certain.

It is the curse of modern statecraft that it everywhere tends to ignore these elementary principles. On the one hand Germany produced the autocratic and aggressive type of State, which allowed liberty neither within nor without its borders. The world has seen to what disaster that species of organisation leads; but, on the other hand, the democratic and undisciplined type of State, more flexible and therefore more responsive to altered circumstance though it be, is peculiarly liable to forget that it too must obey the root principles on which every political organisation must be founded. Ten years ago we were inclined to confuse defence with aggression; to-day we are inclined to think that law can be set aside by policy, that murder is no longer a crime when it is practised wholesale, and that rebellion is no longer treason when it is disguised as self-determination.

The superficial type of mind perceives phenomena but not realities. It is that type which treats symptoms and not

diseases in medicine, which prefers an emotional revival to a reasonable conviction in religion, and a rhetorical flourish to a solid principle in politics. It is a misfortune that the parliamentary form of government and universal suffrage with an insufficiently educated population rather encourage that variety of politician. By temperament and training he is apt to construe a speech as an act ; and to hold that in affairs of State alone, among all the manifold activities of the world, similar causes do not produce similar effects, so long as they are sufficiently disguised by eloquence. The art of politics may be the drapery which covers the ignoble figure of a betrayal. The science of politics would strip those decorative garments, in order to examine whether the model offered for approval was consistent with the known principles of sound statecraft. If the anatomy is defective, no amount of catch-phrases, such as "self-determination," however attractive, can make it anything but a deception, which at the best is dangerous, and at the worst may be disastrous.

A century ago it was perhaps possible to govern a State by rule of thumb ; its organisation was relatively simple, and its reactions upon its neighbours infrequent and slow. In those days politics could be treated as an art, and even as an attractive and amusing drama to the chief actors, without any great risk. The more complex society of the present day cannot properly be directed by those primitive and picturesque methods. The salvation of the State can only be assured when politics are treated as a science, which would take something of the form of a higher psychology, recognising at once the liberty of the subject and the general need of the Commonwealth. Unfortunately, there are no present indications that those who lead politics to-day have any conception that scientific principles underlie the art they practise.

Albert E. F. Leyton.

The death of Albert E. F. Leyton on September 21, in Cambridge, removes a friend of many of us, and a distinguished pathological worker. He was the son of Joseph Grünbaum, a naturalised British subject, and was born in 1869, and, after being qualified, worked at the University of Liverpool, and then as Professor of Pathology in the University of Leeds, and lastly as Director of the clinical laboratory, Addenbrookes Hospital, Cambridge—being a graduate of that University. He was of that rather rare type, a critical medical man of science—that is, he did not accept readily every hypothesis which was put before him ; and the result was, of course, that people often thought him to be not only critical, but hypercritical. As Professor C. S. Sherrington, President of the Royal Society, says in con-

nection with an obituary notice of him in the *British Medical Journal* of October 8, 1921, "He was one for whom the interest of the laboratory was ancillary to, and simply enhanced his interest in, practical medicine." This attitude seems to become more rare nowadays, when the laboratory and the bedside are becoming more and more separated, much to the loss of medical practice. People thought he was too reserved; but, as a matter of fact, he was a very loyal friend and helper.

Years ago he suffered what was really a very serious misfortune to him. In 1896 he was working in Grüber's laboratory in Vienna. At that time Grüber and Durham had devised the very important agglutination test for detecting various kinds of infection by examination of the blood, but, I think, had only carried it to a successful theoretical stage. Grünbaum, as he then was, at once thought of applying it for the diagnosis of enteric fever, and had actually tried it on a few cases, but, having a true scientific capacity, did not rush into print on the subject, especially as his material was scanty. Unfortunately, while he was studying the matter further, Grüber mentioned his work at a congress, where the distinguished French physician, Dr. Vidal, happened to be. Dr. Vidal had a number of typhoid cases under him at the time, tried the test at once, and showed that it was perfectly correct and practicable—with the result that he got the credit for the invention throughout the world, the test being called the Vidal Test everywhere. Of course Grünbaum's name was forgotten in public estimation—which meant that he lost many of the opportunities which he might otherwise have enjoyed. Some years later I wrote some letters to the medical press pointing out these facts on behalf of my friend—who could not speak for himself without being accused of boasting; but, as I was not a bacteriologist, I fear that my humble efforts did not have much effect, and the test was continued to be employed throughout the world under Vidal's name—medical men do not read much, and errors of this kind once started can never be overtaken. I should like to add, however, that unlike certain piracies in connection with some tropical diseases, Dr. Vidal's action in this matter had been quite irreproachable. Even in his first paper he had mentioned that Dr. Grüber had given him the hint, and it was in no way his fault that Grünbaum lost the credit due to him. It is unfortunate that our learned societies do not pay more attention to matters of this kind. The agglutination reaction was an extremely valuable addition to the medical armoury—was, indeed, a discovery almost of the first importance, and its application to typhoid has undoubtedly saved the lives of thousands of people. It was hard on Grünbaum that his full part in this discovery was not more widely recognised; and

I think that several learned societies should have helped in getting the matter set right, in the interests of science in general. After all, the most important part of the mechanism of discovery is the discoverer.

R. R.

The Pasteur Centenary.

The University of the town of Strasbourg, in agreement with the Pasteur Institute of Paris, has decided to commemorate with the greatest *éclat* possible the hundredth anniversary of the birth of Pasteur, who made his debut at Strasbourg, and it has been decided to erect a monument of him before this University, and to have an exposition and a Congress of Hygiene and of Bacteriology there in May 1923, under the patronage of the President of the Republic and other notables. Subscriptions are invited, and should be sent to Monsieur Th. Héring, 6 Rue des Veaux, Strasbourg. The Commissaire Général of the Exposition Interalliée D'hygiène is Prof. Borrell, 3 Rue Koeberlé, Strasbourg.

Public Museums and Galleries

The recent advance in the education of the general public is seen by the increase in the interest taken in the museums of London. In the House of Commons last March Lord Sudeley proved this statement by giving statistics showing the profits gained by the sale of publications within the museums and of picture postcards of the notable pictures in the galleries. He called " attention to the recent return on museums and further results of the past year, and to their general satisfactory character in showing how largely the public has responded to the facilities offered in certain museums and galleries for gaining information as to the exhibits by popular lectures, and by the issue of hand-books, pictorial, and other illustrations." This success was due, he said, to the popularity of the guide-lecturers, who so greatly helped the public in understanding the exhibits. The Resolution was moved: " That the Government be asked to take immediate steps to extend the employment of guide-lecturers and the sale of pictorial illustrations to all museums and similar institutions which are under Government control or influence."

National Collection of Type Cultures

The Lister Institute states in a leaflet that " The Medical Research Council had long had in view the formation of a National Collection of Type Cultures, from which biologists in general and bacteriologists in particular might obtain from a trustworthy source authentic strains of bacteria and protozoa for use in scientific work. The need of an available supply of this kind had long been felt in many directions, and particularly in medical research work, for the study of principles and methods in bacteriological technique and for the systematic classification of bacteria and protozoa in their various species and strains. In the past the needs of workers in this respect had never been fully met. . . . Early last year the Medical Research Council were able, by the courtesy of the Governing Body of the Lister Institute, to make arrangements to maintain a National Collection of Type Cultures at the Institute, where all the necessary facilities have been provided." They also propose " to collect and maintain cultures of fungi of importance in phytopathology, medicine, veterinary science, technology, and soil biology, types useful for teaching purposes, and any rare or interesting species. At present it is not possible to cope with the innumerable strains of common fungi, and room can only be found for those forms with some published distinguishing

name or symbol. The co-operation of bacteriologists and mycologists is earnestly invited, and in return every effort will be made to supply the needs of applicants for cultures." All communications should be addressed to The Curator, National Collection of Type Cultures, Lister Institute, Chelsea Gardens, London, S.W.1.

The Coming Horror of Speaking Films (J. B. G.)

We are all more or less used to the American "moving picture" idea of the English chatelaine who flirts with her butler, and the English squire who wears a loud check cap and knobby-toed shoes, but may the good Lord in Heaven save us from the coming horror of the speaking films.

By the means of a fluctuating beam of light, a photophone transmitter, and a moving film in a camera, the human voice can be made to synchronise with the action of the moving picture. We will thus have the vocal accent as well as the actions of the moving picture actor brought forth in the picture-house. The English-speaking world at large will ultimately, we suppose, cultivate a fine American accent, and use much more extensively American slang terminology.

Apart from this danger, it will certainly be wonderful to hear from afar the voice of some famous man, and to see his actions at one and the same time. A political speech might thus be given in the picture-house during elections, or some great actors might give plays to be sent to all parts of the Empire. It looks as if the stage will soon become a thing of the past.

Notes and News.

H.M. the King has approved of the awards of Royal Medals by the President and Council of the Royal Society to Sir Frank Dyson, Astronomer Royal, for his researches on the distribution and movement of the stars, and to Dr. F. F. Blackman, for his work on gaseous exchange in plants and on the operation of limiting factors. In addition, the Copley medal has been awarded to Sir Joseph Larmor, for his researches in mathematical physics; the Davy medal to Prof. Philippe A. Guye, for his researches in physical chemistry; and the Hughes medal to Prof. Niels Bohr, for his work in theoretical physics.

The Nobel prize for chemistry for 1921 has been awarded to Prof. Walter Nernst, who has, just recently, been elected Rector of Berlin University.

Sir Frank Dyson has been elected Master of the Clockmakers Company.

The Pontécoulant prize of the Paris Academy of Sciences has been awarded to Dr. A. C. D. Crommelin, of the Royal Observatory, Greenwich, for his general astronomical work.

Dr. Irving Langmuir received the LL.D. degree from the University of Edinburgh when he visited the city in connection with the meeting of the British Association last September.

Dr. G. W. O. Howe, who recently left the City and Guilds College, South Kensington, to become head of the department of electrical standards and measurements at the N.P.L., has now been appointed to be first James Watt Professor of Electrical Engineering in the University of Glasgow.

The death of the following well-known scientific men has been announced during the last quarter: F. E. Armstrong, Professor of Mining at the University of Sheffield; F. A. Bainbridge, Professor of Physiology in the University of London; E. J. Bevan, of "Cross and Bevan," and County Analyst for Middlesex; Dr. W. S. Bruce, the well-known polar explorer and naturalist; Jules Carpentier, member of the Paris Academy of Sciences, designer and manufacturer of scientific apparatus; Professor Julius von Hann, the Austrian meteorologist; Dr. P. Cooper Hewitt, of mercury vapour arc fame; Rev. J. B. Lock, the writer of school textbooks in mathematics; G. Mann, Professor of Physiology at Tulane University, New Orleans, and

previously of Oxford; Dr. W. G. Ridewood, zoologist; Dr. H. Woodward, late keeper of geology in the British Museum; Mr. G. W. Walker, F.R.S., one-time superintendent of Eskdalemuir Observatory, chief of the scientific staff at the Royal Naval Mining School, Portsmouth.

We regret to have to record the death of Dr. Gustav Mann in the United States of America.

Dr. Mann was the author of that remarkable book, *Physiological Histology*, which, at the time of its publication, was far in advance of any contemporary work of its kind, and which even now forms a useful volume on the shelf of every histologist.

For some years Dr. Mann was lecturer in Histology at Oxford, where he will be well remembered both for his brilliance and his difficulty in getting on with his colleagues. Gustav Mann was a very hard-working person, and stories are still told at Oxford of how his wife used to lock up his clothes in order to keep him at home for a rest from his histological labours. Like most of us at some time in our careers, Mann was paid a miserable salary by his University, and during his stay at Oxford was pressed hard by his creditors, insomuch that another story figured him as making his bed in his laboratory in order to prevent the Oxford tradesmen from pestering him too much. We cannot vouch for the complete accuracy of this tale, but, from our own knowledge of Oxford salaries and Oxford tradesmen, we feel that there may be more than a grain of truth in the story.

Mann combined a first-class chemical knowledge with an equally first-rate histological knowledge—rare enough at any time, much more so in his day. Mann left some enthusiastic students who shared with their teacher a knowledge of both histology and the chemistry of dyes and staining; of such Dr. S. G. Scott, who died of exposure in Italy during the Great War, was an example.

Dr. Gustav Mann was only fifty-seven at his death, and the world is poorer by a great histologist.

We gather from the *Journal* of the American Medical Association that the foundations of the new University of Jerusalem have now been laid. The Jewish physicians in the United States have given $1,000,000 for the medical college building, which will be furnished inside in accordance with American standards with white tiled operating rooms. Dr. Albert Einstein will be dean of the university, and an American surgeon, assisted by an American staff, will be at the head of the medical department. Patrick Geddes, Professor of Botany of the University of Edinburgh, has drawn up the plans for the building, which will be open to students from all countries.

A valuable catalogue of British Scientific and Technical Books, covering every branch of science and technology, carefully classified and indexed, has been prepared and published by the British Science Guild of 6, John Street, Adelphi, London, and issued at a cost of 10*s*. They say in their Preface that "The classification adopted is such that, so far as practicable, related subjects are placed near one another, and under each head or sub-head the titles are arranged alphabetically according to authors' names. It is thus possible to see at a glance the volumes available in any branch of science and technology. Following this natural order, there is an alphabetical list of authors' names, and a subject index, which should be of service in furnishing an easy guide to a book or group of books upon a particular subject. For the preparation of this subject index, the Committee gladly expresses its obligations to Mr. G. S. Sweeting."

We desire to draw attention to the very useful series of monographs on *Mineral Resources, with Special Reference to the British Empire*, published by Mr. John Murray for the Mineral Resources Committee of the Imperial Institute. Six volumes, dealing with coal and the ores of manganese, lead, tin, tungsten, and chromium, have been in print for some time, and two more,

just published, deal with silver ores (6*s.* net) and petroleum (5*s.* net.) These have been prepared mainly by Mr. H. B. Cronshaw, Ph.D., A.R.S.M., and maintain the high standard of the series. They contain an exhaustive account, geographical, technical, and statistical, of the occurrence of these products throughout the world, together with a full bibliography of the principal publications concerning the subject of the monograph.

Constable and Company have just published a life of the great Metchnikoff, written by his widow, Olga. This book, describing the evolution of a great mind, of a character, and a human life, is an interesting psychological document. It reveals the life of a man who seems to have had a hard struggle in the early stages of his career. We have noted here some of the more remarkable facts brought out in Madame Metchnikoff's book.

When a young man, Metchnikoff persuaded his parents to allow him to go to study at Würzburg, where the celebrated Kölliker was lecturing. Thinking that in Germany the terms began in September, as in Russia, he arrived too soon, and, alone for the first time among strangers, felt completely lost. He was given the addresses of some Russian students, who, however, received him so coldly that he was seized with despair and immediately returned home to Russia.

Later, at Heligoland, Metchnikoff worked out a curious case of alternation of generations in certain parasitic nematodes. Delighted with his discovery, he hastened to communicate it to Leuckart. The latter appropriated Metchnikoff's discovery as his own and published it under his own name. In his despair, the youth confided in Claus, another German Professor of Zoology, who told him that Leuckart was in the habit of such dealings. Later Metchnikoff, at Göttingen, was given a valuable lizard to anatomise and describe. Elie was not good at technique, on account of his nervous temperament; he made a "mess" of the lizard, threw it across the room, and left that laboratory soon after.

Metchnikoff was ever a rebel against social conventions. At Odessa, Cienkovsky, a man of great European culture, tried to take him in hand: he reproached Metchnikoff with a lack of self-control, and undertook the paternal task of civilising this impulsive, fiery, and often violent young man.

He preached him tolerance towards the opinions of others, and strict self-discipline. Later on, after the death of his first wife at Madeira, Metchnikoff, a prey to morbid thoughts, said to himself: "Why live? My private life is ended; my eyes are going; when I am blind I can no longer work; then why live?" He then took a dose of morphia. He did not know, however, that too strong a dose, by provoking vomiting, eliminates the poison. He recovered from this attempt to commit suicide, and later turned once more to science. (*Life of Elie Metchnikoff*, Constable, 1921.)

The Report of the Food Investigation Board for 1920 (H.M. Stationery Office, price 1*s.* net) contains a summary of a good deal of interesting information which has been obtained by the several Committees of the Board. The Engineering Committee is now in a position to report the results of its investigations of the flow of heat through walls and of the thermal conductivity of various insulators. It is found that the heat-flow through the solid parts of a heat insulator is negligible compared with that due to convection currents in the air contained in its pores. Thus, the specific conductivity of a particular substance, *e.g.* cork, depends much more upon the form and size of the air spaces—that is, upon the "packing" of the substance—than upon the specific conductivity of the material considered as a continuous solid—a result which explains the want of agreement in the thermal conductivity assigned by different observers to the same substance. It was, moreover, unexpected, because of the small dimensions of the air spaces concerned. The study of the mode of escape of heat from the surface of a wall has also led to novel results. The chief source of loss is by convection currents set

up in the air. These currents begin at the base and the flow is at first of a simple linear character. Over this region, which extends for, say, two feet from the base, the rate of loss diminishes as the height increases owing to the ascending current of air getting warmed. At a certain critical level, however, the linear flow breaks up into turbulent motion, and the rate of heat loss increases and then becomes constant and independent of the height.

The Fruit and Vegetables Committee is investigating methods for storing apples. Three methods are available: (i) cold storage; (ii) storage in an atmosphere of low oxygen concentration—" gas " storage; (iii) storage in the usual apple shed. A critical comparison of methods (i) and (ii) is deferred until next year, but as between (ii) and (iii) it seems to be established that the storage life of " gas-stored " apples is twice that of those kept without this precaution. In both cases apples grown on grass-land kept better than those from trees on open ground. The Meat Committee has investigated the cause and effect of the " black-spot " mould due to the fungus *Cladosporium herbarum*, which develops on meat in cold store and has decided that this fungus secretes no substance in its growth poisonous to human beings, so that meat so affected need not be condemned as unfit for food. This rather unpleasant conclusion is modified by the statement that " where ' black-spot ' is accompanied by putrefactive bacteria it is *wise* (!) in the present state of our knowledge not to allow such meat to be used for food " (the italics are ours).

The Department of Scientific and Industrial Research has a number of other reports during the last quarter, but they are of too limited an interest for abstract in these Notes. They include the first section of the Report of the Fuel Research Board for 1920 and 1921, dealing with the steaming of coal in vertical gas retorts; also Technical Paper No. 4 from the same Board on the Carbonisation of Peat in Vertical Gas Retorts (describing two admittedly inadequate tests); and a Statistical Supplement to the Final Report of the Nitrogen Products Committee. These are all to be obtained from the Stationery Office, Imperial House, Kingsway, at prices ranging from 6*d*. to 1*s*. 6*d*.

The Publications Department of Messrs. Cadbury's works at Bournville has issued a most interesting pamphlet describing the conversion of eight coal-fired Lancashire boilers to burning oil fuel during the coal strike. The " Scarab " apparatus employed was installed so expeditiously that manufacturing operations were shut down for only four days, while the August holiday enabled four of the boilers to be reconverted for burning coal. Owing to the high price of oil, the oil system cost more than the coal, but in all other respects was found to possess considerable advantages. One ton of oil produced the same amount of steam as two of coal; the labour required was less, the flues and the boiler-house itself kept cleaner, no smuts were discharged into the air, and there are no coal-dumps from which coal-dust is blown where it may do harm. Finally, 15 per cent. more steam was obtained from each boiler when oil was used. Messrs. Cadbury are keeping the whole installation intact and, as far as possible, in position, so that, in case of further strikes, or an alteration in the relative cost of the fuel, the oil system may be restored with a minimum of delay.

We have received from the National Research Council of the United States Bulletins No. 10 and No. 11. The first contains a valuable report on the present state of Photo-electricity by Prof. A. Ll. Hughes, of Queen's University, Kingston, Canada, and the second two papers on the Scale of the Universe, by H. Shapley, of the Mount Wilson Observatory, and H. D. Curtis, of the Allegheny Observatory. Prof. Hughes's report is divided into sections corresponding to the chapter headings of his previous book on Photo-electricity (Cambridge University Press, 1914) with the addition of a long chapter summarising the work carried out from 1913 to 1919 or 1920

on ionising and radiating potentials. The monograph forms a most admirable survey of the subject, and can be obtained from the National Research Council, Washington, D.C., price \$1. The other Bulletin is interesting as presenting the evidence relating to modern views of the size of our galactic system. Dr. Curtis still maintains the dimensions assigned some twenty years ago by Newcomb and others, *i.e.* that the diameter is probably not more than 30,000 light years and the thickness not more than 5,000 light years, while Dr. Shapley considers that these numbers should be 300,000 and 30,000 respectively. Space does not permit of the long abstract which would be necessary to give an equitable summary of the two theses, but the question seems to hinge (1) on a correct estimate of the distance of any one star cluster; (2) on the nature of the spiral nebulæ, since the "island universe" theory of their structure seems to require the smaller estimate of the dimensions of our galactic system.

Christian Champy, the well-known French histologist, has lately carried out a remarkable experiment on male newts. These were starved in spring at the usual time of most active sperm-formation. Such "starvation-castrated" newts were found to have their testes completely degenerated into a band of fat containing a few indifferent germ-cells.

When these individuals were fed up next year, they were found to take on the secondary sexual characteristics of the female triton, and their gonads changed into an ovary-like structure containing true ovocytes.

Intestinal myiasis in man is fairly rare, but a case came under our notice quite recently. A general practitioner brought to the laboratory a number of maggots which had been passed by a child which had suffered from no outward symptoms. These maggots were found to be the larvæ of some muscid (probably *C. vomitaria*). They had probably been introduced into the child's system by its eating maggoty food. The larvæ easily withstand the small percentage of HCl in the digestive juices, and after passing from the stomach would become "acclimatised" to the conditions in the intestine. The doctor who found this case said that in thirty years' practice he had never met with a similar occurrence.

The *Report on the Zoological Survey of India* for the years 1917–20 has just been issued by Dr. N. Annandale, Director. Those of us who have watched the activities of the Zoological Survey of India, as evinced by the many splendid contributions in the *Records of the Indian Museum*, have been forced to admire both the efficiency of the Director and his staff and the enthusiasm which must inspire these men. The staff at present consists of Dr. Annandale, and Drs. Kemp, Sewell, Chandhuri, and Prachad.

In October 1918 the Government of India decided to offer to a limited number of distinguished zoologists or anthropologists who had rendered special service to the Zoological and Anthropological Section of the Indian Museum or to the Zoological Survey of India, the title of Honorary Correspondent, Zoological Survey of India. The following is a list of the first appointments made:—

Resident in India

Dr. J. R. Henderson, C.I.E., Superintendent, Government Museum, Madras; Lieutenant-Colonel J. Stephenson, C.I.E., I.M.S., Principal, Government College, Lahore; Mr. T. Southwell, Director of Fisheries, Bengal, Bihar and Orissa.

Non-Resident

Lieutenant-Colonel A. W. Alcock, F.R.S., C.I.E., London School of Tropical Medicine; Lieutenant-Colonel H. H. Godwin-Austen, F.R.S., Godalming, Surrey; Mr. J. G. Arrow, British Museum (Nat. Hist.), London; Mr. H. Balfour, Pitt Rivers Museum, Oxford; Dr. G. A. Boulenger, F.R.S., British

Museum (Nat. Hist.), London; Dr. W. T. Calman, British Museum (Nat. Hist.), London; Lord Carmichael of Skirling; Sir Charles Eliot, K.C.M.G., The University, Hongkong (now His Britannic Majesty's Ambassador, Tokyo); Mr. H. C. Robinson, Director of Museums, Federated Malay States; Professor F. Silvestri, Laboratorio di Zoologia Generale, Portici, Italy; Dr. A. Oka, Zoological Laboratory, Kotoshihan Gakko, Tokyo; Dr. R. Koehler, Lyon, France; Dr. H. A. Pilsbry, Academy of Natural Sciences, Philadelphia.

The first three gentlemen have since left India, and have now been added to the roll of non-resident Honorary Correspondents.

In the *Records of the Indian Museum*, vol. xviii, is a short note by Tokio Kaburaki, on the "Horse-leech," *Limnatis Nilotica*, which in the process of being swallowed whilst people are drinking infected water, fixes itself to the mouth, throat, and nasal cavities of man and beast. Kaburaki's material was obtained at Quetta, Baluchistan, through Dr. Annandale. The leeches were got in the throat of an Austrian prisoner, who had been brought from Persia: he had sucked up six leeches whilst drinking from dirty pools in Persia. The six leeches had stuck in the back of his throat for eight days.

In the same Records, Dr. Annandale gives some notes on the aquatic fauna of Seistan. In his hurried journey across the desert of Seistan and the Afgan-Perso-Baluch frontier, he cursorily examined the desert springs for animal life. Only three species of molluscs were found: *Melanoides*, *Gyraulus*, and *Corbicula*. Such molluscs showed very little structural response to their environment. Of insects, a mosquito larva was commonest, and sometimes Corida occurred. Ostracods (Crustacea) sometimes swarmed in these desert springs, and one leech, *Limnatis Nilotica*, was noted.

Mr. T. Southwell, in the "Reports," records the first occurrence of a larval cestode in the umbrella of a jelly-fish. The latter was a *Rhizostomous medusa* (*Acromitus rabanchatu*) collected from near Barkuda Island in Lake Chilka. These undoubted plerocercoid larvæ may have come to that position accidentally, and Mr. Southwell regards their occurrence in *Medusæ* as representing a cul-de-sac in their life-history.

We can recommend two books to readers interested in general science. The first of these is *Einstein the Searcher, His Work Explained from Dialogues with Einstein*, by Alexander Moszkowski, translated by Henry L. Brose, Methuen & Co., London, 1921. The work is rather Boswellian and scarcely belongs to the English world, immersed as it is in business, games, and politics, but rather to another world, namely, that of the Old Germany of Goethe and the philosophers and men of science. It is refreshing to turn from our puerile literature of the day to people who discuss with interest and intelligence matters of more general importance than business, games, and politics.

Whatever branch of study he may be engaged in, the scientist is very probably a fisherman, and will welcome the fine scholarly book by Mr. William Radcliffe on *Fishing from the Earliest Times*, admirably reviewed by the Rev. Donald Macrae on page 496. The work must have cost years of labour, and is a valuable addition to the world's stock of learning. It is also very amusing from end to end. There is a plate of Poseidon, Heracles, and Hermes fishing, taken from a lethykos of about 550 B.C. These worthies are dressed in archaic Greek costume, at a time when they wore more clothes than they did later. Evidently Mercury has taken out the other two fishing and is acting as gillie, because he holds something like a gaff and is pointing into the stream. Neptune and Hercules are seated on rocks opposite to him. The former has brought up with his trident a small fish at which he is evidently not pleased (it appears to be a kelt), while Hercules is enthusiastic over a six-foot rod and (evidently) a worm! Fancy Hercules descending to worm-fishing!

CORRESPONDENCE

To the Editor of "Science Progress"

THE CHROMOSOME THEORY OF INHERITANCE

From Prof. E. W. MacBride, F.R.S., D.Sc.

Sir,—In the October number of Science Progress there appear two communications from the pen of Mr. Julian Huxley. One of these is a letter in reply to one of mine published in the previous number of your journal; this letter you were good enough to let me see in proof, and I have replied to it in the October number. The other is an article which is described by Mr. Huxley as an effort to set forth an alternative theory of inheritance to that put forward by me in the number of the journal which appeared in March 1921.

I should like to make a few comments on the facts adduced and the arguments employed in Mr. Huxley's article.

Mr. Huxley begins by giving an able and popular account of Morgan's theory of the chromosomes as the basis of inheritance, and in particular of that special type of inheritance which is ordinarily described as Mendelian. With much of what he says I find myself in cordial agreement. Thus, when we consider that the sperm-head which carries into the egg the paternal heredity is merely a condensed mass of chromosomes, it is obvious that the material basis of inheritance must be in the chromosomes. Moreover, when we note that when the sperm-head enters the egg it swells up and becomes changed into a male pronucleus, which exhibits exactly the same number of chromosomes of the same shapes as those found in the egg-nucleus, with which it is about to unite, we feel impelled to believe that each chromosome must have its own peculiar value, contain in fact its own special type of living substance. This belief becomes strengthened when we further discover that an egg can be stimulated to develop without the intervention of the spermatozoon, and will still develop into a typical animal, since its nucleus contains one sample of each kind of chromosome; whereas if a small egg be entered by two spermatozoa abnormality and death ensue, since in this case the chromosomes are irregularly distributed between the four cells which result from the first division, and several of these do not receive a representative of each type of chromosome.

Again, when we reflect that each cell of the body of a normal animal contains two samples of each type of chromosome, one of maternal and one of paternal origin, and when we consider how this number is reduced to half in the ripe germ-cells, we must admit that there is great plausibility in Morgan's theory that two Mendelian allelomorphs which differ from each other in a single character are distinguished from one another by the fact that a chromosome in one has undergone a modification which has not occurred in the homologous chromosome of the other.

But when Morgan goes further, and endeavours to prove that a particular modification of character is due to a change in a special region of one chromosome, and on the basis of linkage of characters in inheritance to construct

chromosome maps, whilst we admire the boldness and ingenuity of the hypothesis, we are assailed by serious doubts.

Great is the suggestive power of confident assertion, and we have constantly to remind ourselves that when Prof. Morgan and Mr. Huxley tell us that the " gene " or factor which controls eye-colour in the fly Drosophila is " located " at a particular spot in a certain chromosome, they have only the evidence that white eye-colour for example is " linked " with certain other characters. Now, Mr. Huxley tells us that eye-colour is controlled, not only by a gene in one chromosome, but by several other genes situated in different chromosomes as well; this means, in fact, that the original hypothesis on which the whole location of the gene was founded has turned out to be illusory, and that eye-colour may be linked with quite different characters from those with which it was first found associated; but this distressing discovery is carefully masked by the imaginary construction of two totally distinct genes producing the same result.

But whether this assumption of the specific character of chromosome regions is ultimately justified in whole or in part, or not, is totally irrelevant to the points at issue between Mr. Huxley and myself, and it is only in respect to these points that his theory becomes in any sense alternative to mine.

These points are two in number, viz.: (1) Mr. Huxley regards the abnormal specimens which turned up in Morgan's cultures of the fly Drosophila as fair samples of the kind of variation with which natural selection has worked in bringing about the evolution of existing plants and animals, whereas I insist that these "mutants" are degenerative teratological specimens, whose divergence from the type resemble in no way the changes which have taken place during the course of evolution, and (2) Mr. Huxley maintains that there is no evidence for the inheritance of acquired characters. I believe, on the contrary, that there is strong evidence that acquired characters are inherited, and that this kind of inheritance gives us the key to the understanding of the evolutionary process.

To take the first point first. The greatest advance in Mendelian theory, since the days of Mendel, was that made by Bateson and Punnet, when they showed that the difference between dominant and recessive allelomorphs consisted in the absence in the recessive of a factor which was present in the dominant. One is staggered by the calm assurance with which Mr. Huxley informs us that this theory is being " gradually given up." By whom is the theory being given up, we should ask, and why? It is true that Morgan does not like it, and seeks to evade it, because he sees clearly what it implies, but in this he is not followed by English Mendelians, and with justice, for the facts are loud-voiced in its favour—a single instance will make this clear. The albino fly, and, for that matter, the albino mouse, is recessive, when crossed with this type. In the case of the mouse, Cuénot proved that albinism is due to the absence of a substance which he called *chromogen*, whose action was necessary in order that the colour-producing elements should make themselves effective. That something is defective in the albino is also clearly shown by its general weakness of constitution as compared with the type. Now, if the material basis of inheritance lies in the chromosomes it is an irresistible inference that, when a weak and defective individual is produced, the chromatin from which it took its origin must have suffered damage. We know that if the spermatozoa of the frog be exposed to the harmful influence of radiations from radium, and then used to fertilise normal ova, monstrous tadpoles will be produced. In the case of the albino fly complete loss of pigment is confined to the eye; but the accompanying weakness of constitution is no less marked. But it may be replied there are other colours, besides pure white, which are recessive to the type colour. These other colours—eosin and cherry-red, for instance—show the same linkage as albinism, and are supposed to be due to genes which have their location in

the same part of the chromosome as the gene for albinism. The vastly simpler and infinitely more probable supposition is that these mutants represent lesser degrees of damage to the chromosome of the same kind as that represented by the change which produces albinism. It is these minor degrees of damage which are relied on by Mr. Huxley to bring us back to the Neo-Darwinian position—that is, the view that evolution is based on the occurrence of minute inheritable changes in all directions—of which natural selection ensures that only the useful survive. I, too, believe that evolution is based on the selection of small variations; but these variations are of the nature of the expansion and diminution of function, selection, in fact, of vigorous individuals, which react well, and rejection of weakly individuals, which fail to react. These differences are totally dissimilar, not only in amount, but in nature to the mutants of Drosophila. This even Mr. Huxley admits, when he says that "the alterations of a gene bear no relation to any Lamarckian change."

The overwhelming majority of mutations which can be crossed with the type, and show Mendelian inheritance, are of the recessive degenerative nature. So markedly is this the case that Bateson, in 1914, suggested that all evolution had consisted in the "dropping of factors," and that all the qualities of Shakespeare were implicit in the original Amœba, with which animal life began. This, to my mind is the final *reductio ad absurdum* of the whole theory that evolution has been accomplished by the agency of Mendelian mutations.

It may, however, be answered that there are some few mutations which, when crossed with the type, are dominant. This is true, but the odd thing is that even in these cases the mutation is of a monstrous character. This is obvious in the case of certain dominant mutations which turn up in man, such as brachydactyly and premature and excessive sloughing of the epidermis; but, even in the case of the combs of fowls, it can be shown to be highly probable. The original shape of comb, with a single serrated edge, is found in the wild bird, and is the only form of comb that is found in any species of *Gallus*—which is the only genus of birds which possess combs. The combs of domesticated breeds show various degrees of aberration from this; the lesser degrees of aberration, such as the rose-comb, do not obviously appear to be pathological, but the series culminates in the Polish fowl, in which the comb is replaced by a tuft of feathers seated on a protuberance of the skin, beneath which is a huge vacuity in the skull exposing the brain.

The most plausible explanation of the dominant mutations would seem to be that they are due to morbid exaggerations of processes which in the type are regulated and kept in check; but that, in order that this check shall be effective in any given individual, it is necessary that both sides of the house should be normal; that in the hybrid produced by crossing the mutant with the type the check exercised by the maternal or paternal side alone is not sufficiently powerful to regulate the process.

But, Mr. Huxley assures us, the pupils of Prof. Morgan have shown that some of the mutants of Drosophila resemble the type in other species of Drosophila. I have no doubt that they have done so, and, in case Mr. Huxley requires other instances, I will supply them. The fur of the albino rabbit resembles in colour the fur of the ermine, and is sold as such to our unsophisticated factory-girls. Again, there is an albino variety of the axolotl which is of the same pale flesh colour as the cave-newt Proteus. At first sight these resemblances appear to afford a clue to the manner in which the loss of colour took place in the ermine and in the cave-newt; the expert, however, knows otherwise. The lady who buys her ermine muff is careful to look for the yellowish tinge, which indicates the last trace of the colour which the ermine wears in summer. If the albino axolotl be exposed to light on a light background some slight traces of pigment are developed, but the Proteus

similarly exposed develops a deep continuous coat of pigment similar to that of the land-newt.

In the case of the mutants we have the *loss of something* from the constitution; in the case of the wild species we have the *functional regulation of something*, which is a widely different matter. The supposed instances of agreement between Mendelian mutation and specific characteristic will have to be critically tested before any of them can be admitted to be sound.

Mr. Huxley takes me to task because I have called Prof. Morgan's pupils "factor-mad." He admits that they have put forward wild theories, but says that their enthusiasm is pardonable, and that their theories "have not been disproved." No, and I shall venture to predict that they will never be disproved; for I am confident that, as fresh facts turn up, which are inconsistent with these theories in their present form, new imaginary and undemonstrable genes will be invented to account for them, and of that process there is no end.

If the view which I have put forward in the preceding pages is sound, it is hopeless to expect, by analysing the monstrosities of the mutants, to arrive at conclusions as to how the chromatin is built up. On this subject I should like to quote some wise and pregnant words of Prof. R. Harper, which, as retiring President, he addressed to the American Botanical Association ("The Structure of Protoplasm," *Amer. Journ. Botany*, 1917):

"The realisation of the weakness of Mendelism in relation to facts as to the all-pervasiveness and interdependence of plant characters has led some of the defenders of the theory to assert that each unit-factor may possibly influence every part of the mature plant. The difficulty with all such assumptions of hereditary units of whatever kind is more fundamental. Take the case of the serrations of the leaf of the common nettle. Correns told us, in 1903, that entire margins and serrate margins were due to a factor in the germ-plasm, for serratures paired with one for entire margin and the whole was made an example of dominance and segregation. It is to be noted that Correns found teeth weakly developed in his recessives, and I am willing to predict, on the basis of my studies on sugar and starch characters, and aleurone colour in corn, that a whole series of intermediates between serrate and entire can be found, and that a present-day student, instead of saying that there is one factor for toothed margin, would say there are several, perhaps twenty. If serrateness and entirety were found to be absolutely hard-and-fast categories, there might be something to be said in favour of assuming that each was represented by an equally hard-and-fast, definitely limited section of a chromosome. But, if there are all degrees of variation from entire to deeply serrate, the existence of a series of units in the germ-plasm, one for each depth of serrateness, is not obviously suggested. The series of fluctuating units has a unity to the human mind quite as natural as any one of the particular grades of serrateness. This is evidently felt vaguely by those who assume modifying factors, and factors of fluctuating potency. *To assume, however, that we have explained anything or in any way contributed to clear up our knowledge of germ-plasm or heredity* by saying that the fluctuating behaviour of visible characters is explained by modifying factors in the germ-plasm is a preformationism, which would have put even Bonnet to the blush.

"Perhaps the most obvious weakness of all those theories is that they carry in them the vices of the old preformationism. They seem too much like attempts to explain visible and familiar complexity by the assumption of a parallel complexity in the germ-plasm, and this, in spite of the admitted incommensurability of cell-organisation and metaphyte organisation. Driesch, with all his tendency to mysticism, must be credited with having recognised and made clear that the facts of nuclear and cell-division, and the resulting perpetuation of the hereditary complex, made it impossible to assume a

spatial configuration of the germ-plasm in three dimensions, parallel to that of the many-celled organism as a whole."

It seems to me that the consideration of facts such as those which Prof. Harper has set forth in such clear language in the preceding paragraphs leads inevitably to the conclusion that the attempt to find in the divergence of the mutants from type any indication of the elements of which chromatin is built up is a perfectly hopeless one; that Mr. Huxley's monstrous conception of the chromosome as an edifice of genes, each rigidly determined in location with regard to its neighbour, so that the whole is a vast super-molecule, is a vain chimæra, and that we are, in fact, within measurable distance of a time when the whole conception of qualitative factors will be given up, and replaced by a quantitative one—of precisely that "more or less" type, which Mendel expressly excluded from his view when he began his investigations.

I now turn to the second point in which my views diverge from those of Mr. Huxley, viz. the question of the possibility of the inheritance of acquired characters. As evidence in support of my belief in the possibility of this kind of inheritance, I adduced the work of Guyer and of Kammerer. Mr. Huxley endeavours to explain away Guyer's results—this attempt I have dealt with in the October number of the journal—and I shall only add now that Guyer's own view is that the transference of the defective lens from one generation to another is effected by the emission of a hormone *from the lens itself*, which affects the germ-cell, and this, as I have already pointed out, is Lamarckism.

Mr. Huxley passes over Kammerer's experiments with the remark that the results were obtained by Kammerer alone, in a word that the experiments have not been repeated. Mr. Huxley evidently realises that if Kammerer's results should be confirmed *cadit quæstio*, as far as Lamarckism is concerned, a conclusion in which all who are acquainted with these results will agree with him.

I think I can explain to Mr. Huxley the reason why Kammerer's results have not been repeated. Some fifteen years ago I repeated Cuénot's results in crossing wild and albino mice. I easily obtained his results, and I discovered that, given certain conditions (which in my case meant the assistance of a skilled assistant who had been a rat-catcher), nothing was simpler than to perform Mendelian experiments. It is a great deal easier to breed flies than to breed mice, and, without wishing to detract from the value of the work done by Morgan and his assistants, its "most laborious character" to which Mr. Huxley feelingly alludes, is "a feather's weight, which a child's hand poises, compared to the pig-of-lead-like pressure" of the difficulty of carrying out experiments such as Kammerer describes. Again, I speak from experience.

It is, therefore, hardly "playing the game" on the part of Mendelians to endeavour to discredit Kammerer's results by criticising his drawings and insinuating doubts as to his *bona fides*, when they will not go to the labour of repeating his experiments.

I have pointed out in a previous communication how utterly unlike Mendelian mutations are the differences between natural species of animals and plants. I shall add only this: when, by the aid of palæontology, embryology, and comparative anatomy, we are enabled to determine evolutionary series, the great feature of all these is progressive increase or decrease of function, exactly the feature which we should expect to find, if reaction to environment, and the inheritance of its effects, were the driving force behind them; and it is inheritance of acquired characters, which can alone explain recapitulation — the reality of which screams at us at every turn when we investigate the life-histories of animals. Recapitulation fits in so badly with the gene-chromosome hypothesis that ardent Mendelians like Morgan

and Johannsen take the step of denying its existence. If the facts refuse to fit into their scheme, so much the worse for the facts.

It is perfectly true that it is difficult, and often impossible, to show how the distinctions between two allied species are related to function. This is because—to steal Mr. Huxley's fire—the activities of animals are "balanced reactions," and, until we succeed in analysing the physiology of animals, much better than we have done, we cannot hope to show why the alteration of one function influences others. We do not know why, when our race migrates to Australia, their children tend to give rise to taller men—it certainly is not a case of natural selection nor chance transportation of particular genes to that continent.

Having constructed the imaginary edifice of genes, the next step would be to show how it functions in producing adult structure. From taking this step Morgan wisely abstains, but where he fears to tread Huxley rushes in. In answer to my query as to how the cells of the abdomen of a frog tadpole acquired the capacity of forming lenses, he states that all the cells of the body have this power because all the nuclei of the body possess the whole gene-complex—or, as I would put it, all the potentialities of the species. In this assumption I agree with Mr. Huxley—he will find it concisely stated in my textbook of *Embryology*, published seven years ago, but it involves the entire giving up of the Weismannian distinction between germ-plasm and somato-plasm. But when Mr. Huxley goes on to state that this assumption renders it easy to understand the building up of the adult body, and processes like regeneration and reproduction, one is driven to ask whether he imagines himself to be throwing any light on the problems which he discusses. The gene-complex "reacts" differently to different media, and so in one place a head is produced, and in another a tail. Regeneration is of the same character as the completion of a truncated crystal!! Really, Mr. Huxley might be invited to restudy the works of Driesch. Does he forget that the supposed different media to which the gene-complex reacts are created by itself, or that the growth of a crystal is due to the monotonous linking together of the same kind of atoms?

The cytoplasm of the young Nematode egg is indifferent material; as it ripens it has an elaborate organisation bestowed on it by emissions from the nucleus, so that different cytoplasmic substances are arranged in definite relations to one another. To what different media has the gene-complex "reacted" in this case—and is the phrase "reaction to stimulus" a physico-chemical conception at all?

In conclusion, I should like to comment particularly on two paragraphs of Mr. Huxley's article. He presumes to judge of the mental prejudices which prevent his opponents from accepting the views of Morgan, and suggests it is because they dislike the idea of chance variations as the raw material of evolution, and "refuse to accept the universe." I pass over the questionable taste of a young and comparatively untried biologist like Mr. Huxley attributing motives to senior biologists, amongst whom must be reckoned the distinguished botanist Sir Francis Darwin—who made the inheritance of acquired characters the subject of his presidential address to the British Association at its Dublin meeting in 1908. I beg to assure him that it is not because they have prejudices that his opponents fail to agree with him: it is because they know considerably more of the universe than he does, and find the Morgan-hypothesis inconsistent with the facts.

Then, in the opening sentences of his essay, he indicates that he is putting forward a new theory, and trusts to time to enable it to gain the victory over the older one. Nothing could be a more complete travesty of the facts. I well remember the enthusiasm with which the Mendelian theory was received, when it was introduced to the scientific world in the early years of this century. We thought that at last the key to evolution had been discovered. As a

leading Mendelian put it, whilst the rest of us had been held up by an apparently impenetrable hedge, viz. the difficulty of explaining the origin of variation, Mendel had unnoticed cut a way through. But, as our knowledge of the facts grew, the difficulty of using Mendelian phenomena to explain evolution became apparent, and this early hope sickened and died. The way which Mendel cut was seen to lead into a *cul-de-sac*. It is because I have reason to hope that at last a real way through has been discovered that I wrote this essay.

E. W. MacBride.

November 8, 1921.

To the Editor of "Science Progress"

MR. BALFOUR AND THE ENCOURAGEMENT OF MEDICAL RESEARCH[1]

From Colonel W. G. King, C.I.E., Ind. Med. Ser. (ret.)

Sir,—In reply to a recent question on the subject of awards by Government for scientific research workers whose results have been in an unusual degree of wide public benefit, Mr. Balfour deprecated any special action in the case of Medical Science. He held that the dole system of the Civil List sufficiently met requirements of all branches of science. The sanction of special awards (such, for example, as the sum of £30,000 allowed by Parliament in the case of Jenner) would, he contended, present the "overwhelming" difficulty of determining which of the various workers who had previously secured data of utility in solving a specific problem had the merit of being primarily decisive or merely adjuvant.

If, in grouping sciences generally with Medicine, Mr. Balfour had before him factors influencing their constitution or methods (physical and mental) of production which were reasonably on a par, his argument for equality of treatment would necessarily receive acceptance; but he failed to take cognisance of an important factor in connection with Medical Science which renders his comparison incorrect and his decision inequitable. In dealing with the medical profession, he has forgotten it is moulded on an ancient caste system as rigid as any practised in the East, is governed in ethics by the Medical Council as ruthlessly as by a *Punchayet*, and that the Hippocratic oath still throws its shadow on certain of its rulings. These ethics have no taint of trade-unionism, in seeking to make the public a milch-cow; on the opposite, beyond dictates of inter-professional courtesy, they are strictly in the interests of the public.

Putting aside the open question of whether, in the present day, certain rulings of the profession cannot be legally defied, it is a fact that any form of concealment of medical knowledge gained by a member of this peculiar caste and appropriated for self-interests is "taboo." Consequently, the Medicine Man is precluded from exploiting for monetary benefits, by means of a patent, a discovery, for which he may have toiled for years and have expended ill-spared private funds.

It is evident that this ethical ruling is against the monetary interests of the individual, and, in all but enthusiasts, is liable to inhibit research—a considerably more potent factor thereto than the incitement of jealousy which Mr. Balfour professed he dreaded! Yet it is enormously operative in the cause of the finer instincts of humanity, and, where labour and health-saving are brought about, indirectly the public purse is fed. Were it otherwise, in times of epidemics and wars, might not the doctors aspire to the sublime rank of profiteers? Small royalties on new-fangled but potent drugs, surgical instruments, and equipment, and on far-reaching organic products combining results of chemical and bacteriological skill might well bring in

[1] See Science Progress, No. 62, pages 286 and 298.

fortunes to individuals. To take an instance in point: when the Panama Canal was about to be undertaken by America, with the full knowledge that the French had incurred a gruesome failure owing to mortality from malaria, had the work of Ross not been made public property, an interview at Washington by a business agent to the effect that he could supply information by which this deadly malaria could be overcome would have elicited instant attention. The agent could have struck a bargain that, were his method employed by an intelligent man (such as subsequent history showed Gorgas eminently was), he would guarantee malaria would be practically extirpated, at a cost for sanitation of one cent per head of the population per day. It is tolerably certain the American Government would not have objected to promising him one cent per day extra per head—free of supertax—if he proved his case in practice. The bargain would have left the American Government the gainer on completion of the Canal, according to Gorgas, in consequence of sanitary work effected on the Isthmus during the ten years of construction, of a sum which " will not be considered an exaggerated estimate, of eighty million dollars."

This would have left a bonus from which an award might have been given to the remnants of the gallant Reed Commission of the United States Army, which, at the cost of two lives of its members, had placed at disposal the method of yellow fever prevention, and thus contributed to their country's success. Comparing the work under the French, who had not the advantage of knowledge gained by Ross as to malaria or by the Reed Commission as to yellow fever, Gorgas has further claimed that there resulted during the ten years a saving of 71,370 human lives. Similar measures conducted less strenuously have placed West Africa in a position to refute the stigma of being the "white man's grave."

That research workers in other sciences are not thus restrained from obtaining patents to cover their discoveries is self-evident from a ruling which appears in *Notes on the Conditions under which Grants are made to Individuals and Research Workers and Students in Training*, issued by the Committee of the Privy Council for Scientific and Industrial Research in 1920.[1]

Irrespective of the fact that there is no bashfulness exhibited as to securing " any benefits and profits arising " from protection by patent, this ruling shows how Mr. Balfour's " overwhelming difficulty " might well have been solved by referring any question of disputed merits for award to a committee of medical experts. Indeed, it would be well, in view of Mr. Balfour's dictum—if that be final—that the medical profession should free itself of the ethical burden as to patents by giving permission in special cases to members to secure patents, on the approval of a sub-committee of the Medical Council appointed permanently for that purpose.

It is possible Mr. Balfour was influenced in arriving at his decision by his placing an undue value on another of the peculiarities of the medical profession. In days when bacteriology and microscopy were not empirical remedies and hypotheses as to disease causation received universal attention if a theory were the offspring of a leading member of the profession it was received with due reverence; when a lucky hit was made the man concerned was exalted in professional estimation. Remnants of this appreciation are still evident. Hypotheses, however, in the present day, count for little when balanced

[1] Para. 20 (ii).—The Committee of Council reserve the right to determine, after consultation with the bodies and persons who have co-operated in the conduct and maintenance of the investigation, whether, and, if so, to what extent and in what proportions the Committee of Council and those bodies and persons (including the worker) shall secure to themselves by patent, designs, or otherwise, the ownership of the results of the investigation, and any benefits and profits arising therefrom."

against accomplishment in the less conservative branches of science. Certainly this is the case in War Office awards, when confronted with the question of ultra-brainy schemes for campaigns in the late war, and in the Department of Government dealing with discoveries aiding the efficiency of war material. As an instance of awards under the latter head, official records show that for a patent bullet £7,000 were paid, for a silvering process of mirrors £2,500. Were equally practical methods of discrimination used, Mr. Balfour would find that, whilst the medical profession has still room for honouring the producer of a *successful* hypothesis, it gives the palm of discoverer to the man who, by his discrimination and toil, amidst the many factors influencing man and his environment, demonstrates the facts. Were it not so, there could exist no *raison d'être* for the existence of the Society for Defence of Research, which includes several thousand medical men on its strength. This appreciation of facts gained by research, as contrasted with mere hypotheses, was well exemplified in the case of Jenner, who, when a house-pupil of the famous John Hunter, "often discussed the subject of small-pox with the great anatomist, and, on one occasion, when relating his hopes and fears of the possibility of substituting vaccination for inoculation, the characteristic reply of the great surgeon was: "Don't *think*, Jenner, but *try*."[1]

Yours faithfully,

W. G. KING.

LONDON,

November 14, 1921.

[1] *The History of Inoculation and Vaccination*, XVIIth International Congress of Medicine, p. 63; issued by Borroughs Wellcome & Co., 1913.

ESSAYS

SKETCH OF A LOGICAL DIVISION OF CHEMISTRY

(Prof. Ingo W. D. Hackh, A.B., Ph.C., College of Surgeons, San Francisco, U.S.A.).

INCREASE in knowledge and the rapid unfoldment of natural laws renders specialisation more and more necessary. Chemistry to-day permeates every natural science, and is, as the science of matter, the corner-stone of all knowledge dealing with matter from the macrocosmos to the microcosmos, astronomy to biology. Therefore the growth of chemical knowledge renders it necessary to divide chemistry into many branches, a process which has been going on especially during the last two decades. A uniform classification is difficult, however, for the various factors and phenomena are not always clearly understood, and not distinctly different in kind, but differ mainly in degree.

A classification, above all, must be practical, and it seems best to divide, as far as possible, into three main branches:

I. Theoretical Chemistry.
II. Practical Chemistry.
III. Applied Chemistry.

These three branches represent, in general, the application of the maxim—study every natural phenomena as to cause, appearance, and effect. Thus:

Theoretical Chemistry is the study of the laws that govern chemical changes—the doctrine of cause.

Practical Chemistry is the observation and description of the phenomena and chemical individuals, as well as a study of the composition of things—the doctrine of appearance.

Applied Chemistry is the utilisation of chemical knowledge for the welfare of mankind, and may be termed the doctrine of effect.

There are gradual transitions everywhere in life and nature, and nowhere an abrupt change without connecting link. Between one extreme and the other there are always found connecting tendencies, sometimes clearly recognised, sometimes hard to distinguish. Sharp and clear-cut boundary lines are not to be drawn in nature. Hence, the classification outlined centres around principal factors; everywhere are connecting links to other centres, thus forming a complicated spacial structure interwoven with many connections. The limits of a single branch are not so distinct and sharp as the classification would let it appear. The table represents this classification on a plane surface in which the connections and relations of the different branches cannot be fully shown.

I. THEORETICAL CHEMISTRY

The cause of every chemical phenomena appears to be the action of chemical force in relation with physical energy. Omitting a discussion of the nature of this force and energy, and without speculation on the structure of the atom, there are the two factors: chemical force and physical energy. Accordingly, the subdivision of pure chemistry and physical chemistry.

A SCHEMA OF A LOGICAL DIVISION OF CHEMISTRY

CHEMISTRY (Science of Matter)

I. Theoretical Chemistry (Doctrine of cause)	IA. PURE CHEMISTRY (Chemical force)	1. STOICHIOMETRY (atom) 2. RADIOCHEMISTRY (electron) 3. STEREOCHEMISTRY (molecule)
	IB. PHYSICAL CHEMISTRY (Chemical force and physical energy)	4. MECHANOCHEMISTRY 5. THERMOCHEMISTRY 6. ELECTROCHEMISTRY 7. PHOTOCHEMISTRY
II. Practical Chemistry (Doctrine of appearance)	IIA. OBJECTIVE (Systematic description of compounds)	8. INORGANIC CHEMISTRY 9. ORGANIC CHEMISTRY 10. ANALYTICAL CHEMISTRY
	IIB. SUBJECTIVE (Composition of nature)	11. ASTROCHEMISTRY 12. GEOCHEMISTRY 13. BIOCHEMISTRY
III. Applied Chemistry (Doctrine of effect)	IIIA. DIRECT (Direct application to man's benefit)	14. SANITARY CHEMISTRY 15. PHARMACEUTICAL CHEMISTRY 16. MEDICAL CHEMISTRY 17. NUTRITIVE CHEMISTRY 18. FERMENTATION CHEMISTRY 19. AGRICULTURAL CHEMISTRY
	IIIB. INDIRECT (Indirect application to man's benefit)	20. TECHNICAL CHEMISTRY 21. INDUSTRIAL CHEMISTRY 22. COMMERCIAL CHEMISTRY 23. ENGINEERING CHEMISTRY 24. MANUFACTURING CHEMISTRY

IA. **Pure chemistry** pertains to chemical force as far as this force can arbitrarily be designated, and would therefore include:

1. STOICHIOMETRY, which investigates the amount of chemical force as far as it expresses itself in the doctrine of definite or multiple proportions. Under this heading comes atomic weight, valency, affinity, chemical equilibrium due to concentration, and the speed of reactions caused by concentration and not due to thermal, or electric causes.

2. RADIOCHEMISTRY, which takes up subatomic phenomena and studies the laws of disintegration and transmutation. It deals with intrinsic chemical force and the structure of the atom. It is the ultimate meeting ground of physics and chemistry in which energy and matter appear to become synonymous.

3. STEREOCHEMISTRY, which considers the spacial arrangement of the atoms in the molecule. It includes not only the asymmetric atoms of C, S, Se, Sn, Si, and the stereoisomers of N, Co, Pt, Cr, etc., but also the allotropic forms of elements and the structure of crystals.

IB. **Physical chemistry.**—To this branch belong the phenomena resulting from an interaction of chemical force and energy. Accordingly, the different manifestations of energy may be termed mechanical, thermal, electrical, and radiant energy. Hence there are:

4. MECHANOCHEMISTRY, dealing with the mechanical properties of substances: density, surface tension, pressure, compressibility, conductivity of sound, mechanical strength and resistance.

5. THERMOCHEMISTRY treats of thermal phenomena associated with chemical change (thermodynamics) and includes phenomena such as heat of formation, heat of combustion, heat of neutralisation, as well as the melting and boiling points, expansion by heat, conductivity of heat, etc.

6. ELECTROCHEMISTRY considers the electrical and magnetical properties of matter such as electro-motive force, ionisation, electro-affinity, magnetic permeability, resistance, and conductivity.

7. PHOTOCHEMISTRY deals with light and its relation to substances and chemical changes: *e.g.* photo-activity, luminescence, refraction and dispersion, absorption and colours, spectroscopy.

II. Practical Chemistry

The scope of practical chemistry is the study of the appearance of matter and its interrelations. It is twofold—objective and subjective.

The objective part includes not only the systematical description of chemical elements and compounds, but also a study of their reactions and recognition as well as the technical operations of the chemist. The old and well-known classification seems practical and effective, hence:

8. Inorganic Chemistry, or the chemistry of polar compounds generally, treats of the elements and compounds not containing carbon.

9. Organic Chemistry, or the chemistry of non-polar compounds, deals mainly with the thousands of carbon compounds.

10. Analytical Chemistry, or the chemistry of reactions, includes the methods and operations as well as the apparatus and instruments for qualitative and quantitative analysis. Its purpose is the recognition, detection, and determination of the composition of matter—in short, the main part of professional chemistry.

The subjective part of practical chemistry describes the material structure of the universe, that is, the chemical composition and changes in nature. Accordingly, it is divided into:

11. Astrochemistry, which explores the chemical structure of the cosmos, and, with the help of photochemical methods, discovers the constituents of celestial bodies. It glimpses into the great cycle of world-creation and world-disaster—a transformation requiring eons—and hints at a cosmical evolution from latent nebulæ into radiant suns and their gradual decline.

12. Geochemistry, which investigates the composition of our planet and teaches the changes occurring in the atmosphere, hydrosphere, and lithosphere. Air, oceans, and rocks are its kingdom, and it studies their transformation, decay, and formation, as well as the great cycle of water, the pulsating carrier, diffuser, and concentrater of matter.

13. Biochemistry, which investigates the chemical secrets of life and inquires into the functions and actions (physiological), as well as the structure (morphological) of cells in their healthy or diseased (pathological) condition. The chemical dynamics of life phenomena considers not only the protoplasm and cell community of plant, animal, or man as a chemical mechanism (metabolism), but also its material relation to the surrounding nature, *e.g.* the carbon cycle, nitrogen cycle, phosphorus cycle, etc.

III. Applied Chemistry

The ultimate purpose of all science is the progress of mankind, be it either for material or spiritual improvement. Hence, the more or less apparent purpose of applied chemistry is to serve the welfare of man. This application, of course, is either IIIa—direct, or IIIb—indirect.

The direct application of chemistry can be grouped under:

14. Sanitary Chemistry, including the supervision of water-supply, sewage, sanitation, waste disposal, and other hygienic necessities.

15. Pharmaceutical Chemistry, dealing with the composition, manufacturing, and testing of remedies, while Therapeutical Chemistry studies the application, dosage, and effect of remedies.

16. Medical Chemistry, or clinical chemistry, employs analytical methods for diagnostic purposes, whether it be for the detection of disease or crime.

17. Food Chemistry, or chemistry of nutrition, examines and controls the production and preparation of food products, and includes the methods of detecting adulterants and preservation.

18. FERMENTATION CHEMISTRY includes the brewing and malting of beverages, vinegars, the manufacture of industrial alcohol, yeast, and certain colouring matters.

19. AGRICULTURAL CHEMISTRY, in its narrow sense, deals with the chemistry of soils, fertilisers, manures, etc., and has as its purpose the improvement of land values and the increase of crops.

The indirect application of chemistry includes the many branches of industrial and manufacturing chemistry as well as its use in professions, trades, and arts. They may be grouped in various ways, either as to the source of the raw material (*e.g.* of vegetable, animal, or mineral source), or the respective industries, or the respective purposes. The following is an attempt to combine some of these factors.

20. TECHNICAL CHEMISTRY, or the application in technic, *e.g.* (*a*) photographical (not photo-chemistry); (*b*) pyrotechnical (explosives, matches, fireworks); (*c*) ceramical (pottery, vases, and their metallic colours); (*d*) laundering (cleaning, bleaching, washing, and starching).

21. INDUSTRIAL CHEMISTRY, or the application in industry, *e.g.* (*a*) textile (fabrics and dyes); (*b*) leather (tanning materials); (*c*) paper (fibres, cellulose, etc.); (*d*) rubber and allied substances.

22. ENGINEERING CHEMISTRY, or the application in engineering, *e.g.* (*a*) metallurgy (iron, steel, alloys, and heavy metals); (*b*) building materials (cement, mortar, lime-bricks); (*c*) fuel (gas, tar, coke, petroleum, asphalts).

23. COMMERCIAL CHEMISTRY, or the application in commerce, *e.g.* (*a*) perfumery (essential oils, flavouring materials, fruit essences); (*b*) soaps, washing powders, candles; (*c*) paints, inks, stains; (*d*) varnishes and shellacs, etc.

24. MANUFACTURING CHEMISTRY, or the manufacture from raw materials: (*a*) acids, alkalies, and salts; (*b*) fats, oils, and waxes; (*c*) silicates (glass and porcelain); (*d*) carbohydrates (sugars, dextrin, starch, etc.); (*e*) proteins (casein, ferments, etc.); (*f*) resins (gums, oleo-resins, agar-agar); (*g*) glue and gelatine.

The classification outlined above is merely an attempt at a logical and uniform classification which enables a clearer definition of certain chemical branches. For instance: photo-chemistry and photographical chemistry are distinctly different, yet some dictionaries consider them synonymous. The enumeration of the many branches is also an index of the extensive proportion which chemical knowledge has acquired in human affairs.

"UNOFFICIAL" MOSQUITO CONTROL IN ENGLAND (John F. Marshall, M.A.).

"*So far as my experience goes, it has been demonstrated that mosquitoes can be as completely exterminated in any locality as dirt can be swept from a building or as weeds from a walk, . . . and with the exercise of no more intelligence and much less labour than is required in the performance of many domestic duties. My experience would lead me to conclude that, if mosquitoes continue to exist in any locality, it is because the people are too indifferent to the annoyance to take the trouble to be rid of it.*"—Extract from a speech delivered by Mr. W. J. Matheson at the First Anti-Mosquito Convention in New York, December 16, 1903.

As most people nowadays are aware, a mosquito, or "gnat," is a two-winged fly, easily recognised by its long "beak," or "proboscis." This proboscis is actually a sheath containing a formidable collection of pointed instruments, with which the female mosquito pierces the skin of men and animals, and rapidly sucks their blood.

Mosquitoes were not really taken seriously until the year 1897, when Ross

announced his epoch-making discovery that the parasite causing malarial fever could, and could only, be transmitted from one person to another by the successive bites of the same mosquito. It was subsequently shown that many kinds of mosquitoes were capable of acting as "malaria carriers," but that all those possessing this undesirable gift belonged to one particular genus, namely the *Anopheles*.[1]

As soon as the above facts became public property, it was correctly surmised that districts in which malarial fever was prevalent might be made healthy by a wholesale destruction of the mosquitoes responsible. As is well known, the larval and pupal stages of the mosquito are passed in water, the duration of the former being something like two or three weeks, and that of the latter about as many days. The larva is a well-known "wriggly" creature, generally to be observed hanging head downwards with its tail-like breathing-tube just breaking the water surface. Mosquito larvæ are very easily destroyed, either by mixing some poisonous substance with the water, or by covering its surface with a film of oil, which prevents the larvæ from breathing, and causes them to drown. Of the two methods, the latter is usually much the cheaper, and thus we find that oil-spraying, assisted as far as possible by drainage operations, forms the basis of practically all the anti-mosquito schemes now being worked in various parts of the world.

The earlier mosquito campaigns were naturally confined to districts in which malaria was prevalent, and the successful operation of numerous schemes, extending over large tracts of country and supported in many cases by official funds and by special legislation, fully demonstrated the practicability of abolishing mosquitoes (and, in consequence, mosquito-carried disease) from any locality desired.

The defeat of the *Anopheles* mosquitoes gave rise to the suggestion that anti-mosquito measures might with advantage be adopted in towns and villages to minimise the often intolerable nuisance due to the prevalence of "harmless" mosquitoes (as the many "non-malarial" kinds are popularly, though very erroneously called), with the result that a great number of "unofficial," schemes, depending largely upon voluntary subscriptions and upon voluntary work, are in existence (especially in the United States) at the present time.

We may, then, roughly separate anti-mosquito organisations into the two following classes:

(I) The *Official* type, operating in a district where a mosquito-conveyed disease and the appropriate mosquitoes co-exist, and usually aided by official funds, labour, legislation, etc.

(II) The *Unofficial* type, operating in a district for the purpose of minimising the nuisance due to the prevalence of mosquitoes of all kinds, and unassisted (at any rate in the early stages) by municipal funds, labour, by-laws, etc.

. . . . .

There is no question that the subject of mosquito control is much better understood abroad than it is in this country. In England, indeed, the opinion of "the man in the street" is almost invariably based upon three distinct fallacies, namely, that English mosquitoes (i) are all "non-malarial" ones, and (ii) are perfectly harmless, though admittedly a source of (iii) unavoidable annoyance. These three fallacies must be dealt with separately.

(i) Of the total of twenty-five known British mosquitoes, three are malaria-carriers of the *Anopheles* genus, two of them being distributed freely all over England. During the war, the frequent co-existence in military areas of malaria "cases" and *Anopheles* mosquitoes led to many epidemics of

[1] In the year 1900 a precisely similar connection was proved to exist between the disease of yellow fever and a particular mosquito, *Stegomyia fasciata*.

fever. These epidemics were, however, speedily suppressed owing to the fact that, under war conditions, the necessary measures of mosquito control could be officially enforced as well as officially financed.[1]

(ii) The prevalent fallacy regarding the twenty-two "non-malarial" English mosquitoes as "perfectly harmless" is somewhat surprising, in view of the fact that throughout every summer and autumn various forms of "blood-poisoning" admittedly arising (however indirectly) from the bites of these insects are of constant occurrence in every town and village in the country. The following statement by Professor Lefroy, of the Imperial College of Science and Technology, South Kensington, an eminent authority on entomology, was published in *The Times* on September 9, 1921:

"During this year there have been quite an abnormal number of deaths from either wasp-stings or the bites of the brown mosquito (which does not convey malaria), or from the bites of flies which have not been identified. To some extent this has been due to the fact that the heat and dry conditions have led these insects to attack man more freely; but we are ignorant of the reason why the bite of a harmless brown mosquito can cause death within two or three days, when we know that no specific disease such as malaria has been introduced. It is extraordinary that coroners should pass verdicts of "Accidental Death" in such cases without emphasising the need of investigation into the actual cause of death when a healthy person has been bitten by such an insect as a common brown gnat or mosquito. This is a subject which undoubtedly requires investigation, and yet is tacitly accepted as the cause of death in quite frequent cases."

(iii) To demonstrate the inaccuracy of the third "popular belief" (that it is impossible to minimise the annoyance caused by mosquitoes in a given district) a mass of conclusive evidence has been collected from all parts of the world. It is to be hoped that amongst this evidence a place may eventually be found for the results achieved by the Hayling Mosquito Control, the first "unofficial" organisation of the kind in England, a brief description of which is given below.

. . . . .

The formation of the Hayling Island scheme was not definitely resolved upon until the autumn of 1920 (a more than usually bad "mosquito year" both in England and on the Continent), when it was too late to attempt anything in the way of active anti-mosquito operations. During the month of September, however, a large number of mosquitoes were caught, and their subsequent identification (by means of Dr. Lang's admirable *Handbook of British Mosquitoes*) showed them to be almost entirely of three "non-malarial" kinds, namely: (1) the common brown "domestic" mosquito, *Culex pipiens*; (2) the large "banded-legged" mosquito, *Theobaldia annulata*; and (3) the "salt marsh" mosquito, *Ochlerotatus detritus*.[2] That these were the prevalent species of the district was confirmed by the observations of the following year.

The mosquito *Ochlerotatus detritus* (which is extremely common all round the English coast) is generally described as breeding in "brackish" water. This, however, seems to be a rather mild way of stating the case, seeing that the writer has frequently found the larvæ in enormous numbers, not only in stagnant sea-water of "full strength," but also in sea-water which, through evaporation, has been found (by titration with silver nitrate) to be of super-

[1] See Local Government Board publications, *Reports and Papers on Malaria Contracted in England in* 1917 (published 1918), and *Reports and Papers on Malaria Contracted in England in* 1918 (published in 1919).

[2] The prevalence of the last-named mosquito had been predicted by the well-known authority Mr. F. W. Edwards, of the British Museum (Natural History), to whom the writer is indebted for very much advice and help.

normal salinity. The larvæ will also hatch out quite successfully after being transferred into fresh water at an early age.

It was not possible to organise any extensive searches for hibernating females during the winter 1920–21, but a certain number of *Culex* and *Theobaldia* were discovered and destroyed by fumigation.

Early in 1921 the Island was divided up into eighteen administrative sections, each one being in charge of a " section secretary," and a circular explaining the objects of the campaign was drawn up and sent to every householder. The circular drew attention to the habits and life-history of mosquitoes, to the methods of destroying their larvæ, and to the success achieved by anti-mosquito organisations abroad; concluding with a summary of the various ways in which assistance was required. Each circular was accompanied by a stamped envelope bearing the address of the appropriate section secretary and by a " reply card " worded as follows :

" *I approve of the objects of the Hayling Mosquito Control, and I hope to assist in one or more of the ways mentioned below :*

" (1) *By carrying out as far as possible such instructions as may be supplied to me from time to time for preventing mosquitoes breeding in my house, garden, etc.*

" (2) *By reporting to the secretary of my section the location of any pond, pool, ditch, water-tank, etc., in which I observe any larvæ (' wrigglers ').*

" (3) *By organising or taking part in field expeditions for seeking out ' breeding places ' and dealing with them when found.*

" (4) *By collecting mosquitoes and mosquito larvæ in accordance with directions to be supplied to me.*

" (5) *By giving a donation of £ s. d.*

" *You may therefore enter my name on the List of Members.*"

. . . . .

Instructions were printed on the above cards requesting the recipients to indicate by crosses the ways in which they expected to help. In spite of the novel character of the scheme, the response elicited by the circular was extremely encouraging.

A systematic " charting " of all the ponds, ditches, marshes, and other " water places "[1] on the Island was at once commenced, the particulars of each case being given a " register number," and entered on a separate page of the " Control Record " book of the Section (Fig. 1), the lower part of the page being reserved for the entry of details referring to future observations, larvicidal treatments, etc. Any larvæ found during the preliminary inspection were, of course, destroyed.

As each water-place was thus registered, its representation on a large-scale wall-map was coloured blue and connected by a red indicating line with the outer of two concentric circles ($\frac{5}{8}$ and $\frac{5}{16}$ inches diam.) drawn on the map, the particular register number being inscribed within the smaller circle. The space between the circles was coloured red, to direct attention to the possible danger of larvæ being present whenever there was no definite information to the contrary.

Since the completion of the registration, the progress of the work throughout the Island has been continuously indicated on the Control Map by

[1] Writers on this subject are frequently at a loss for some general term to describe " anything in which water has collected or is liable to collect." Such expressions as " water receptacle " or " water container " nearly meet the case, but do not sound correct if applied with reference to a ditch or marsh. " Water-lodge " has been proposed, and seems worthy of consideration. In this article the term " water-place " has been tentatively used, chiefly in the hope of eliciting some better suggestion.

covering up the red circle belonging to any particular water-place with a perforated disc or "washer" varying in colour according to the information received. Thus, a *white* covering washer indicates that the corresponding water-place is *dry*; a *black* washer, that the water has been *found free from larvæ for no apparent reason*; a *yellow* washer, that *larvæ are absent and that some of their natural enemies, such as larva-eating fish, are present*; a *green* washer, that *paraffining operations have been carried out* (Fig. 2).

Whenever a water-place is paraffined, its register number is entered in a special almanac, *under a date three weeks ahead.* The almanac is inspected every day and the green washers surrounding those register numbers which appear under the current date are removed from the map. The water-place is then shown as "dangerous" again until further information is forthcoming.[1]

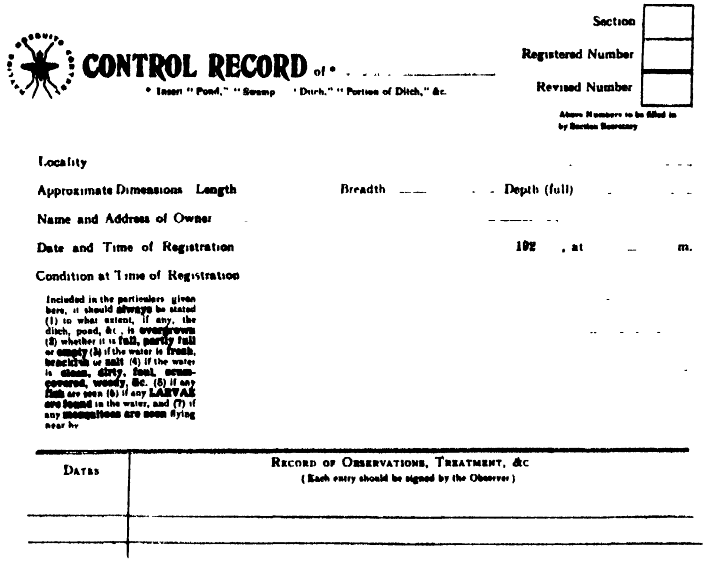
. . . . .

Anti-mosquito measures may be summarised under the two headings of "prevention" and "cure," and the methods available were fully described in the general circular. It was first pointed out that the breeding of mosquitoes in the proximity of a dwelling-house could be prevented altogether by abolishing collections of standing water and by covering over water-butts and tanks in which mosquito eggs might otherwise be laid. Attention was then drawn to the ease with which larvæ, if found, could be destroyed, either by throwing the water away on the ground or by pouring a spoonful of paraffin on its surface. It was also pointed out that, since the paraffin did not mix with the water, the latter could be drawn off unchanged as required, either by a siphon or by a spigot inserted in the lower part of the butt.

Turning now to what may be called "outside" operations, these are again separable into the categories of prevention (chiefly exemplified by drainage) and cure, namely, destruction of larvæ. In cases like the present, draining to any appreciable extent is out of the question owing to the financial limitations of a newly-formed organisation, and attention must necessarily be limited to the question of larvicides. Here, again, expense is the deciding factor, since the cost of the larvicidal operations must be reduced to the minimum figure consistent with success. Whatever be the larvicide selected for the work, it must obviously be used with the greatest possible economy; in other words, *water should never be treated with the larvicide unless the presence of larvæ has been actually observed.* The application of this principle at Hayling, by means of the system of "inspecting and recording" described above, has proved very successful, and it is probably not too much to say that it is of fundamental importance in all cases where expense has to be considered.

As previously mentioned, larvæ may either be suffocated by a film of oil or poisoned by diffusing some suitable substance in the water. In the first method, as might be expected, many varieties and grades of oil have been successfully employed, the usual practice being to select some kind which is both cheap and easy to obtain in the district concerned. In England,

[1] The control map actually in use is built up of the requisite sections of the $\frac{1}{2500}$ scale Ordnance Survey (25·344 inches to the mile) which is published in sheets measuring about 38 by 25 inches. The separate sheets are mounted on millboard $\frac{1}{8}$-inch thick, stiffened at the back by horizontal wooden strips which are grooved in such a manner that any number of sections may be rapidly "assembled" upon a light vertical framework attached to the wall. For "covering up," thin steel washers (easily obtainable of the dimensions quoted) are painted and suspended on minute brass "L" hooks screwed into the map at the topmost points of the "register number" circles. The washers are very conveniently picked up and placed in position by means of a magnet.



CONTROL RECORD of * ....

* Insert "Pond," "Swamp," "Ditch," "Portion of Ditch," &c.

Section
Registered Number
Revised Number

Above Numbers to be filled in by Section Secretary

Locality

Approximate Dimensions Length Breadth Depth (full)

Name and Address of Owner

Date and Time of Registration 192 , at m.

Condition at Time of Registration

Included in the particulars given here, it should **always** be stated (1) to what extent, if any, the ditch, pond, &c., is **overgrown** (2) whether it is **full, partly full** or **empty** (3) if the water is **fresh, brackish** or **salt** (4) if the water is **clean, dirty, foul, scum-covered, weedy, &c.** (5) if any **fish** are seen (6) if any **LARVAE are found** in the water, and (7) if any **mosquitoes are seen** flying near by

DATES	RECORD OF OBSERVATIONS, TREATMENT, &C (Each entry should be signed by the Observer)

FIG. 1 Upper part of page in a Control Record book.

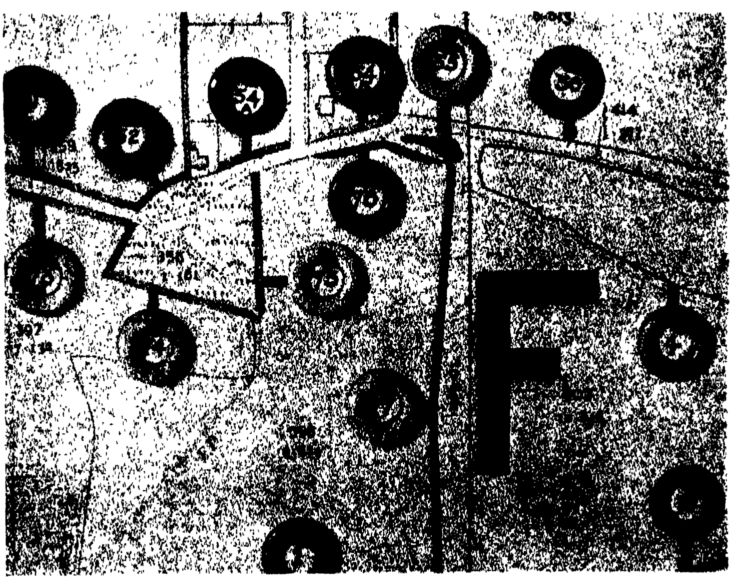


FIG. 2. - Portion of Control Map, indicating *empty* ditches.

No. 73E, 74, 75, 77 (*white* covering washers); *larva-free* ditches. Nos. 51C, 52, 54A, 54B (*black* washers), *paraffined* ditches. Nos. 1, 76 (*green* washers); pools *containing larva-eating fish*. Nos. 4, 55 (*yellow* washers), and an *unexamined* well. No. 56 (uncovered *red* circle). From the red circle of ditch No. 2, *paraffined three weeks previously*, a green washer has just been removed.

ordinary paraffin is probably the best oil to use; this, when sprayed on the water surface, forms a uniform and sufficiently lasting film.[1]

As regards larvicides of the "poisoning" class, a large number of substances have been recommended (chiefly by American writers), but in the majority of cases definite figures are difficult to obtain. In some instances where full particulars have been given experiments carried out at Hayling have demonstrated that either the advocates of the larva poisons in question are unduly optimistic, or that the resisting power of the average English larva is unusually high. As an example of this, a mixture of carbolic acid, caustic soda, and resin, compounded according to an American recipe, and alleged to be effective in the proportion of 1 in 10,000, was on one occasion added to the water of a larva-infested tank up to a strength of 1 in 4,000, with the result that the majority, if not all, of the larvæ were as active as ever eighteen hours later. Again, the addition of one part of sulphate of copper to 5,000 parts of water has been extensively recommended, but experiment shows that, when used in this proportion, it has no larvicidal effect whatever.

The use of larva poisons is obviously inadmissible whenever any likelihood exists of the water so treated being drunk by men or animals; and in all cases in which expense has to be considered, the superiority of the paraffining method is overwhelming, as the following considerations will show.

Owing to the scarcity of information regarding the approximate amount of oil required to treat a given area of water surface, experiments were made at Hayling early in 1921 to ascertain the expenditure of paraffin (as well as of time) involved in "filming" measured stretches of a larva-infested ditch of considerable length and of an average width of three feet. An ordinary pneumatic sprayer of the "one-handed" type was used, and the result of several trials showed, in round figures, that one pint of paraffin was sufficient to treat forty yards run of the ditch (*i.e.* to cover forty square yards of surface) in the average time of two minutes. These figures were confirmed on subsequent occasions, total destruction of the larvæ being observed in each case.

Now, since a pint is equal to 34·659 cubic inches, the result of distributing this amount of paraffin over a surface of 40 square yards is to produce a film having a thickness of

$$\frac{34 \cdot 659}{40 \times 9 \times 144} \text{ inches,}$$

which reduces to almost exactly $\frac{1}{1500}$ of an inch.

Still considering our forty square yards of surface, let us now suppose our pint of paraffin to be replaced by a pint of some "larva poison," and let us furthermore suppose the depth of the water to be one inch. Under these circumstances it is obvious that the poison would be mixed with the water in the proportion of 1 in 1,500, and if we suppose, for the sake of argument, that this were the correct dilution for the particular poison concerned, the larvæ would be destroyed just as well (though not necessarily at the same cost) as they would have been by the paraffin film. If, however, we were to repeat the process at a place where the depth, instead of being one inch, were

[1] The addition to the oil of small quantities of other liquids to increase its "spreading power" has been advocated, amyl alcohol and asphalte varnish being among a number of liquids recommended. Experiments made by the writer with the latter liquid showed, however, that its addition to paraffin in the proportions specified actually decreased the spreading power. By kind permission of the Headmaster of one of our leading schools, a series of experiments relating to the spreading qualities of different oils and to various other matters connected with anti-mosquito work are now being carried out in the school laboratory by some of the students.

ten inches, then our pint of larva poison would be "watered down" to a proportion of 1 in 15,000, and the requisite strength of 1 in 1,500 could only be secured by using ten pints of the poison instead of one.

Quite generally, if d be the depth of the water in inches, n the number of parts in which one part of the given larva poison is effective, and c the ratio of its price to that of paraffin, then, if the cost of treating a given area by paraffining is taken as unity, the cost of treating the same area by the poison is given by the expression

$$\frac{1500 \times c \times d}{n}.$$

As an illustration of the use of this formula, we may take the case of cyllin, the only larva poison recommended in a recent Government publication, the degree of dilution specified being a teaspoonful to a gallon, a proportion which works out at 1 part in 1,280. The price of cyllin being about six times that of paraffin, the relative cost of employing it in the strength given would be

$$\frac{1500 \times 6}{1280} \times d,$$

or almost exactly $7 \times d$ times that of treating the same area with paraffin.

If, for example, the depth of the water were 15 inches (a very usual figure) the cost of the cyllin required would be more than 100 times that of the necessary paraffin.

For the treatment of water-places to which, owing to inaccessibility or any other reason, frequent visits of inspection are inconvenient, the adoption of the poisoning method, which produces a more lasting effect than the other, is sometimes advisable. In cases, too, where excessive growths of water-weeds prevent the creation or maintenance of an effective oil film, larva poisons are often indispensable; and it is not difficult to imagine other situations in which the advantages gained by their use may amply justify the additional expense.

JOHN F. MARSHALL.

REVIEWS

Studies in the History and Method of Science. Edited by CHARLES SINGER. [Pp. xiv + 560, with 55 plates and 104 illustrations in the text.] (Oxford: at the Clarendon Press, 1921. Price £2 8s. net.)

THE first volume of these *Studies* appeared in 1917. It has taken nearly four years for the second volume to make its appearance. Future volumes will no doubt be issued at shorter intervals. In the meantime the new volume offers ample compensation. It is almost three times the size of the first volume, and superior in quality as well as in quantity. If the first volume was sold out so quickly, the present volume should be out of print long before the year is out.

Let it be said at once that the present *Studies* constitute a very substantial addition to our knowledge of the history of science. There are fifteen essays in the volume, nearly all of them by experts in their several subjects. The range of subjects is wide, and the numerous plates and illustrations are at once illuminating and delightful. Dr. Singer and his colleagues are to be congratulated on the result; and the Clarendon Press, too, deserves more than a word of thanks for its share in producing such a thing of beauty.

In a professedly periodic volume like the one under review it is not always possible to do even justice to all the sciences or to all the centuries. Each volume is likely to give special prominence to some subject or period at the expense of other subjects or periods. It must be left to the series as a whole, or at least in the long run, to redress the balance. The first volume gave special prominence to medieval studies. The present volume shows a more even distribution as regards time, but it gives special prominence to certain subjects, namely, biological studies, to which about half the volume is devoted. This should be a special attraction for biologists, without discouraging others, for even the biological essays contain much that is of general scientific interest, while other essays are devoted to other sciences, or to problems on the borderland of science.

The biological essays relate almost to all the ages since the beginning of science. Dr. Singer leads off with Greek Biology and its Relation to the Rise of Modern Biology. This essay of about a hundred pages is quite a treatise in itself. It describes in some detail the biological work of Aristotle and Theophrastus, and their influence down to the eighteenth century or even later. Even if one does not altogether subscribe to his general reflections on Greek Science, and regrets his omission of any reference to the more philosophical aspects of Aristotelian biology, there can be no doubt that Dr. Singer deserves the warm thanks of all who are interested in the early history of science in general and of biology in particular. Dr. Singer's account of Aristotle is supplemented, in another essay, by Prof. Platt, who throws new light on Aristotle's conception of the structure and function of the heart, and incidentally clears up some obscure passages which have been the despair of translators and commentators of Aristotle's works. Early Greek medicine is described, in an interesting manner, by Mr. E. T. Withington, in an essay on "The Asclepiadæ and the Priests of Asclepius." Mr. F. C. Conybeare

introduces to us four Armenian Tracts on the structure of the Human Body: The tracts date from the twelfth century or earlier. Those interested in the history of medicine, in the East and West, will find here a very readable translation and a helpful introduction. Dr. H. Hopstock's illuminating account of Leonardo da Vinci as anatomist brings us to the beginning of modern times. Dr. Hopstock is one of the greatest living authorities on Leonardo, and, although he writes with great moderation, he has no difficulty in making his reader realise the greatness of his hero. A great thinker and a great artist, Leonardo was the greatest naturalist of the fifteenth century. The beautiful plates which accompany the essay bring home to the reader how effectively Leonardo used his artistic genius in the service of science. The History of Anatomical Injections is dealt with very fully by Mr. F. J. Cole, and the list of biological essays in the present volume is completed by A Sketch of the History of Palæobotany, written by the late E. A. Newell Arber.

Intimately connected with the history of biological science is, of course, the invention of the microscope, of the history of which Dr. Singer has made a special study. The present volume contains an essay on the Steps leading to the Invention of the First Optical Apparatus. In just under thirty pages Dr. Singer marshals the chief historic data relating to his subject, and presents us with one of the most interesting narratives in a volume of many interesting narrations. Dr. Singer's story ends with Galileo, to whose scientific work a special essay is devoted. The writer of the essay is Mr. J. J. Fahie, and his account of the Scientific Works of Galileo, with some account of his life and trial, is worthy of our foremost authority on the subject. An equally interesting and able account of Roger Bacon and the State of Science in the thirteenth century is contributed by Mr. Robert Steele, to whose excellent qualifications for the task these pages have recently borne witness in a review of his edition of Roger Bacon's *Secretum Secretorum*. An important contribution to the early history of Mechanics is made by Mr. J. M. Child in an account of Archimedes' Principle of the Balance, and some criticisms upon it. And Dr. J. L. E. Dreyer's account of Medieval Astronomy corrects some misconceptions in M. Pierre Duhem's great work. This completes the list of contributions on the history of science.

There remain three essays on what may broadly be called the Method of Science and connected philosophical questions. An essay on Hypothesis is contributed by Dr. F. C. S. Schiller, who writes in his usual style. Much more important is the essay on Science and Metaphysics, by the late Mr. J. W. Jenkinson. Finally, Mr. F. S. Marvin must be praised for one of the most stimulating essays in the volume. His theme is Science and the Unity of Mankind. *A priori* and *à posteriori* he tries to show the essentially social nature of science and its tendency to become the fundamental bond of the human race. In the future teaching of history, he rightly thinks, a large place must be given to the history of science as the field on which the nations have always worked together most easily. In his Preface, Dr. Singer gives a very encouraging account of the recent progress of teaching and research in the history of science. It is to be hoped that future Prefaces will continue to record sufficient progress to encourage the hopes of Mr. Marvin and those who think with him.

A. WOLF.

MATHEMATICS

A Treatise on the Integral Calculus. By JOSEPH EDWARDS, M.A. Vol. I. [Pp. xx + 907.] (London: Macmillan & Co., 1921. Price 50s. net.)

IN a review of the second edition of Mr. Edwards's *Differential Calculus* Miss C. A. Scott (*Bull. New York Math. Soc.*, **1**, 1892, 217) ventured to poke

fun at "Mathematical Tripos Mathematics," and to wish that the author, instead of consulting Examination Papers set at Oxford, Cambridge, London, and elsewhere, had spent the time in reading "memoirs published mostly elsewhere." It is to be feared that the publication of the *Integral Calculus* will merely convince the mathematical world of the justice of this gibe. Problems there are in plenty, a magnificent collection, each with its source duly acknowledged (including one by "Asparagus" of the *Educational Times*), but the general theory is sadly deficient. It seems as if the author fell asleep some thirty or forty years ago, and is not in the least aware of any recent work. He acknowledges in the Preface indebtedness to the works of Hobson and Forsyth, but shows no sign of having read them. There are references to Hobson, it is true, as well as many to Bertrand and Serret and Williamson, but they are to the Trigonometry and not to the Functions, as one would have expected. On page 76 reference is made to the ordinary notation for the hyperbolic functions as being "now commonly adopted by *modern* writers," and the book quoted was published in 1888. Following this it is rather a shock to find mention of tables of these functions published in 1914.

Earlier reviewers tried to acquaint Mr. Edwards with the idea of uniform convergence, but in vain, as is evidenced by his curious proposition, curiously proved, on p. 44, on the integration of series.

The proportions of the book are so astonishing, too. After long and, in many cases, far too complicated investigations of standard forms and reduction formulæ (for which the student would be much better advised to consult Bromwich's shilling tract, *Elementary Integrals*) we get 120 pages on Quadratures, and 150 pages on Rectification. These include, it is true, such notice as the author deigns to give (six pages in all) to Stokes's Theorem and double integrals, but there are pages and pages on pedals, lemniscates, Cassini's ovals, bipolar curves, biangular co-ordinates, areas and arcs in terms of trilinear co-ordinates, etc., etc.

The book is, in fact, a hotch-potch of curious and disconnected mathematical puzzles.

F. P. W.

Calculus for Beginners. By H. SYDNEY JONES, M.A. [Pp. ix + 300.] (London: Macmillan & Co., 1921. Price 6*s*. net.)

THIS book is yet another attempt to write an introduction to the Calculus "sufficient for practical applications," without any mention of the word "limit." The necessary consequence is vagueness, and the use of phrases such as "when Q is very very small, AC′ becomes equal to AC″ (p. 60)," "when Q is exceedingly close to P" (p. 212), "suppose x very very great" (p. 109). Series are of course mentioned without reference to convergence, except that "to be of service in computation the individual terms must be getting smaller and smaller."

The author is strangely inconsistent in his notation; the exponential number is denoted by the Greek ϵ on p. 259, although elsewhere the usual convention is adopted; and the treatment of the Leibniz theorem (of course spelt Leibnitz), unnecessarily heavy as it is, is made still more clumsy by the use, just here, of the old factorial notation instead of the modern n!

There is a wonderful picture of a staircase on p. 105.

Altogether it is difficult to see the need of the book, when excellent ones (*e.g.* Gibson) covering much the same ground, so very much better, already exist.

F. C. W.

The Transition Spiral, and its Introduction to Railway Curves, with Field Exercises in Construction and Alignment. By ARTHUR LOVAT HIGGINS, M.Sc., A.R.C.S., A.M.Inst.C.E. [Pp. viii + 111, with 10 illustrations.] (London: Constable & Co., Ltd., 1921. Price 6*s.* net.)

IN this book the various formulæ used in laying out a transition curve are collected and amplified, with special reference to the curve known as Glover's Spiral.

After a general account of the formulæ, there are a set of worked examples which should prove useful to the practical man, and the close agreement between the cubic parabola and the Glover's Spiral is pointed out. It may be thought unfortunate that the term "deflection angles" has been used in places for what might better be called "tangential angles"; but Mr. Higgins can at least argue that if this is a fault it is by no means singular to his book.

M. T. M. O.

ASTRONOMY

The Elements of Theoretical and Descriptive Astronomy. By CHARLES T. WHITE, A. M., formerly Professor of Mathematics of Harvard College. Eighth Edition. Revised by PAUL P. BLACKBURN, Commander U.S. Navy. [Pp. xi + 309, with 84 figures and 9 plates.] (New York: John Wiley & Sons. London: Chapman & Hall, 1920. Price 17*s.* 6*d.* net.)

THIS volume is the eighth edition of a work first published in 1869 to meet the requirements of the United States Naval Academy. At the date of its initial publication it represented fairly the state of astronomical knowledge, and it gave in a simple, concise, and readable treatment the essential facts required by a student taking an elementary course in astronomy. With successive revisions, however, the volume has gradually become hopelessly out of date, and that it has reached an eighth edition is doubtless due to its adoption as a textbook at the U.S. Naval Academy.

Only a few instances can be given to illustrate the failure to bring the work up to date; the volume contains many others. Our knowledge of prominences is thus summed up: "Mr. Lockyer, of England, who has since examined them, pronounces them to be merely local accumulations of a gaseous envelope completely surrounding the sun." In dealing with solar eclipses, no mention is made of information derived from eclipses later than 1868. The greater part of the section dealing with the solar motion is concerned with the work of Otto Struve and "Mr. Airy." The much more important modern investigations are disposed of in two lines. Star-streaming is not even referred to. The important work of recent years in the determination of radial velocities and of the orbits of spectroscopic binaries is represented by the statement that "Mr. Higgins, using a spectroscope of large dispersive power, and carefully comparing the spectrum of Sirius with that of hydrogen, found that the line F in the spectrum of Sirius *was displaced* by about $\frac{1}{160}$th of an inch." The only important event since 1891 considered to be worth chronicling in the table of astronomical chronology is the completion of the Cape Photographic Durchmusterung.

These instances will suffice to illustrate the success of the reviser. It is not probable that the volume will find much sale outside the Academy for which it was originally written.

H. S. J.

The Parallaxes of 260 Stars derived from Photographs. By S. A. MITCHELL assisted by C. P. OLIVIER, H. L. ALDEN and others. Publications of the Leander McCormick Observatory, Vol III. [Pp. 659]. (New York: Columbia University Press, 1920.)

THIS publication contains the detailed results of the parallax determinations of a first series of 260 stars derived at the Leander McCormick Observatory.

It illustrates in a remarkable way the power of modern photographic methods: the observations were commenced in the middle of 1914 and up to 1920 260 parallaxes of a high order of accuracy had been determined. In addition, numerous plates of other stars have already been obtained, so that probably about 100 determinations per year can be added to the series. This work has been accomplished with a single telescope and a small staff, who have not only taken the photographs but have also measured and reduced them. The results testify to the industry, organising ability, and enthusiasm of Prof. S. A. Mitchell, who is heartily to be congratulated upon the skill and perseverance with which he has carried on his programme of work.

The methods of observations follow generally upon the lines indicated by Schlesinger in his epoch-making work at the Allegheny Observatory. They are fully outlined in the preface to this volume, which contains an interesting account of the history and methods of parallax determination; it can be recommended to anyone desiring information upon the subject. With modern methods, the accidental error of observation can be reduced to somewhat less than one-hundredth of a second of arc, but it is not so easy to ensure that systematic errors have been entirely eliminated. Examination of the Leander McCormick results indicates that the systematic errors are extremely small—of the order of a few thousands of a second, so that the series may be regarded as one of the most accurate as well as one of the longest which has emanated from a single observatory H. S. J.

Simple Lessons on the Weather. By E. STENHOUSE, B.Sc. [Pp. viii + 135, with 12 plates and 62 figures.] (London. Methuen & Co.)

THIS book explains some of the physical processes of meteorology in a simple and lucid manner, and is primarily a school textbook; it can, however, also be read with advantage by anyone interested in the subject who does not possess much scientific knowledge.

There are twenty-eight chapters altogether, dealing with such subjects as the formation of rain and hail, fog, dew, etc., and the measurement of rainfall and sunshine. There is even a little astronomy included to help to explain the seasonal changes of the weather. A good feature is the frequent use of "isopleth" diagrams for displaying a large mass of statistical information in a very clear manner, for the principle underlying these diagrams must in any case be mastered by those who wish to understand modern synoptic-charts and weather forecasting.

Included among the twelve plates are some excellent cloud photographs, and others of great beauty representing snow, rime, hoar frost, and hail.

E. V. N.

PHYSICS

British (Terra Nova) Antarctic Expedition, 1910-13: Terrestrial Magnetism. By CHARLES CHREE, M.A., Sc.D., LL.D., F.R.S. [Pp.xii + 550, with four figures and 60 plates.] (London: Harrison & Sons, 1921.)

THIS volume, the cost of the preparation and publication of which has been defrayed from the fund which was raised by public subscription in memory of Captain R. F. Scott and his companions, contains an elaborate discussion of the magnetic observations obtained on the Antarctic Expedition of 1910–13. Although an enormous amount of work has been involved in compiling this volume, the results would have been available at an earlier date but for the war; in spite of the delay so caused, the publication is more prompt than in the case of some similar expeditions. Frequently the delay in publication is so great that the value of the scientific results obtained is materially reduced, and a plea may be put forward for an endeavour

in future to publish as early as possible at least an abridged summary of the results.

The magnetic observations secured on this expedition comprised daily magnetographs of three components of the earth's magnetic field, supplemented by absolute observations at more or less regular intervals for the purpose of determining the base-line values of the magnetographs. A large amount of material was therefore available for discussion. In addition, by previous arrangement, a number of simultaneous quick runs were obtained in the Antarctic and at various permanent observations, during which were recorded, on a very open-time scale, the variations in the several components of the earth's magnetic field.

From a discussion of the absolute observations, the most probable base-line values are deduced, and the hourly values of the elements hence obtained from the magnetographs. These enable the diurnal inequalities and the daily absolute ranges to be determined and discussed. The results obtained are compared with those obtained during the National Antarctic Expedition of 1902–4 and with those obtained at Observatories in lower latitudes. This is followed by a discussion of the annual and diurnal variation of magnetic character and of the evidence in favour of a 27-day period. A detailed comparison of the records obtained during certain of the quick runs in the Antarctic and at other observatories is also given. The Antarctic is a region of numerous and violent magnetic storms, and most of the remainder of the discussion is occupied with a description of the various types of disturbance and with comparison of the records of the more prominent storms obtained in the Antarctic and elsewhere. A chapter is included dealing with the connection between magnetic disturbances and aurora.

Although no very important new conclusions are arrived at, and the discussion is in general confirmatory of previous knowledge, it forms material addition to the data on which any theory of the variations of the terrestrial magnetic elements in general, and of magnetic storms in particular, must be based. The hourly values of the elements have been tabulated at the end of the volume, so that the material is available for any other investigator to discuss at any future time any points which may not have been touched upon in the present volume.

H. S. J.

Traité de Dynamique. By Jean D'Alembert. **Les maîtres de la pensée scientifique.** Collection de mémoires et ouvrages. Publiée par les soins de Maurice Solovine. [Two volumes. Pp. xl + 102, 182.] (Paris; Gauthier Villars, 1921. Price 7 francs.)

Perhaps in no other great branch of human knowledge have the professors in the past paid so little regard to the history of their subject as in mathematics and its allied sciences. To many students of mechanics the names of Archimedes, Galileo, Newton, D'Alembert and Lagrange are but convenient labels for certain laws and theorems, and their knowledge of these men is confined to a few pleasing anecdotes of doubtful authenticity. Happily, signs are not wanting of a change for the better and there is at the present time a marked revival of interest in the history of mechanics. This is partly due to the influence of modern theories which have driven us to a deeper study of the work of the old masters. An Einstein was needed to show us how little we had understood of the genius of Newton. But we must also acknowledge our debt to the educationalists who have shown us that the best we can do for our students is to bring them into a living contact with the greatest minds that the race has produced. If in their turn they are to advance and extend the bounds of knowledge it is not sufficient that they should be our disciples: we must lead them into discipleship with those greater men who have made our work

possible, that in their writings they may learn something of the long and hard struggle by which our present knowledge has been won.

The study of applied mathematics seems to us to be at a low ebb in England, and we are convinced that this is largely because we have too often presented it as a static body of propositions based upon incontrovertible logic. We must make our students see mechanics grow. To this end there are two courses open to us. We can show them mechanics growing in current research, or we can show them its gradual growing in the past. It is often contended, though we think it false, that the nature of the subject prevents that early acquaintance with research which may be enjoyed by students of the other natural sciences. In an honours degree examination in chemistry and physics we may well find questions relating to knowledge gained during the last few years; in the corresponding examination in mechanics we shall probably find little which is less than a century old. It may be that there is more than the mere conceit of the mathematician in his contention that his subject is so much more difficult than others that this state of affairs is unavoidable. As a mathematician, we doubt it. There can, however, be no question but that the student will gain a new interest and a deeper understanding of his subject through the reading of its greatest classics. If he has the good fortune to work in one of our great libraries he will have every opportunity for this. In other circumstances it will not be so easy. We therefore welcome the series of "les maîtres de la pensée scientifique" published at a price of about 3 francs per volume, with the promise that "les mémoires et les ouvrages français seront réimprimés avec grande exactitude d'après les textes originaux les mieux établis, et ceux des savants étrangers seront traduits intégralement et avec une rigoureuse fidélité." About half a dozen works have already appeared, and D'Alembert's *Traité de Dynamique* is the first dealing with mechanics. We are promised, at an early date, Galileo's *Dialogues* and Newton's *Principia*. We hope that this list will be extended and that we may have other works in the same attractive and accessible form.

G. B. J.

William Sutherland. A Biography by W. A. OSBORNE. [Pp. 162.] (Melbourne: Lothian Book Publishing Co.; London: The British Australasian, 1920. Price 7*s*. 6*d*. net.)

WILLIAM SUTHERLAND was a physicist of exceptional ability, whose work has failed in many quarters to secure adequate recognition. This biography, written by Prof. Osborne at the request of some of Sutherland's Australian friends and academic colleagues, will be found to indicate probable reasons for Sutherland's comparative isolation. If, also, it begets a belated homage to a man who, during the twenty-six years preceding his death in 1911, contributed to various branches of physics sixty-nine original papers, the hope of the biographer and his committee will be fulfilled.

Sutherland died at the age of fifty-two. Apart from a few years spent in Scotland in early childhood, and three years' study at University College, London, he lived his life in Australia. His love for his home was exceptional, and to this, mainly, must be attributed the fact that he never held a high University post. Openings in England occurred more than once, but were declined; and even posts in Australia, if distant from his home in Melbourne, had no attraction for him. In addition, the responsibilities of a university teacher were irksome, because Sutherland found them to hinder the prosecution of his researches.

Dr. Osborne has been able to give a very pleasant description of many aspects of Sutherland's life. Naturally, that obtaining the most attention is his work in physics. It is necessary to make only one criticism of this account. Sutherland is best known to physicists by reason of his contribution to the kinetic theory of gases in relation to the variation of viscosity with

temperature. The biographer makes a mistake in supposing that Sutherland's law of viscosity variation is necessarily associated with another, and less reliable, of Sutherland's efforts, viz. the deduction of an inverse fourth power law of force between molecules. The viscosity law is on a much broader basis, and is completely independent of the actual law of molecular force. It is significant that, although the inverse fourth power law seldom finds acceptance among physicists, the viscosity law is generally held to be sound, and has recently received confirmation in the more formal analysis due to S. Chapman.

Sutherland had, besides, great literary ability, and was deeply interested in music and art. These the biographer also touches upon; and the picture he is able to draw is that of a man of many and varied achievements—a man to be admired and loved, but, withal, somewhat pathetic.

A. O. Rankine.

CHEMISTRY

Thermodynamics and Chemistry. By F. H. Macdougall, M.A., Ph.D. [Pp. v + 391.] (New York: Wiley & Sons; London: Chapman & Hall, 1921. Price 30*s*. net.)

Prof. Macdougall has, in his own words, set himself the task of writing "a book which, in addition to being accurate, logical, and sufficiently rigorous, will furnish the student with numerous examples of the application of the principles of the science." On the result of his work he deserves to be complimented: the book under notice is one of the most satisfactory accounts of the principles of thermodynamics, with particular reference to chemistry, which the reviewer has seen. The difficulties are mostly faced and disposed of honestly; lame expressions such as "it can be shown," or "by analogy we see," which appear too frequently in books on thermodynamics intended for chemists, are not noticed except in the last chapter (radiation and quantum theory), where a full discussion would have involved more space and mathematics than would have been expected.

Any exposition of chemical thermodynamics which is easy reading may be taken as either so rudimentary as to be trivial, or so dishonestly evasive and superficial as to be prejudicial to good teaching. Prof. Macdougall makes free use of the elements of the calculus—he could not do otherwise with justice to his subject or his readers, but the results are eminently practical. "No apology is offered for the considerable number of mathematical equations which will be found throughout the book. They are as essential as chemical equations in a book on descriptive chemistry." The analogy is sound: it is quite possible to write a book on chemistry without formulæ or equations, but it is not desirable. The collections of examples and exercises at the ends of the chapters are commendable.

The treatment of the subject follows the classical analytical methods of Gibbs and Planck rather than the use of the cycles more common in elementary books. Whilst the former method is more elegant, it is probable that beginners derive more benefit from the older treatment. For this reason, the reviewer believes that the book should be read by those students who have already had a brief course of elementary thermodynamics, such as is usually comprised in general lectures on physics. The deduction of the Clapeyron-Clausius equation given by Prof. Macdougall (pp. 123–9) would not appeal to beginners so forcibly as the usual proof with a cycle.

The teaching of thermodynamics, in the opinion of the reviewer, may best follow the order adopted in dynamics. The laws of motion as given by Newton are sufficient to allow of a good deal of progress being made before beginning analytical methods. The reader of Gibbs can hardly fail to notice the influence of Lagrange: Gibbs did for the older thermodynamics of

Carnot and Lord Kelvin what Lagrange did for Newton's dynamics, and the analogy is more than formal. Nernst has expressed the opinion that the use of thermodynamic functions merely proves that an author has studied the writings of his predecessors with intelligence. There are, however, several results which have been deduced by analytical methods only, and Nernst's description might be applied to the reverse process of translating these into elementary modes of treatment. Both methods of treatment seem desirable in a course of study. The reviewer is of the opinion that a purely descriptive account of the fundamental laws of energy is possible, and would not be without value to chemists who are not equipped with the modest mathematical knowledge required for real study, but no practical applications of these laws can, of course, be made without the calculus.

The subjects treated in the book under notice are those usually dealt with. Two chapters on the applications of the phase rule are a little foreign to the subject, and the space might have been used in the last chapter (on recent developments), which is a little too condensed. The definition of an ideal gas is by no means clear: it would appear that a number of properties are required, whereas many of these are implied in others.

It is to be regretted that the price of the book is so high as to put it beyond the means of numbers of students.

J. R. PARTINGTON.

Ammonia and the Nitrides. By E. B. MAXTED. [Pp. viii + 114.] (London: S. & A. Churchill, 1921. Price 7*s*. 6*d*. net.)

IN this monograph the author devotes the first forty-eight pages to the synthesis of ammonia from its elements. He gives a good account of the work of Haber and his co-workers. But in the first line of the book he plunges into thermodynamic formulæ in a way which must be bewildering to a reader not already well acquainted with the subject before opening the book.

On p. 9, in a discussion of the formulæ which have been used to express the heat of combination of nitrogen and hydrogen to form ammonia, the author states:

> "The equations expressing the variation of Q with temperature are of such a nature that they pass through a maximum, then decrease, and at a still higher temperature pass through zero and become negative. The point is of considerable interest in connection with the formation of ammonia at very high temperatures . . . the equilibrium percentage of ammonia should decrease with increasing temperature so long as the reaction is exothermic, but should pass through a maximum value and rise once more with any reversal, at high temperatures, of the sign of Q."

The above word *maximum* should obviously read *minimum*.

In this deduction the author has fallen into an amazing error. The "equations expressing the variation of Q with temperature" are obtained from the measurements of Q by Haber or calculated from measurements of specific heats, and are of the type:

$$Q = a + bt - ct^2.$$

An equation of this type is sufficiently accurate to represent the experimental results up to perhaps 1,000° C., but as there are no measurements of Q at high temperatures, an extrapolation of the above formulæ is quite unjustified. If the experimental results below 1,000° C. were more accurate, we should probably express them by means of an equation of the type:

$$Q = a + bt - ct^2 + dt^3,$$

which, if extrapolated to high temperatures, would give still another reversal of the sign of Q.

Some fifty pages are devoted to the chemistry of the nitrides and about a dozen pages to active nitrogen. Our knowlegdc of the nitrides is at present very incomplete, but a fairly complete summary of what is known is to be found in these pages.

All through the book, full references are given to original papers and good indexes are included. We can recommend the book to all who are at all interested in the synthesis of ammonia or in the chemistry of the nitrides.

R. E. SLADE.

Chemical Technology and Analysis of Oils, Fats, and Waxes. Vol. I. By J. LEWKOWITSCH, M.A., F.I.C., edited by GEORGE H. WARBURTON. Sixth edition, entirely rewritten and enlarged. [Pp. xviii + 682, with illustrations and numerous tables.] (London: Macmillan & Co., 1912. Price 36*s*. net.)

ALTHOUGH this is a standard work, necessary to all those interested in oils, fats, and waxes, it is not incapable of improvement. It is to be regretted, therefore, that the present volume differs so little from the previous edition as to render the description " entirely rewritten " hardly justifiable as applied to vol. i. The editor points out that the greatest advances have been made on the technological side, so that vol. ii, when it appears, may show greater signs of rewriting. He considers that the purely scientific side has, during the last few years, been somewhat neglected; but it would seem that some of the scientific work which has been done since the publication of the last edition had been overlooked in the preparation of the present one.

A table of melting-points of the amides, anilides, etc., of the fatty acids is given, but no mention is made of the redetermination of these constants by Robertson. M'Bain's work receives but a few lines, whereas much older work of doubtful value takes up a disproportionate amount of space. Moreover, one would have thought that the prominence of explosives during the past few years would have prevented the statement that nitroglycerine forms the chief ingredient of almost all modern high explosives.

The present volume could have been greatly improved by thorough revision, careful selection of data, and the introduction of modern chemical conceptions; indeed, it suffers from a complaint common to many works with an established reputation. Further matter is added from time to time, but the outlook and arrangement of the first edition is retained, with the result that the book becomes a repository for a mass of information collected from the literature presented in a very indigestible form.

O. L. B.

A Textbook of Practical Chemistry. By G. F. HOOD, M.A., B.Sc., and J. A. CARPENTER, M.A. [Pp. xii + 527, with 161 illustrations.] (London: J. & A. Churchill, 1921. Price 21*s*. net.)

THE *raison d'être* of this book is, according to the Preface, " an attempt to place in the student's hands a Manual of Practical Chemistry which shall be a reference-book in a convenient form " ; it aims " at giving a thorough knowledge of general methods," and an " attempt has been made to indicate the best methods of doing everything." By avoiding unnecessary verbiage, the authors have been successful in confining a very large amount of instructive and suggestive matter between the covers of the volume. Sufficient theoretical explanations and notes are included to make the various experimental operations intelligible and to render the book very readable. References are made throughout to standard textbooks, and, in many cases, to original papers. The scope of the book may be realised from the following brief notes on the

several sections. I. INORGANIC PREPARATIONS.—This is a useful section; instructions for the preparation of nearly 200 substances are given, including a few colloids. II. QUALITATIVE ANALYSIS (INORGANIC).—The reactions of the common radicles and of some of the "rare" elements are given. A course of analysis follows; more attention might have been paid here to optimum concentrations for reactions, but otherwise the course is complete enough for the average student. III. QUANTITATIVE ANALYSIS (INORGANIC).—This is subdivided into Gravimetric Methods, Electrolytic Methods, Volumetric Methods, and Gas Analysis. The list of estimations is very comprehensive. The description of the preparation of Baryta solutions requires a little more extension. Insistence is laid on standardising at the same temperature all measuring instruments used in volumetric analysis. This is unnecessary, as suitable and instructive corrections can be applied when instruments are calibrated at different temperatures. IV. ORGANIC PREPARATIONS.—Instructions for nearly 300 preparations are given. The value of this section is enhanced by the fact that the chief properties of many of the substances to be prepared are described. V. ORGANIC ANALYSIS (QUALITATIVE AND QUANTITATIVE).—Qualitative analysis is dealt with in eight pages; this part could be improved by some notes on the separation of organic compounds from each other. The subdivision on Quantitative Analysis describes the estimation of elements and of the more important radicles. VI. PHYSICAL CHEMISTRY.—An elementary course is condensed into some eighty pages. The value of this section would be increased by including some of the E.M.F. measurements which have important applications in Physical Chemistry.

The whole volume should be of much use to many classes of students.

L. F. G.

The Electronic Conception of Valence and the Constitution of Benzene. By HARRY SHIPLEY FRY, Ph.D., Professor of Chemistry and Director of the Chemical Laboratory, University of Cincinnati. [Pp. xviii + 297, with numerous diagrams.] (London: Longmans, Green & Co., 1921. Price 16*s*. net.)

THIS series of monographs has hitherto consisted of "summarised accounts of progress made in recent years" in various branches of Inorganic and Physical Chemistry. The above book scarcely comes within the original scope of the series, as it is essentially an account of the author's own views on the electronic conception of valency. Other work in this field is summarised in a bibliographical review of about 12 pages. In spite of the interesting theory presented by the author, it is to be regretted that a complete summary of modern ideas does not find a place in the monograph. The outlook on this account is very circumscribed; there is practically no discussion of the idea of polar and non-polar valency, and the theories of Lewis and Langmuir and the magneton theory of valency are completely ignored. The essential weakness of the presentation is that no differentiation is made between the valency forces operating in carbon compounds and those occurring in compounds ionised in solution and in the solid state.

Basing his theory of valency on the earlier ideas of Sir J. J. Thompson, he develops the conception of positive and negative valency, and this idea, as put forward by the author, is very suggestive. Evidence is adduced to show that the occurrence of negative hydrogen and positive chlorine is possible in carbon compounds.

The major portion of the book is devoted to the benzene nucleus. In his theory of the benzene ring he makes use of the Collie benzene formula, making the alternate C and H atoms + and — charged respectively. The author thus ascribes an electro-positive or electro-negative character to each

group, explaining any abnormal behaviour of organic compounds by a reversal of the ordinary sign of charge of the group. This method of formulation of the benzene derivatives is presented clearly and possesses considerable merit in view of the complexity of the processes of substitution in the benzene ring.

The physical properties of benzene compounds are taken into consideration, particularly those of molecular volume, absorption spectra, fluorescence, etc. The theory of absorption of ultra-violet light, by the benzene nucleus is, however, open to serious criticism. Almost any series of frequencies taken at random would give satisfactory agreement with the theory of keto-enol tautomerism put forward by the author.

The book does not appear suitable for degree students, but those undertaking research in organic chemistry may find the author's theory suggestive of new lines of work.

W. E. G.

An Introduction to Organic Chemistry. By D. Ll. HAMMICK, M.A., Fellow of Oriel College, Oxford. (Pp. viii + 258, with diagrams). (London: 1921. G. Bell & Sons. Price 6*s*. net.)

THE present work has been written with a view to enabling beginners in chemistry to become familiar with the elementary notation and technique of organic chemistry earlier than is usually the case, and is not written with a view to covering any special syllabus, but in order that the student may start the organic portion of chemistry on level terms with the inorganic and physical sections.

The author assumes, of course, that the reader has an acquaintance with elementary chemistry, and with general laboratory operations. After introducing the subject, and discussing the question of valency, structural formulæ, and methods of analysis, the book deals with the production and isolation of ethyl alcohol, thence to other alcohols, and so to the discussion of homology, the essential facts of which are very clearly shown in the table on page 25.

Interspersed in the text are suitable simple experiments which can be readily carried out in the laboratory. Thence by easy stages the beginner is introduced to the subjects of esters and isomerism. Chapter IV is devoted to the action of sulphuric acid upon alcohol, leading to the formation of ether and ethylene, thus affording an opportunity for discussing saturation and unsaturation. After this the usual derivations are dealt with, such as aldehydes, fatty acids, ketones, paraffins, amides, etc., the latter half of the book dealing with aromatic compounds.

The book is essentially readable throughout, and it would be difficult for anyone who has worked carefully through the text, and the experiments, not to have had his interest and even his enthusiasm aroused for the wonders of organic chemistry, which may serve to carry him forward through the occasionally more tedious parts of the subject when he comes to tackle the larger textbooks.

Mr. Hammick has succeeded in condensing the elements of organic chemistry into small compass with great skill, and those requiring an introductory textbook on the subject will do well to consider the book carefully.

F. A. M.

Organic Analysis, Qualitative and Quantitative. By E. DE BARRY BARNETT, B.Sc., F.I.C., and P. C. L. THORNE, M.A., A.I.C. [Pp. xi + 168, with 31 illustrations.] (London: University of London Press, Ltd., 1921. Price 7*s*. 6*d*. net.)

PART I of this book gives the usual tests for the elements and for separating the substances under examination into classes depending on the elements

present. This is followed by a section dealing with the possible types of compound in each class.

Part II contains instructions for the quantitative estimation of carbon, hydrogen, nitrogen, sulphur, phosphorus, and halogens. Then follow the determination of molecular weights, the estimation of groups, and methods for the estimation of selected compounds.

A most useful part of the book is an Appendix giving the student careful instructions for using Richter's *Lexikon* and Beilstein's *Handbuch*. This should encourage him to consult the original literature.

The book is well printed; a few obvious misprints have, however, been noticed. The diagrams are clear, but we wish that Fig. 8 illustrated a more modern type of combustion-furnace, less wasteful of gas. The authors suggest that the student should complete his identification by a quantitative analysis of an element or group in the compound. This would make rather a large claim on the student's already limited time. The descriptions and reactions of the compounds are sometimes rather brief, but due importance is given to physical properties, and instructions are given for determining these, including the refractive indices of liquids. Some references to the original literature might have been included in Part II with advantage.

It should prove a useful book, and should encourage the student to think about his work.

J. N. E. D.

Animal Proteins. By HUGH GARNER BENNETT, M.Sc. **Industrial Chemistry,** Edited by SAMUEL RIDEAL: [Pp. xiv and 287, with 2 figures] (London: Baillière, Tindall & Cox, 1921. Price 15*s*. net.)

IN this volume 203 pages are devoted to the manufacture of leather, 70 to the manufacture of gelatine and glue, and 8 to food and miscellaneous animal proteins; it seems, therefore, that a title more descriptive of the work might have been chosen. An interesting account is given of the raw materials, and the manufacturing processes employed in the leather and glue trades, and the variations in procedure and in starting materials for the preparation of the various commercial grades of leather are indicated. On the other hand, but the vaguest description is given of plant, and there is not a single illustration. The organic chemistry of the proteins is but touched upon, and the chemistry and physics underlying the various processes are very inadequately dealt with.

Except in the case of chrome-tanning, no methods of analytical control of manufacture are mentioned, indeed the book conveys the impression that tanning, as practised in this country, is purely empirical. The book may be of value to students and others who require information on trade matters, but, as little is said on the trend of research in the industry and the general scientific aspect, it is unlikely to encourage chemists to seek posts in tanneries.

O. L. B.

Biochemistry: A Study of the Origin, Reactions, and Equilibra of Living Matter. By BENJAMIN MOORE, M.A., D.Sc., F.R.S. [Pp viii + 340.] (London: Edward Arnold, 1921. Price 21*s*. net.)

IT is stated in the Preface that this book is not intended as a general text-book of biochemistry, but is meant to give some prospect of the origin and reactions and balances of living matter; it is divided into fifteen chapters, of which the first two were written before the experiments described in the succeeding chapters were carried out. These experiments are described almost verbatim from the *Proceedings of the Royal Society*, while the remaining chapters are re-edited from the author's *Recent Advances and Further Advances in Physiology*. The opening chapter contains an account of the

author's views upon the intimate relationship between colloids and crystalloids in the living organism, and the effect of their mutual interaction upon rhythmic processes such as fatigue and recovery; the second chapter deals with speculations on the possible origin of life by the gradual evolution upon the cooling globe of inorganic colloidal solutions, which in presence of sunlight were able to effect photosynthesis; then follows a description of the author's own experiments upon the production of formaldehyde by the action of colloidal ferric hydroxide upon carbon dioxide in sunlight. In the third chapter is to be found an account of experiments by which the author seeks to demonstrate the assimilation of nitrogen from the air by the green plant—experiments which will hardly carry conviction to the critical reader. The remaining chapters are devoted to enzymes and catalysts and secretion and glandular mechanism, and the equilibrium of colloid and crystalloid in living cells. While it must be acknowledged that the book, as a whole, contains a number of original and suggestive ideas, one cannot help wondering whether the methods employed for their experimental verification are so unimpeachable as to enable them to be recommended for the guidance of students of Honours Schools, for whose benefit, according to the Preface, the book is intended.

P. H.

Dairy Bacteriology. By Orla-Jensen, Dr. Phil, translated from the second Danish Edition by P. S. Arup, B.Sc., F.I.C. [Pp. xii + 180, with 70 illustrations.] (London: J. & A. Churchill, 1921. Price 18*s*. net.)

The author, who is professor of technical bio-chemistry in the Polytechnic College of Copenhagen, was formerly Director of the Swiss Experimental Dairy Farm, and has thus an intimate knowledge of the highly-evolved dairy industries of two countries, and the translation of the second Danish Edition now makes his accumulated experience available to English readers. The first fifty pages of the book are devoted to a brief introduction to the general properties and methods of cultivating bacteria and moulds. The second part of the book, which is more specialised and valuable, deals with the cleaning of milk, the normal and abnormal microflora of milk, and the preservation of milk; then follow chapters on Lactic Acid Fermentation in the Dairy Industry, on the microflora of butter, and on the ripening and defects of cheese; the final chapter deals with methods of judging of the cleanliness and freshness of milk, and makes a strong appeal, as indeed does the whole book, for the maintenance of a high standard of cleanliness in all stages of the handling of milk from start to finish. It is to be hoped that this translation may be widely read by those interested in the improvement of the milk supply of this country, and that it may contribute to the attainment of the sequence—healthy and clean cows, good milk, healthy children—with which the author concludes a very interesting and readable book.

P. H.

Fundamental Principles of Organic Chemistry. By Charles Moureu, Member of the Institute, and of the Academy of Medicine; Professor at the Collège de France. Authorised Translation from Sixth French Edition by W. T. K. Braunholtz, B.A., A.I.C. [Pp. xviii + 399.] (London: G. Bell & Sons, Ltd. Price 12*s*. 6*d*. net.)

In the author's Preface to the English edition he states that, while "there is no dearth of chemical treatises proper, . . . small initiatory books of a simple and at the same time high character are, however, rarer." The present

work includes an account of the chief theories of the present day, and a summary and very general consideration of the more important functions.

Close on one-third of the book is devoted to the Introduction and General Theories. The author includes a brief sketch of Dalton's Atomic Theory, and of Avogadro's law, which are not usually given in organic chemistry books. Perhaps the best section of this chapter is that on Stereochemistry, which is often so sketchily treated in introductory works, the Theory of the Tetrahedron being particularly well explained, as well as the sub-section on racemic compounds.

The second chapter considers the hydrocarbons, and is remarkable for discussing the aromatic compounds immediately after the aliphatic ones, contrary to the usual English method.

The third chapter is devoted to the functions containing oxygen, and here again we find the author abandoning custom and discussing the phenols immediately after the esters. The subsequent chapters are devoted to Functions containing nitrogen, organo-mineral compounds, heterocyclic compounds, and, finally, a chapter on dye-stuffs.

Though arranged somewhat differently from English works, the book is developed with a logical sequence well adapted for beginners.

C. C. R.

GEOLOGY

Geology of the Mid-Continent Oilfields. By T. O. Bosworth, D.Sc., M.A., F.G.S., F.R.G.S. [Pp. xiv + 314, with 8 plates and 24 text-figures.] (New York: The Macmillan Company, 1920. Price 16s. net.)

The Mid-Continent Oilfield, which comprises an expanse of territory stretching through Louisiana, Oklahoma, and Texas, forms an appropriate subject for geological study. Not only is its production greater than that of any other oilfield—it had supplied almost one-sixth of the world's output to 1919—but its geological relationships are such that it can be conveniently treated as a unit without a detailed discussion of adjacent fields. In this book the author gives an able summary of the already large volume of work on this region carried out by American investigators. The descriptive parts, which form a concise account of present-day knowledge of the area, are mainly based on the publications of the United States Geological Survey and of the Geological Surveys of the States concerned.

The introductory chapters contain a useful bibliography and give the history of the development of the region. These are followed by a brief but lucid account of the geological structure and the stratigraphy. The major portion of the book is occupied by a description of the individual accumulations, with abundant details both structural and statistical, while the final chapters are concerned with chemical and physical data relating to the character of the oil and with natural gas and the production of gasoline therefrom. In the interesting account of the helium which is found in the natural gas of certain districts and was utilised for airships during the war, a few inaccuracies may be noted. It is scarcely correct to state that helium has never been liquefied or that its critical temperature is $-264°$ C. The accepted figure for the latter is $-267{\cdot}7°$ C., while Onnes succeeded in liquefying appreciable quantities of the gas as early as 1908.

It is perhaps to be regretted that the book is so much descriptive of the work of others and that so little space is given to general discussion. Practically only the last chapter is devoted to the latter, and even this occupies but a few pages. So far as the origin of the oil is concerned, the author maintains that vegetable matter, imprisoned in the sediments, is the most probable source, while he is also in favour of the validity of "White's Law" regarding the relationship between the devolatilisation of the organic material and the occurrence of oil and gas.

A. S.

A Manual of Seismology. By CHARLES DAVISON, Sc.D. [Pp. xii + 246, with 100 figures.] (Cambridge: at the University Press, 1921. Price 21*s.* net.)

THE subject of seismology is of comparatively modern growth, and borders on many branches of science. A knowledge of the essential facts of the subject is necessary alike to the geologist, the geodesist, and the physicist. Dr. Davison has performed a useful service in collecting together these facts into a connected form in the volume under review, which may serve both as a textbook for the student and as a reference volume for the investigator requiring information on any particular point. For this purpose, the comprehensive references to original papers will prove of great value. In general, at the beginning of each chapter is given a list of the more important memoirs dealing with the branch of the subject under consideration, whilst detailed references required in each section are collected in a footnote at the end of the section.

The volume forms one of the Cambridge Geological Series, and it is, therefore, natural that particular attention is given to the geological side of the subject. It is perhaps to be regretted that, for the sake of completeness, more space was not devoted to the recent physical developments, connected with the propagation of seismic waves and the information to be derived from earthquakes as to the physics of the earth's interior, although other works are available which deal in detail with these branches.

A real need is met, and well met, by the publication of this work.

H. S. J.

Oil-Finding: An Introduction to the Geological Study of Petroleum. By E. H. CUNNINGHAM CRAIG, B.A., F.R.S.E., F.G.S. Second Edition. [Pp. xi + 324, with 13 plates and 20 text-figures.] (London: Edward Arnold, 1920. Price 16*s.* net.)

A COMPARISON of the second edition of this book with the first one shows that, while the arrangement is substantially the same, the volume has undergone considerable revision and has been much enlarged. In the earlier chapters on the origin of petroleum, the inorganic theories, however, still receive scant treatment, while the exponents of the "animal origin" are informed, at some length, of the utter failure of their theory when tested in the field. For the author, the "vegetable origin" is the theory most in accord with the facts; he holds most strongly the opinion that the mineral oil is derived from the decay of vegetable matter and owes its local concentration to the influence of geological structure. The importance of the theory of origin is urged so strongly by the author that it is all the more unfortunate that his treatment of the subject is polemical. In a book of this kind, it would probably have been better if the inorganic theories had not been dismissed in such a cavalier fashion and the "vegetable origin" discussed from a less antagonistic point of view. The whole subject is so far removed from any state of finality and the theories so far in advance of practical verification that it is doubtful whether a hard-and-fast adherence to any one mode of origin for all the diverse occurrences is wise.

Among the new chapters which have been added is one on natural gas and another on oil-shales and torbanites. In the former, comment is made on the general lack of recognition of the importance of gaseous petroleum, and the author points out that, in North America, "the value of the gaseous petroleum won and utilised in a year is far in excess of that of the oil." It is also interesting to note that, while the author is sceptical of the possibility of workable quantities of oil in great Britain, he is more hopeful about the occurrence of natural gas. In the latter chapter, emphasis is laid on the different nature of the "yellow bodies," which are the source of the oil in

the two rocks; in torbanite they are regarded as gels composed of incipient drops of petroleum and inorganic colloidal material, while in oil-shales the organic portion of the gel is inspissated oil rich in nitrogen and heavier hydrocarbons.

The revision of the later sections of the book, which deal with field work, has resulted in increased lucidity, which is all the more welcome as these chapters are intended for beginners. Of the value of the book, not only as a summary of present knowledge of the geological aspect of "petroleum," but also as a textbook for those likely to be engaged on geological work in oil-fields, there can be no doubt. In future editions this value would be enhanced by the inclusion of references to the literature, which are conspicuously absent from the present one.

A. S.

Field Methods in Petroleum Geology. By G. H. Cox, Ph.D., E.M., C. L. Dake, M.A., and G. A. Muilenburg, M.A. (Pp. xiv + 305, with 50 text-figures). (New York: McGraw-Hill Book Company, 1921. Price, 24*s*. net.)

In the oil industry, more than in any other, has the value of the geologist been recognised, and it is mainly due to this fact that we owe the already large number of books on the subject of oil geology. While most of the latter are concerned with the purely geological aspect of oil, and deal with the theories of origin and the mode of accumulation, the book under review is more directly concerned with the actual field work, especially from the point of view of surveying. Indeed, only some forty-five pages are devoted to any geological discussion, the remainder being devoted to a description of surveying instruments and the methods of utilising these in reconnaissance work and detailed surveys.

The instruments described include the compass, level, barometer, alidade and plane-table, and an adequate account of each is given. This is followed by the application of these to the determination of distance, elevation, and structure, while the last chapter deals with the details of field operations in general. The treatment throughout is clear, and very complete, while the numerous diagrams are of much aid in the understanding of the text. Not only is there an excellent balance between theory and practice, but the book abounds in hints of a very practical nature, many of which are the result of wide field experience. Although the book is intended for workers in oil-fields, it should prove very useful to geologists in general, especially those who have to carry out survey work in unmapped countries.

A. S.

GEOGRAPHY

Map Projections. By A. R. Hinks, C.B.E., M.A., F.R.S. Second Edition, revised and enlarged. [Pp. xii + 158, with 21 illustrations.] (Cambridge: at the University Press, 1921. Price, 12*s*. 6*d*. net.)

Mr. Hinks's book on Map Projections is too old a friend to need much in the way of review. But it is very satisfactory that a second edition has given the opportunity for it to be brought up to date, and enabled the author to give us the benefit of the experience gained since the book was first written.

Three new chapters have been added, dealing respectively with corrections and additions to previous chapters, projections for topographical maps on large scales, and the history of map projections. There are also some additional tables.

It is unfortunate that the "corrections and additions" could not be

embodied in the text instead of being given separately as notes, but one realises that they are not numerous or important enough to justify the extra expense which this would have involved, though they unquestionably add to the value of the work.

The book undoubtedly meets a real need for all who have to do with maps. While treating the subject scientifically, and with sufficient fulness for most purposes, it assumes no mathematical knowledge which is beyond the powers of the average student.

M. T. M. O.

Geography: Physical, Economic, Regional. By J. F. CHAMBERLAIN, Ed.B., S.B. [Pp. xviii + 505, with 192 illustrations and 17 maps.] (London: J. B. Lippincott Company. Price 15*s*. net.)

THE advertisement on the loose cover of this American publication is in the nature of a preface, and clearly indicates the scope and character of the work itself:

"No longer can our national life be one of isolation, even if we so desired. To perform efficiently and justly our part in world and national affairs we must know geography. The great need is for a fuller grasp of physical and economic geography and the regional geography of our own country."

The British or other foreign student who makes his study of geography from a purely academic or scientific point of view is liable in a book of this kind to find his standard of values quite upset and to feel considerable disappointment with a work which, from the point of view from which it is written, may be very valuable.

Again, the British teacher of geography, in discussing an American school textbook, is more often than not quite unacquainted with the character and standard of the teaching of the subject in the schools of America, and therefore does not know upon what foundation the teaching given in the book is based.

As a result of this, if the book is read as a complete textbook on the subject, without the careful co-ordination of the teaching of other subjects—especially physical science—quite false impressions may be produced.

On both of these grounds it is often difficult to recommend American geographies to English students unless the latter are sufficiently well trained to be able to study the text in a detached or critical manner.

One or two extracts from the book at present under review will illustrate these difficulties, and incidentally show how the purpose for which a geography is written may give colour to the information in it.

In the first chapter, dealing with man in relation to his physical environment, the following sentence occurs:

"Good pottery clays have led to the manufacture of world-famous pottery and porcelain at East Liverpool, Ohio, Trenton, New Jersey, Sèvres and Limoges, France and other places."

This is a case in which a purely economic point of view of the United States as a producer governs the whole statement to the complete exclusion of other considerations, such as the artistic and the historical aspects.

The ease with which a false impression may be created where the co-ordination with scientific teaching is not clearly outlined is illustrated in the following short account of the formation of glaciers:

"Snow-fields that endure for many years or centuries gradually become consolidated into ice, and slowly move from higher to lower levels. These

moving masses of snow and ice, together with what they transport, are called glaciers."

The book contains a number of such inadequate scientific descriptions, which, without experience on the part of the student or the aid of a good teacher, are liable to be very misleading.

Apart from these defects, serious as they are from one point of view, the book is full of valuable information as to the products and regional details of the United States, and for this purpose teachers will find it useful.

W. C. B.

BOTANY AND AGRICULTURE

Studies in French Forestry. By THEODORE S. WOOLSEY, with two chapters by WILLIAM B. GREELEY. [Pp. xxvi + 550, with frontispiece and 21 figures.] (New York: John Wiley & Sons; London: Chapman & Hall, 1920. Price 36s. net.)

FORESTRY in this country and elsewhere has been greatly influenced by the German system, whilst the study of French forests and forestry has been to some extent neglected. Mr. Woolsey's book therefore comes at an opportune moment, when efforts are being made to replenish the timber resources depleted during the war.

The argument for State Forestry is placed in the correct perspective, not so much as a financial investment as for the indirect benefits accruing to the community at large. The inherent characteristic of the French attitude is summed up in one sentence, "the sacrifice of present benefits for the future generation." Such long-sighted policy necessarily demands control of forest destruction by the State, and the legal status of French forests is by no means the least interesting of the aspects dealt with in these pages.

The almost universal appreciation of forestry by the people of France is a salient factor in estimating the growth of the official policy; nevertheless, the clash of national and local interests, as, for instance, in the eroded areas of the French Alps, has probably at times been as acute as in the history of control in any other country.

The tendency in the public forests is to grow high forest systems, with long rotation periods, necessarily yielding a comparatively low financial return. In the private-owned forests, however, coppice and coppice-with-standards prevail with a short rotation and a correspondingly higher return. Under private ownership there is a danger of over-cutting and impoverishment of the soil, but indiscriminate felling is prevented by the forest code, and in areas where erosion may result the regulations are extremely strict. On the other hand, the newly afforested land is relieved of taxes for a period of thirty years, either wholly or in part, according to the importance of such afforestation to the State.

The policy of the French forest service is well presented, together with a considerable mass of relevant data in a useful form. Most of the chapters are of a technical character, and are intended more especially for the professional forester; but there is, too, a great deal to interest the general reader, especially the history of the reclamation of the Gascon dune-belt, carried out between 1810 and 1870, and the afforestation of the Landes from 1875 onwards. The former involved an area of nearly a quarter of a million acres, and the latter 185,000 acres; moreover, both these large undertakings were in part carried out with the active co-operation of the landowners themselves. No less interesting is the well-known story of how the French Government has checked the processes of erosion in the Montane area of the South, despite the initial local opposition.

The success of these achievements must not alone be gauged by their

financial returns, though the forests of the Landes yield about 6 per cent. on the invested capital. The conversion of a fever-stricken and thinly populated waste into a healthy and prosperous region that has become a national asset is a feat of which any Government department has a right to be proud.

It is achievements such as these which command our respect for the French forest service, and in making the story of its work more widely accessible the author has earned the gratitude of all students of forestry and its methods. It is, however, rather as the considered opinion of an experienced American forester on French forests and forestry that the chief interest of these pages lies.

The last chapter gives an account of the work of the American forest engineers in France, and brings home to one not only the importance of timber in modern warfare, but also the enormous demands which could not have been met but for the efficiency with which the French forests had been managed. Some idea of the magnitude of the drain which the war entailed can be gathered from the fact that the Americans alone cut two million board feet per diem! Not without some reason does the author say that the American lumbermen, used as they were to the huge trees and extensive forests of the West, "took off their hats" to the service which raised the timber that it fell to their lot to cut.

E. J. S.

Breeding Crop Plants. By PROF. H. K. HAYES and PROF. R. J. GARBER. [Pp. xviii + 328, with frontispiece and 66 other illustrations.] (New York: McGraw-Hill Book Co., 1921. Price 21*s.* net.)

A BOOK conceived on practical lines, and containing a great deal of information useful both to the breeder and grower.

Most of the more important work on the genetics of crop plants is summarised, with references to a Bibliography occupying some twenty pages and containing citations to about five hundred original papers.

Despite the comparative infancy of the commercial application of genetical methods, one is impressed, in reading these pages, by the number and importance of its achievements. Rust-resistant strains of cereals with improved yield, "Mosaic" resistant beans, high-yielding cottons and tobaccos, are merely a few of the better-known examples of what have been already produced. It is not only as showing what has been done that the chapters devoted to particular species are useful, but also as indicating what there is still to do.

In most of the species treated the probable origin of the cultivated form is considered, with some account of the related wild types. The chief strains grown are summarised, as well as their ascertained behaviour under control.

In the historical résumé which serves as an introduction it is of interest to learn that an Englishman, one John Goss, had, in 1820, observed segregation in controlled crosses, although he did not formulate therefrom any generalisation such as we owe to Mendel. Only six years later Sargeret in France recognised dominance in melons.

A considerable amount of space is devoted to experimental methods in which both the theoretical and practical aspects receive due recognition.

E. J. S.

Lichens. By ANNIE LORRAIN SMITH, F.L.S. Acting-Assistant, Botanical Department, British Museum. [Pp. xxviii + 464, with 135 figures in the text.] (Cambridge: at the University Press, 1921. Price 55*s.* net.)

THIS is the second volume issued of the projected Cambridge Botanical Handbooks, the first being Volume I. of the Algæ. Its publication was delayed

owing to war conditions, and this delay has made it possible to include more recent interesting work on the subject-matter, *e.g.* it has allowed of the inclusion of a discussion and criticism of Dr. Church's paper on *The Symbiosis of Lichens*, a paper in which his views on the "subaerial transmigration" are extended to this group in a manner from which our author dissents.

The work begins with a comprehensive account of the history of lichenology: Schwendener's theory of the composite nature of the lichen-plant is here briefly mentioned. The more detailed consideration of this subject, of the cultural work of Bonnier and Möller, and of the recent views of the relationship of the component parts of the lichen thallus expressed by Elenkin, Danilov, and Paulson respectively, is left for the next chapter in which the gonidia and hyphæ are studied in detail.

Then follows a very full account of the structure of the thallus and of the structures—cephalodia, soredia, istdia, etc.—peculiar to lichens; the various methods of reproduction are next discussed. The vexed question of the sexuality of the fungal component of the lichen is examined at some length; and comparisons are made with similar fungal forms. The author sums up with Fünfstück's statement that fertilisation has not yet been proved, but only rendered very probable. The recent observations, not here mentioned, of Killian on Venturia, and of Dodge on a species of Ascobolus, go far towards increasing this probability. Chapters on the systematics and phylogeny of the group follow, the origin of the lichens and the relationship of the different fungal partners being considered.

A feature of the book which greatly impresses the present writer is the manner in which each part of the subject receives its meed of attention. This is exemplified in the space allotted by the author to the Physiology, Bionomics, Ecology, and Economics of this very interesting group. Economically, except for their use as dyes, lichens are not very important at present; but we are presented with a very interesting account of their uses in past times. Ecologically the group is an important one, lichens being prominent pioneer forms, and this part of the subject is fully dealt with.

The book is well illustrated, a number of reproductions of habital photographs being a welcome feature. A few of the figures appear to be a little too diagrammatic; and the chapter on Systematics, strangely enough, is not provided with any figures. A much-needed glossary, a bibliography, and an index are provided. The volume treats comprehensively of an important and interesting group, and makes an admirable addition to this series.

E. M. C.

Soil Conditions and Plant Growth. By E. J. Russell, D.Sc., F.R.S. Fourth Edition. [Pp. xii + 406, with 5 plates and 28 figures in the text.] (London: Longmans, Green & Co., 1921. Price 16*s*. net.)

As this work has already been reviewed we need only note the changes made since the last edition in 1917. That these are considerable may be gathered from the fact that the 243 pages of that edition have now extended to 406, though a great part of this difference is accounted for by reduction in the size of the page. Perhaps the most important addition to this valued classic is the provision of a more adequate index to the subject matter, and also an index to the authors quoted.

The changes in the general text are mainly those necessitated by the advances in knowledge of the last few years, notably in the sections on the micro-organic population of the soil, colloidal properties, soil acidity, etc.

It is satisfactory to learn from the Preface that it is intended to maintain the more general treatment here adopted, and that the detailed exposition of special branches is to be dealt with in a series of monographs under the editorship of Dr. Russell.

It is unfortunate that a book which ought to be in the hands of every student of agriculture or botany should not be accessible at a lower figure, and for this reason it is perhaps to be regretted that the plates, admirable of their kind though they are, were included in the present edition.

E. J. S.

The Introduction to the Flora of Natal and Zululand. By Prof. J. W. BEWS, M.A., D.Sc. [Pp. vi + 348.] (City Printing Works, Pietermaritzburg. Price 15*s*. net.)

ALTHOUGH termed a Flora, this is essentially a field handbook, not the least merit of which is its small size and thin paper, which render it convenient for the pocket and light for transit.

Doubtless for this reason no attempt has been made in the present state of our knowledge to give diagnostic characters for the 3,786 species dealt with, though one may perhaps look forward to the time when this may be possible on the lines of the British *Botanist's Pocket-book!*

Keys are furnished to the Families and Genera, of which latter there are over nine hundred. Under every genus all the species are listed, and for each of these notes are added respecting the normal habitat and not infrequently an indication as to frequency and the Zulu name.

The whole forms a useful manual which should prove of value both in the field and herbarium.

E. J. S.

The Garden of Earth. By AGNES GIBERNE. [Pp. xiv + 178, with coloured frontispiece and 47 illustrations.] (London: Society for Promoting Christian Knowledge. Price 6*s*. 6*d*. net.)

THIS is a popular account of some aspects of plant life intended for young people.

Information is conveyed in a pleasant style, but it is to be regretted that the text is not entirely free from the all too common error, in books written for the young, of carelessness in detail.

There is no justification, either for the sake of brevity or simplicity, for the inculcation of ideas which will subsequently have to be unlearnt. Examples are furnished by the statement (p. 31) that Diatoms probably move by means of cilia, or the inference which a child would naturally draw from a sentence on p. 54, that all plants with hypogenous stamens have unwholesome fruits.

The use of familiar terms for unfamiliar processes is only advisable where they are free from misconception. On these grounds exception must be taken to the use of the term "digestion" for carbon-dioxide assimilation, or the phrase "brain of the plant" in relation to the sense response of the root system.

On the other hand, we would commend the attention which is focused upon phenomena which from their very familiarity are apt to be considered lightly.

E. J. S.

ZOOLOGY

British Mammals. Written and illustrated by A. THORBURN, F.Z.S. Vol. I. [Pp. 84, with 25 plates in colour and pen-and-ink sketches in the text.] (London: Longmans, Green & Co. Price (two volumes) £10 10*s*.)

British Mammals forms a companion work to Mr. Thorburn's well-known *British Birds* and *A Naturalist's Sketch Book*. It is to be completed in two volumes, not procurable separately, the first of which has reached us. The plates, as beautifully reproduced as their predecessors, are accompanied as

before by a slight description of each species depicted. These consist mainly of citations from well-known authors, but even so we prefer them to the notes in Mr. Thorburn's *British Birds*.

The paintings are typical of the artist, a remark that makes praise superfluous. Perhaps the most pleasing of all in the volume under review is the Stoat in winter pelage; yet there are exceptions. The picture of the Wild Cat is the most disappointing that we have seen from Mr. Thorburn's gifted brush, while the pen-and-ink sketches leave one with the impression that the artist is not at all happy in the use of this medium.

Lovers and students of mammals will welcome this very fine series of accurate and beautiful pictures which fill a gap that has so long been felt in mammalian literature.

W. R.

The Natural History of South Africa. By F. W. Fitzsimmons, F.Z.S., F.R.M.S., etc. Four Volumes. [Vol. I, pp. xix + 178, with 58 illustrations; Vol. II, pp. xi + 195, with 48 illustrations; Vol. III, pp. xiii + 278, with 63 illustrations; and Vol. IV, pp. xix + 270, with 61 illustrations.] (London: Longmans Green & Co. Vols. I and II, 1919, price 9*s.* each. Vols. III and IV. 1920, price 12*s.* 6*d.* each.)

THE individual volumes of this series deal with the following: Vol. I. Primates, Chiroptera, Carnivora (including the Felidæ and Cynolurus); Vol. II. Carnivora (the remaining species); Vol. III. Ungulata; and Vol. IV. Insectivora, Rodentia, Cetacea, and Edentata.

The author is well known as an authority on the mammals of South Africa, and in his position as Director of the Port Elizabeth Museum has had extremely good opportunities for studying these forms at first hand. A glance at these volumes will at once convince the reader that he has made the most of the opportunities and is, in addition, a good photographer and first-class naturalist. A large amount of the matter is original, but previous accounts have been considered. The illustrations are very good, and most of them from the author's own photographs, and many of them give a clear idea of the natural surroundings of the animals as well as the creatures themselves. The reading matter is lucid, and contains valuable notes taken in the field. To most people there is a fascination surrounding the beasts of Africa, and this will be enhanced by a book like the present, which presents its matter in a very palatable form. The volumes are invaluable for naturalists visiting South Africa, and also for anyone wishing an accurate account of the mammals. Each volume is provided with a good index and an outline of classification, and another good feature is the inclusion of a list of all the species dealt with and references to the paper describing them.

The title seems to suggest that these volumes are to be followed by others, and in view of the enjoyment they have provided I sincerely hope this is the case.

C. H. O.

Mountain and Moorland. By Prof. J. ARTHUR THOMSON, M.A., LL.D. [Pp. viii + 176, with 7 illustrations.] (London: Society for Promoting Christian Knowledge, 1921. Price 6*s.* net.)

IN *Mountain and Moorland*, the latest addition to the Nature Lover's Series edited by W. P. Pycraft, we have as our guide Prof. J. Arthur Thomson, than whom it would be difficult to find a more distinguished and interesting companion in pursuit of nature study. The adaptable nature of all living creatures to their ever-changing environment is brought home to all readers of this little book, which we only reluctantly lay down when we arrive at the index. The author makes the rocks, streams, tarns, moors, and meadows unfold their mysteries and deliver up their secrets.

Whether it is the wonderful union of some symbiotic algæ and a fungus,

which we find in the lichens clinging to the rocks on the mountain, or the similar association between the common ling or heather and the fungus which surrounds its roots and renders life possible in the poor soil of the moors, Prof. Thomson reveals it all to us with a lucidity and freshness which we seldom find in books of this sort. The carnivorous Butterwort and Sundews, quaint plants which have become carnivorous owing to their hunger after nitrogenous food supplies, and which possess digestive processes very similar to our own, bring home to us how a touch of nature makes all living creatures kin.

The direct cause together with the process of the annual change of the stoat's coat from rufous to white is difficult to explain, as also is the more gradual change spread over periods of years in other carnivores which change their habitats and thereby alter their pelage for protective purposes. This gradual change of coloration is seen in the leopards frequenting the dry hillsides and low bush jungle when they pass into the highlands and forest belts in Africa. It is while living under the latter conditions that cases of melanism only occur. In such cases the pigment is intensified and the hair darkens. Likewise in a certain species of mongoose (*Herpestes gracilis*), a near relative of the stoat, we get a variation of colour from a dull brown to a brilliant red according to the nature of the soil which it frequents. Why is it that the black tips of the ears of the mountain hare and the stoat's tail never become white in these seasonal changes? Is senility alone capable of changing black into white?

The references to books for those who wish to extend their knowledge of any particular subject are most useful and are likely to be a great help to the student of nature, while the index at the end adds to the general usefulness of this admirable little book.

R. E. DRAKE-BROCKMAN.

Nature all the Year Round. By J. ARTHUR THOMSON, M.A., LL.D. [Pp. viii + 253, with 52 illustrations by ALICE M. DAVIDSON.] (London: The Pilgrim Press, 1921. Price 12*s.* 6*d.* net.)

ALL teachers, to whom this book should appeal, will not find themselves so happily situated as to be able to conduct their pupils along nature's by-ways and occupy their Sunday afternoons by training the powers of observation of their young companions in the open fields as we all would desire. But even those less fortunate dwellers in the towns can usually get away now and again for an hour or two in these days of easy, rapid, and cheap means of travel. In most of the larger towns the local museums, which should always be open to the public on Sunday afternoons, will very often, though not from a physical point of view as healthful, be as instructive as a ramble along the hedgerows. The great point which Professor Thomson's book brings out is that every month in the year has its own peculiar mystery to unfold, and these little secrets are as often as not only revealed for a few hours or days at one particular season, and not to be seen at any other time of the year.

All children have an innate passion for collecting, and it should be the aim of the teacher to direct this passing fancy or craze in the proper channels so as not only to educate their powers of observation but also their imaginations. Children should be taught to appreciate beauty early, for it is this study of the beautiful (and where can it be better found than in nature) that alone can bring joy and contentment into their lives. Organisations such as the Boy Scouts and the Girl Guides could scarcely do better than to make *Nature all the Year Round* one of their textbooks. Let us, as Professor Thomson says, cultivate the alert, scrutinising, probing habit of mind, and we shall find romance everywhere.

The peculiar habits of the hornbill during brooding are interesting, and more could be said about this quaint bird; but one thing might be mentioned, namely, that as soon as the young are hatched (there are one to two eggs

usually in a clutch) the parent birds break down the circular mud walls which they built up to block the entrance to the nest, and let the female bird emerge. This the latter does, looking very much the worse for wear. Her long tail has completely gone, and she has very few feathers left to cover her nakedness !

What a familiar sight the spider's web is, yet how many of us know the various stages in the weaving of the web of our common garden spider ? This wonderful inherited gift ! We are told that every young spider can weave a web, the very first time, which is true to type in a few hours ! But I must not quote any more from the pages of this delightful book. It should find a place in every house where there are young children.

After being charmed it is difficult to find fault, but some allusion to the *Anopheles* mosquito, the vector of malaria, might have been made, while in the drawing of the *Culex* larva the *Anopheles* larva might have been drawn alongside, showing the different manner in which it breathes at the surface of the water.

The drawings on the whole are good, and serve their purpose admirably.

Ours is a wonderful world, and *Nature all the Year Round* helps us to realise and appreciate it. R. E. DRAKE-BROCKMAN.

The Wit of the Wild. By ERNEST INGERSOLL. [Pp. 212, with 16 illustrations.] (London: George Routledge & Sons, Ltd.) Price 6*s*. net.

THIS is a collection of nature studies taken haphazard from Mr. Ingersoll's notebooks. We are suddenly transported from the enigmas of bird-life to the wonders of the ocean, and in each study we are subtly led on and suddenly left thinking. Truth has at times to take a back seat and give way to Mr. Ingersoll's poetic style, particularly on page 79, where he attempts to describe the courtship of lions. The reviewer's experience has been that, among those deer and antelope where polygamy is the rule, the females instinctively and meekly follow the strongest and stoutest of the males. Among the monogamous antelopes the pairs remain not only faithful for a season but until one or the other dies.

Among the animal partnerships, that between the rhinoceros and a species of starling (*Buphaga erythrorhyncha*) is one of the most interesting. Wherever one of these stupid pachyderms is found a small flock of the birds may be seen in close attendance, hunting in its ears and on its back and flanks for the blood-distended ticks with which their host is usually liberally supplied. For this consideration the birds in their turn warn the rhinoceros of approaching danger by suddenly leaving him with noisy exclamations which invariably rouse the stupid beast from his lethargy. This same starling is commonly seen in close attendance on buffaloes, domestic cattle, and camels.

When discussing whether animals commit suicide I find myself in agreement with Mr. Ingersoll. Most of us have at one time or another experien ed the almost uncanny instinct and intuition of some wild beasts. Doubtless if we live long enough among certain species one or two individuals will stand out among the rest as super-beasts, as did Lobo, the wolf-king; but I must admit that I personally have never met with a wild beast that has deliberately committed suicide, like Mr. Seton's mustang.

On page 164 it is stated that the umbrette occupies its huge nest for only one season. Unless disturbed, *Scopus umbretta* will occupy the same nest for several seasons. Along some of the boulder-strewn mountain-streams in Africa, where sudden spates are of frequent occurrence, this little stork builds its nest, in a suitably-forked tree along the bank, which looks to all intents and purposes just a huge mass of driftwood and plants, were it not placed well above the high-water mark.

The book is illustrated with photographs, which are all good; but they have little relation to the text, and seem to have been inserted as an after-thought.

R. E. DRAKE-BROCKMAN.

A Textbook of Oceanography. By J. T. JENKINS, D.Sc., Ph.D. [Pp. x + 206, with 42 illustrations. (London: Constable & Co., 1921. Price 15s. net.)

IN spite of our maritime supremacy, there is no modern textbook on Oceanography in English, as distinct from navigational manuals, since Maury's *Physical Geography of the Sea and its Meteorology*, published in 1861, which is hopelessly out of date, and therefore misleading for beginners. Dr. Jenkins's book is one which will add much interest and valuable aid to the studies of intending naval and mercantile marine officers; it will also be most useful to submarine telegraph engineering students.

The book is divided into five chapters, and there are useful appendices of conversion tables, and an *amplified* bibliography, such as is seldom found in any book, and it is a most valuable addition.

Detailed descriptions of the elaborate apparatus required for Oceanographical research, the methods of analyses of sea-water, and the pros and cons of the drift-bottle methods of investigation, some very interesting records of these latter experiments being cited; for instance, one such bottle drifted 10,700 sea-miles, and some set free off Cape Horn were found on the south coast of Australia and the west coast of New Zealand, suggesting the possibility of a current right round the southern oceans. The drift of derelict vessels also affords an interesting side-light on ocean currents; the famous case of a timber-laden schooner which drifted about for nearly three years, and covered a distance of not less than 8,000 sea-miles, is given.

The description of the conformation of the floor of the abyssal depths and the nature and composition of the various oozes covering them, and the paucity of the species of fauna inhabiting these great deeps, is also commented upon. In view of the interest being taken in the forthcoming attempt to ascend Mount Everest, which is 29,000 feet high, the book states, what may not be generally known, that a sounding taken off the Ladrone Islands was 31,620 feet.

Those interested in Arctic and Antarctic exploration will gather much new data on their favourite subject, for this book sets out to touch upon everything anent the "Seven Seas," and beyond.

The bathymetrical distribution of fish, abyssal as well as those of economic importance, the controlling factor, the plankton, governing it, and the thermal and other conditions contributing thereto—in other words, the metabolism of the sea—is expertly described, as would be expected of the author of *The Sea Fisheries* (reviewed in these columns January 1921. Pp. 502–4).

A mathematical dissertation on the tides is given, and the erroneous theories of early observers exploded. The description of the so-called "Gulf Stream" should be attractive and useful to the general reader.

Numerous references are given for the elucidation of points not strictly oceanographical.

The book is fully illustrated, there is an index, and the format is quite up to the usual standard of excellence of the publisher. A. W.

The Birds of California. By WILLIAM LEON DAWSON, Director of the Museum of Comparative Oology; Author of *The Birds of Ohio* and (with Mr. BOWLES) of *The Birds of Washington*. Parts I and II profusely illustrated with photographs, photogravures, drawings, and paintings, the last chiefly by Major ALLAN BROOKS, D.S.O. Three editions in parts. (The Birds of California Publishing Company, Santa Barbara and Los Angeles, 1921. Price $5, $2 and $1 per part respectively.)

THE original intention of the publishers of this work was to put it on the market in three volumes, appearing simultaneously. This plan, however, was discarded in favour of the one now adopted of issuing the work in parts, the first two of which have been published.

The work represents a stupendous undertaking. Three editions will appear simultaneously, the FORMAT DE LUXE being the most elaborate and the most pleasing. This itself is being produced in seven different editions, the differences lying mainly in the paper used and in the substitution of photographs of particular interest in one locality for those of particular interest in another. The de luxe format, in all its editions, represents the finest American work in book production.

The Book-lover's edition differs from the de luxe in having the margins much reduced and also the number of colour-plates and photogravures. The text and half-tone cuts are exactly the same.

The student's edition has the margins still further reduced, as also the colour and other full-page illustrations, and it is printed on cheaper paper.

The text illustrations and half-tone cuts of photographs are the same in all editions and are to number over a thousand. They are very well reproduced from excellent originals. The photogravures are, unfortunately, not so successful, which is a pity as they apparently include the pick of the author's photographs. In the two parts that have reached us there is a single actual hand-made photographic print. We understand that each issue of the de luxe format is to include at least one such. If subsequent prints are of the same quality as this first, they will form an attractive and important feature of the work. But of all the illustrations, the colour plates will probably make the strongest appeal to the bird student. Painted by Brooks, our greatest Canadian artist, they have been very successfully reproduced in the four-colour process by a London firm. Comparison of the pictures with actual skins leaves a most favourable impression. As to the paintings themselves, they are what we have learnt to expect from Major Brooks's talented brush. Their value is enormously enhanced by the fact that the artist is one of America's leading ornithologists—quite apart from his art—and is thoroughly conversant with his subjects in life. He is particularly fortunate in his portrayal of typical attitudes, a comparatively rare and most valuable gift.

The text is made up, for each series, of a concise scientific description, followed by an article on the bird in question. Whether we like the author's style or not must be a matter of personal taste. The amount of fresh information embodied in the text will make this publication a most important edition to American Ornithological Literature. The fact that the author is an ardent and exceptionally able oologist adds to the value of the book, for the eggs as well as the birds get their full share of attention. *The Birds of California* is therefore an all-round production, and must take its place, from every standpoint, in the front rank of the world's bird books. W. R.

Insect Pests of Farm, Garden and Orchard. By E. D. SANDERSON. Second Edition by L. M. PEAIRS. [Pp. vi + 707, with 604 figures.] (New York: John Wiley & Sons, 1921. Price 26*s*.)

SOME years ago it was estimated that the annual damage done by insects in the United States reached the extraordinary total of $1,000,000,000, and it is generally conceded that this was a conservative estimate and the actual loss is now greater in addition to the rise in the prices of such commodities. The study of these pests therefore becomes of great practical importance. The first edition of the present manual was issued in 1912, since when there has been a great increase in our knowledge of the pests and the method of combatting them, with the result that, while this volume is termed a second edition, it possesses several new chapters and a great deal of new matter.

The first chapters are general ones dealing with the nature of the injuries, structure of insects, methods of control, etc., and the subsequent chapters take up the most important crops and deal with the insects affecting them.

The whole book is lucidly written and copiously illustrated and should be assured of a wide sale. C. H. O'D.

Insects and Human Welfare. By Charles Thomas Brues, Assistant Professor of Economic Entomology, Bussey Institution, Harvard University. [Pp. xii + 104, 42 figs. and charts.] (Cambridge: Harvard University Press; London: Humphrey Milford, 1920. Price 10*s*. 6*d*. net.)

This little work is an attempt to present before the lay public some of the aims and methods of economic entomology in a form which will illustrate the biological relationships of insects to their environment. The professed entomologist will find therein little beyond what is already familiar information, but the general reader should be able to glean from its pages some idea of the manifold ways in which insects affect human welfare, and of the enormous importance entomology has in a country like the United States of America. Species of insects which are useful to man may be counted on the fingers of one hand, and, apart from a passing introductory reference to the honey bee, the silkworm, and the lac insect, Prof. Brues confines his remarks to injurious species. Chapter I is devoted to the relation of insects to public health, and it is in this aspect of the subject that the entomologist has so far achieved his greatest triumphs. Reference is made to the important rôle played by Mosquitoes in the transmission of malaria, yellow fever, dengue, and filariasis. The House-fly also comes in for its share of recrimination, and stress is laid upon its capacity for spreading the micro-organisms of typhoid and infantile diarrhœa; the Body-louse also is not forgotten in its relation to typhus and trench fevers. Rats and plague, Tsetse-flies and sleeping sickness, and Tick-borne diseases are also briefly touched upon. Chapter II treats of some of the more severe of the crop pests; Chapter III deals with forest insects and Chapter IV with household pests. In his concluding remarks the author refers to the outlook for the future. The biological method of reducing the numbers of injurious insects, by pathogenic organisms and predatory and parasitic enemies, is regarded by him as being the most promising field in which to speculate concerning future developments.

Although the book can only be regarded as the very briefest survey of the subjects with which it deals, nevertheless the information that is given is thoroughly accurate and up-to-date. Rather greater completeness might have been achieved had the author added a chapter on insects in relation to domestic animals. Both the letter-press and the illustrations are clear, although none of the latter call for any special mention. A. D. Imms.

ANTHROPOLOGY

Fishing from the Earliest Times. By William Radcliffe, sometime of Balliol College, Oxford. [Pp. xvii + 478, with 56 illustrations.] (London: John Murray, 1921. Price 28*s*. net.)

This is a book of worth—in a worthy setting. The author has done his painstaking work of research in a masterly way, and the printer and publisher have combined to place his results in an admirable form for his readers to judge of their quality.

Here is no manual on "How to Fish," and the tyro need not expect to find it a guide to style in casting a salmon-fly, nor to the relative merits of a "Jock Scott" and a "Butcher," any more than he will find in the famous "Book of Deer" instruction on methods of stalking.

The author has set himself to track up—as none has hitherto done—the earliest references, in classical and other literature, to fishing of all kinds, giving particular attention to: (1) rod fishing; (2) first notice of the jointed rod; (3) first notice of the "fly," whether artificial or natural; as also the different kinds of bait. A practical fisherman himself, and a scholar to boot, his conclusions are marked by a clear appreciation of the evidence available, and he never overstrains it in favour of a preconceived opinion. The book is learned but never dull, nor is it a mere classification of hooks and fishes.

Like his prey, the author is often lured into many devious byways, shady nooks and sunny corners, but, like the Pied Piper, he lures pleasantly his readers after him.

In olden days it was a " far cry to Loch Awe," more recently as far, or further, to Tipperary; but when Mr. Radcliffe gets his readers on the trail he paces them leisurely round the globe, and not only by the watery parts of it—ransacking with purposeful patience, and brightly commenting upon, the archives of Greece and Rome, of Babylon, Assyria, Egypt, Palestine, India, China, Japan, the country of the Aztecs, with peeps, *en passant*, at the recently discovered frescoes in the caves of Palæolithic and Neolithic man. He brings to light much that is of interest to the antiquary and folklorist and he explodes not a few common but erroneous beliefs. He exonerates Dr. Johnson from being the *fons et origo* of the saying about the " fool " and the " worm " ; frees Plutarch, who has been accused of despising fishing, from any such stain on his character, but, lest "all men should speak well "of fishermen, he reminds us that Plato and Byron did not admire them. Again, he shows the origin and purpose of the *jus primæ noctis*—for he gets queer fish in his net at times—like its analogue the *mercheta mulierum*, so frequent in charters, to be nothing more dangerous to chastity than a tax originating in the Church's desire partly to encourage continence, and partly to take under its ægis a practice of continence in vogue, and traced to the days of Tobias mentioned in the Vulgate version of the Book of Tobit, where the reference would suggest an earlier origin, probably also religious. Yet the popular error is hydra-headed, and though it has been dealt with at different times (e.g. *Archæologia Scotica*, vol. iii, p. 64) it appears in our own day in one of the stories by Neil Munro.

Mr. Radcliffe claims for Martial of the *Epigrams* the honour of " the very first mention of a fishing-fly." It occurs in Book V, Ep. xvii:

" Odi dolosas munerum et malas artes.
Imitantur hamos dona. Namque quis nescit
Avidum vorata decipi Scarum musca."

It is difficult to understand why, when the MSS. themselves read *vorata . . . musca*, so many editors of highest repute, though by no means all, read *vorato . . . musco*. Had there been a MS. reading *musco*, one might suppose that, according to the canon of textual criticism, *proclivi lectioni præstat ardua*, editors might choose *musco*, especially as *vorata* is a somewhat heavy word to go with *musca*. They might also be influenced by the fact that we have no mention, in the notices of various crafts, of any artificial fly-makers, although *Quis nescit* would imply a prevalent use of that lure. But as evidently none of the MSS. gives *musco*, which, as a conjecture, is but shakily supported by information derived from Pliny and Athenæus, who, in turn, derive from Aristotle, and as the point of the epigram is rather enhanced by the reading *musca*, Mr. Radcliffe's reasoning would appear to be sound.

He thinks and searches for himself, not accepting without proof the *ipse dixit* of even Paley, and if classical editors shrug their shoulders at this little parody—" Nor do these Olympian editors, who sit beside their proof-sheets, and whose notes are hurled far below them in the valley, condescend to explain to us poor gropers after light how moss to a sea-fish like the Scarus can be of value as food "—they can anathematise him in the words of the Accadian folk-saw:

" Thou dost evil,—
To the Everlasting Sea
Thou shalt go."

But then, as Mr. Radcliffe, at the beginning of his book, adopts the wish of Andrew " of the brindled hair "—

" Grant that in the shades below
My ghost may land the ghosts of fish,"

they will all be satisfied.

Whether one agrees with Mr. Radcliffe in all his conclusions or not—and they are so carefully considered that they are difficult to resist—one can unfeignedly admire the wealth of information and the critical acumen he brings to bear on his subject. Nor can one fail to be impressed with the extraordinary diligence he has shown in presenting his readers with quotations numerous and apt, bringing to light many they had known and forgotten, and many from sources difficult of access of which even well-read scholars need not blush to acknowledge their ignorance.

One beauty of this book is its discursiveness. *Ne sutor supra crepidam* is a good rule generally, but in the "contemplative man's recreation" it would be boredom. Thoughts *will* rise like trout in June, and Mr. Radcliffe captures not a few, and lays them out for our inspection and admiration. Fish of many kinds, toothsome and t'other sort: Fish Gods; Fish Symbols; the "mighty" (to borrow Izaak's epithet) fish-suppers and sauces of Imperial Rome, with their "mighty" cost which pauperised many a "mighty" man; the power of the chefs, who boasted, not always vainly, that they did more for the success of international congresses than the appointed delegates; the shrewd wit—*calliditas*—of the ancient fishermen, and their general poverty,—all provoke to discursiveness, and the author and his readers are the better for letting themselves "go."

In a fine chapter on Chinese Fishing we learn that Confucius was a sportsman as well as a sage—"The Master angled, but did not use a net; he shot, but not at birds perching"—and in every chapter of this book the sage shines out as clearly as the sportsman.

From all lovers of the "gentle art," especially if they are scholars, as also from those who, though they know "little Latin and less Greek," are lovers of big and little fishes, I bespeak a cordial welcome for this admirable book, which will assuredly become one of the "classics" on Angling.

DONALD MACRAE.

Man and his Past. By O. G. S. CRAWFORD. [Pp. xiv + 227, with a frontispiece, 2 maps, and 10 other illustrations.] (Oxford; H. Milford, 1921. Price 10*s*. 6*d*. net.)

THIS well-written volume consists of nineteen chapters dealing with various aspects of the study of mankind, in the widest sense. The book consists of discussions rather than descriptions, and indeed the chapters are really a series of connected essays. Thus chapter iii answers the question, "What is archæology?" Chapter iv discusses the relations of archæology to history; chapter v is concerned with archæology and anthropology (in the confined sense); and later chapters deal with methods of archæology, distributions, excavations, old roads, museums, and other matters. All the chapters make pleasant reading, and are redolent of culture; but the book contains little that will be new to archæologists or anthropologists. The author is rightly anxious for a synthesis of all the various branches of the study of mankind, for which he suggests the name "andrology," to distinguish it from anthropology in the confined sense. In Mr. Crawford's hands archæology is by no means a merely academic science; it has a living relationship to sociology; he has attained a truer perspective than ordinary historians, and his criticisms of the ordinary methods of teaching history (chapter ii) are very timely. In his discussion of museums, he puts forward sound ideals, but his criticism of existing institutions scarcely does justice to the improvements of the last twenty-five years, which have been brought about so largely by the Museums Association.

A. G. T.

Invertebrate Palæontology. By HERBERT LEADER HAWKINS, M.Sc. [Pp. xix + 226, with 16 plates.] (London: Metheun & Co., 1920. Price 6*s*. 6*d*. net.)

THE sub-title describes this book as an "introduction to the study of fossils,"

and it is intended for the general reader as well as for students of geology and zoology. It is not a mere textbook, but is, as the author justly claims, a serious introduction to the general results of the science of palæontology, and to the meaning and implications of these general results. Part I, including five chapters, deals with the "materials, methods, and principles" of palæontology. Part II is entitled "Historical Biology," and consists of six chapters dealing in a general manner with the respective invertebrate faunas of the pre-Cambrian, Proterozoic, Deuterozoic, Mesozoic, and Cainozoic eras, and finally with "crises in evolution." The book has much less purely descriptive matter in it than an ordinary textbook, and much more discussion of general problems. Indeed, whether intentionally or otherwise, the author assumes on the part of the reader a very considerable knowledge of geological and zoological facts—and of geological and zoological terminology—and we think the book is too difficult for the general reader. It is also not likely to interest many geologists, except such as are also avowedly palæontologists. But to zoologists it should have real value. Mr. Hawkins writes throughout from a thoroughly evolutionary point of view, and advanced zoological students, who wish to understand the kind of illumination that palæontology throws and may throw upon zoological theory, would do well to consider carefully what the author has to say. In particular, the last chapter in Part I, which is entitled "Biological Palæontology," is full of suggestive ideas, which give much food for thought, whether or not the reader agrees with all the suggestions thrown out. This important chapter could have borne, however, a fuller infusion of modern zoological philosophy, and, among other things, the author appears to accept too readily and too generally the theory of recapitulation in development. The discussion of hemeræ and "morphogenetic modes" in chapter iv of Part I should be very valuable and suggestive to zoologists. Part II is clearly written, but would have gained by less meagre references to foreign strata, even though the book is intended for English readers. For instance, it is unfortunate that the Algonkian should have been dismissed in a few lines. The illustrations are good, and the publishers must be commended for the low price at which the book is produced—which is almost "pre-war." A. G. T.

Totem and Taboo. By PROFESSOR S. FREUD, LL.D. Authorised English translation by A. A. BRILL, M.D. [Pp. xii + 268.] (London: G. Routledge & Sons, 1919. Price 10s. 6d. net.)

THIS new work by Professor Freud consists of a series of four essays, which were originally published independently, but which all have the common object of pointing out the "resemblances between the psychic lives of savages and neurotics." The author explains that the essays represent his "first efforts to apply view-points and results of psycho-analysis to unexplained problems of racial psychology." The four chapters respectively deal with: (1) The Savage's Dread of Incest; (2) Taboo and the ambivalence of the Emotions; (3) Animism, Magic, and the Omnipotence of Thought; and (4) The Infantile Recurrence of Totemism. The author is able to deal with taboo much more thoroughly and—as he himself acknowledges—much more successfully than with totemism. This is no doubt due to the fact that the phenomena of taboo are much better understood by ethnologists than is totemism. The second essay is extremely interesting, and the parallel between the symptoms of what are called "compulsion neurotics" and the superstitions of savages cannot be regarded as other than most striking. The same inner anxiety and dread, the same meticulous observance of meaningless rules, are to be found in both cases. And the instances of savage superstitions given in this chapter bring home forcibly to the reader what a painful mass of illusions, what a mountain of dominating conceptions that bear no relation to objective reality the savage carries about with him

through life. The phenomena of taboo appear to possess the quality of an "ambivalent" emotion—a strong desire to perform an action and a contrary and more powerful impulse repressing that desire. The discussion of this question in the second essay is very suggestive and enlightening. The other chapters carry less conviction, and in particular some of the inferences in the last chapter will probably strike most readers as very far-fetched. There are a few printers' errors, *e.g.* "world" instead of "word" on page 112.

A. G. T.

ENGINEERING

A Treatise on Airscrews. By Whyrell E. Park, A.R.C.Sc. (The "D.U." Technical Series.) [Pp. xii + 308.] (London: Chapman & Hall, 1920. Price 21*s*. net.)

This publication is typical of the present somewhat breathless transition in this country from regarding the mathematically equipped engineer as a freak, or a joke, to a somewhat exaggerated notion of his possibilities.

Mr. Park, as a Whitworth Scholar, received a training specially appropriate to an author in this series, inasmuch as his mathematical equipment is for use and not for original work. His mathematical sections reproduce in a satisfactory manner the better known variations of Drzewiecki's method, and his notation and symbols are much better selected than usual, though we still look in vain for any approach to the agreeable uniformity established in German literature.

But when he writes on the physical principles his of subject his training leaves much to be desired:

"... from the beginning design has assumed the nature of a compromise between aerodynamic requirements and engine efficiency."

"... the whole of design theory has been developed by mathematical manipulation. ..."

"In the special case, when the airscrew is rotating and moving forward in such a manner as to produce no disturbance of the air, and consequently no thrust, the air behaves as a solid nut, and the analogy to bolt and nut action is complete. ..."

"... a Newtonian Medium of small independent particles which is mathematically continuous."

The reviewer has marked thirty or forty passages such as the above, some of which merely require rewriting to make them clear, but others of which seem to show lack of clear view of the physics of the phenomena.

The torque power coefficients in figures 13, 14 are spaced very unevenly, whereas those in fig. 21 are spaced more evenly, while the principles of interpolation would lead us to expect still more even distribution still. In fact, the numerical illustrations give the impression of having been made up a little artificially. The words "practical," "theoretical," "hypothesis," etc., are used with considerable author's licence, sometimes almost in opposite senses, in different places.

Notwithstanding these limitations in the author's equipment, there is much in the book which, if not original, is at least more accessible than elsewhere.

He has the immense advantage of having an accumulation of test experience at his disposal, and this may always be relied upon to keep the tedious computations of actual design from any disastrous error. And the very appearance of inconsistency is often a sign of freedom from slavery to an inadequate hypothesis.

To recount in detail the valuable aspects of the book is of course impossible, but the student who has conscientiously worked through it will certainly be equipped to take a place in the design room, though not perhaps in the laboratory.

The second part, embodying the author's ripe experience in giving shop

instructions, is a really good epitome of shop practice, though of course other methods than some of those laid down have their advocates. Those who in the early days of aviation had to find out their rules of construction by trial will best appreciate how easy it is, up to a point, to become a designer nowadays. Beyond that point the best brains are still required.

The mechanical production of the book is of the highest quality as to printing, diagrams, and mathematical symbols.

The use of heavy type for emphasis is not always happy, and might be restricted with advantage.

A. R. L.

Electrical Engineering. By T. F. WALL, D.Sc., D.Eng., A.M.Inst.C.E., A.M.I.E.E. [Pp. xi + 491, with 463 diagrams and illustrations.] (London: Methuen & Co., 1921. Price 21s. net.)

EVERY author of a text-book on electrical engineering seems, in writing the book, to have an aim and object which, at least in his opinion, differentiates his work from all the other and numerous textbooks that have already been written on the subject. The author of the volume under review is no exception to this generalisation, although his chief object in writing the book—viz. "to give a survey of the principles of electrical engineering which shall be as complete as possible in one volume of moderate size"—is one which is doubtless in the mind of every writer on this branch of engineering.

To what extent he has succeeded in following his aim, may perhaps best be judged from a brief résumé of the contents of the book. It is divided into six sections, each of which is more or less complete in itself, and deals with one of the fundamental divisions of the subject. These are: I. Static Electricity; II. Magnetism; III. Direct-Current Elecricity; IV. Electro-Magnetism; V. Alternating Currents; and VI. Units. These sections contain chapters dealing with the following subjects: First Principles; Some Deductions from Coulomb's Law of the Force between Electrified Particles; Potential-Capacity; the Dielectric and the energy of the electric field; Electrical Machines and Electrometers; Coulomb's Law of the Force between Magnetic Poles; Magnetic Induction—Magnetic Properties of Iron and Steel—Effects of Temperature on Magnetisation; Diamagnetic Substances—Permanent Magnets; The Earth's Magnetic Field; Electric Resistance; Electrolysis; Electric Accumulators; Thermo-Electricity; Magnetic Effects of Electric Currents; Electromagnetic Induction; Galvanometers, Fluxmeters, Oscillograph, Wattmeters, Ammeters, Voltmeters, Frequency Meters; Single, Two, and Three Phase Alternating Current Systems; Harmonic Analysis; Magnetic Fields due to Alternating Currents; Use of Complex Quantities in A.C. Problems.

Although grouping together of the elementary with the advanced, of the introductory with the more specialised, will doubtless appeal strongly at the present time to the student's pocket, it is open to debate whether such a course is a wise one. More than half of the volume is devoted to the earlier sections on the fundamentals of electricity and magnetism, and to the parts of the subject which belong almost more properly to the elementary physics laboratory than to the electrical engineering lecture room, so that the serious engineering student will still require further and more specialised books to help him over the later stages of his work. The general appearance of the book savours of a mathematical treatment of the subject, although in no case is this treatment unduly difficult; but, at the same time, it seems of rather doubtful utility to the student to discuss these elementary portions in terms of the integral calculus. The average university student takes his electrical and mathematical courses at the same time, and will not reach a useful stage in the calculus until he has dipped fairly deeply into electrical work.

As a reference book to the student who is finishing his course, as well as to the electrical engineer, the book should prove distinctly useful, since it sets out clearly the underlying principles of the subject, and the manner in which the more advanced portions are built up on them—details which are often apt to be forgotten in the later stages. One might pehaps wish for a somewhat more detailed treatment, with practical details, in parts; but every work must necessarily have its limitations in some directions. The diagrams are good, and the book has been well prepared and printed, while it is not over encumbered with historical references which merely clog up the main subject.

PHILIP R. COURSEY.

Wireless Telegraphy with Special Reference to the Quenched Spark System. By BERNARD LEGGETT, A.M.I.E.E. [Pp. xv + 485, with 230 plates and diagrams.] (London: Chapman & Hall, 1921. Price 30*s*. net.)

As the author states in his Preface, the literature of radiotelegraphy published in English has in the past been lamentably lacking in anything more than mere outlines of the Quenched Spark "system," whereas it has dealt *in extenso* with other types of apparatus. The present work attempts to overcome this deficiency by presenting a textbook of Wireless Telegraphy and Telephony with particular reference to quenched spark apparatus. The reason for the aforementioned deficiency is not hard to find—and is probably due to at least two main causes: firstly, the commercial interests of the leading English wireless company, which were directed in other channels; and, secondly, national prejudice against a "system" that not only originated in Germany, but received practically the whole of its development in that country—or, at least (and what is practically the same thing), under German engineers working in other countries. What development work has been carried out here has received little publicity in English, not only for commercial reasons, but mainly because, outside of the ordinary electrical press, this country, until quite recently, possessed no technical radio journal in which such work could be chronicled.

Although quenched spark apparatus has now been adopted extensively in many countries, and adapted to the needs of several different wireless companies, it may be noted that, with the exception of certain British Army apparatus developed by the Signals Experimental Establishment during the war, the bulk of the instruments described and illustrated in this book are of Telefunken design—so that the volume really accomplishes little towards dispelling the all-prevalent opinion as to the Teutonic nature of the whole "system."

The just criticism levelled in this book against most British pre-war literature as to its biassed view-point cannot unfortunately be removed from the present work, since the author apparently holds the opinion, shared by many other writers in the radio sphere, that the particular "system" with which he is most familiar is the only one that is of much use at all for practical work. It would seem that the perfectly impartial, and at the same time comprehensive, wireless textbook has yet to be written. In common, too, with many other writers on similar subjects, the author appears to be well informed as to the latest developments of the company with which he is, or has been, connected, but appears to be very much out of touch with the doings and general practice of other companies. Some of the comparisons drawn under these conditions are perhaps, therefore, not altogether justifiable.

The author states that the work under review has been compiled from a series of technical notes taken over a period of several years. It is probably as a consequence of this that the treatment is in some cases somewhat

scrappy, while the style of the whole book would have been improved if more careful revision of these notes had been made before putting them together. Particularly is this noticeable in the Introductory Chapter—and the more unfortunately so since it is from the earlier chapters that the reader often unconsciously forms an opinion as to the merits of the book as a whole. In a volume that is in any case a bulky one, the reader does not wish to be wearied by unnecessary (and almost verbatim) repetitions.

But to turn now to a more detailed résumé of the scope of the book—the Introductory Chapter gives a brief account of the chief landmarks in the development of the quenched spark "system." Next follows the commencement proper of the technical description of the wireless apparatus itself, and devoting himself entirely to his own "system," the author plunges straight into the theory of h.f. oscillation production by condenser discharges, and the theory of quenching. In Chapters III to VIII details are given of the apparatus used for transmission and reception as manufactured by the Telefunken Company (the Gesellschaft für drahtlose Telegraphie), and Messrs. Siemens Bros. These descriptions are accompanied by many excellent illustrations, showing not only the external appearance of the instruments, but details of their construction as well.

With Chapter IX begins a section of the book devoted to the practical applications of quenched spark apparatus for land and ship installations, their applications in warfare and in aeronautics. These chapters are also liberally interspersed with good photographic illustrations, which add greatly to the value of the book to the student unfamiliar with the appearance of such commercial apparatus. The care and maintenance of quenched spark apparatus is dealt with in detail in Chapter XIV—a chapter which is evidently written almost entirely for the wireless operator.

Chapters XIII and XV are the only two which deal to any serious extent with apparatus other than of the Telefunken type—the former discusses continuous wave telegraphy and telephony (including valve, arc, and high-frequency alternator apparatus), while the latter sets out some theoretical ideas as to the mechanism of wave propagation and the variations in directional effects and in the intensity of the received signals.

The work, covering as it does nearly five hundred pages, is of too bulky and too comprehensive a nature for detailed criticism of its contents beyond the general remarks as to its scope that have already been made. In keeping with the aim of the book the bulk of the bibliographical references given at the end of each chapter are to German works or articles, and this limitation detracts somewhat from the general usefulness of the volume to the student, since to one unacquainted with other work in this field an entirely false impression might be given. Since these references are presumably intended for the edification of readers of the student class, it is a great pity that more care has not been taken in their compilation and checking. The colloquial and often inaccurate abbreviations of these references sometimes used in conversation should also scarcely find a place in print in a volume of this character. It is to be hoped that in future editions these and other blemishes and inaccuracies may be rectified, and that the author will include more up-to-date information with regard to other types of apparatus when comparing them with those of the quenched spark type.

The general appearance of the book is excellent with the exception of the mathematical portions, where there is some lack of consistency in the symbols used, and between the symbols and the diagrams to which they refer. The well-known internationally recognised conventions with regard to such symbols might have been adhered to with advantage, and in particular, after perusing Chapter II one feels tempted to suggest that in this connection the author might profitably have studied some of the English scientific publications which he apparently professes to despise.

Aside from these criticisms, the general scope of the volume is excellent. Would that there were more equally comprehensive ones on other branches of radio work!

PHILIP R. COURSEY.

MISCELLANEOUS

Archimedes. By SIR THOMAS HEATH, K.C.B., K.C.V.O., F.R.S., Sc.D. Men of Science Series, edited by S. Chapman, F.R.S. [Pp. 58.] (London: Society for Promoting Christian Knowledge. New York: The Macmillan Co., 1920. Price 1*s*. net paper; 2*s*. net cloth.)

SIR THOMAS HEATH and the Society for Promoting Christian Knowledge are to be congratulated on the publication of this very able little work. No branch of history is really more edifying and interesting than the history of mathematics; and when we reach a (still) higher stage of civilisation men will recognise this fact. The advance of mathematics is probably coincident with the advance of human intelligence—when the former ceases, the latter is probably temporarily in decline. Actual science is nothing but reasoning based on measurements, and is, therefore, essentially mathematical; and until it becomes mathematical it is only sub-science—though this is not nothing.

Histories of mathematics are apt to be too cursory to be really useful. This small work is so ably done that it describes not only the results of Archimedes but exactly how he reached them. Undoubtedly he was the culminating man of science of the ancient civilisation of Europe—and Newton was perhaps the only other man of science who can be classed with him. It is wonderful to note how nearly Archimedes came to the Calculus. The proposition regarding the area of a triangle in terms of the sides was his, and not Heron's, as usually stated. (By the way, Heron's name is not in the chronology at the end of the book, nor is that of Claudius Ptolemæus, of Alexandria, nor those of several others.) Without using a single figure, Sir Thomas Heath gives perfectly lucid demonstrations of all Archimedes' propositions which he deals with.

We noticed some time ago the same author's admirable *Euclid in Greek* (Cambridge University Press, 1920). Like the work mentioned above, it not only fulfils the promise of its title but also delights us with a lucid history of ancient geometry. As the author says, there is no subject "better calculated than the fundamentals of geometry to make the schoolboy (or the grown man) *think*."

Handbook of Metallurgy. By Prof. CARL SCHNABEL. Translated by Prof. HENRY LOUIS. Third Edition, revised by the translator. Vol. I: Copper—Lead—Silver—Gold. [Pp. xxi + 1171, with 705 figures.] (London: Macmillan & Co., Ltd., 1921. Price 40*s*. net.)

PROF. CARL SCHNABEL'S *Handbook of Metallurgy*, through its translation by Prof. Henry Louis, is well known to English metallurgists, and the third edition, which is now issued, is certain of a favourable reception. It is not, as in former cases, simply a translation, but a revision carried out entirely by Prof. Louis himself; because, as he points out, the chief recent advances in metallurgical practice having taken place in English-speaking countries, there was no reason to wait for the publication of the third German edition. The matter added by Prof. Louis is of distinct value and in every way excellent, with the result that this book is a marked improvement on previous editions.

The first volume, which has now been published, consists of four parts covering the metallurgy of copper, lead, silver, gold. The first, on copper,

contains additional matter dealing with calcination furnaces, blast-roasting, and the Bessemer process of copper extraction ; but it cannot be said to be fully up-to-date. The explanation, which is given by the reviser, is that the war intervened during the work of revision when much of the book had already been prepared and was in type. The effect of this is more liable to be noticed in the case of a metal such as copper, the smelting practice of which has been undergoing various changes during the last few years, and details of some of the improvements have only recently become available. However, this section contains much valuable information and will be found of distinct use. It is hoped that Prof. Louis will be able to give a brief account of these modern developments as an appendix to the second volume. The portion devoted to lead has been enlarged by descriptions of the Savelsberg process, pot-roasting, blast-roasting without lime, and down-draught processes. It is noticed that the American form of spelling the word " draught " has been adopted, which can scarcely be commended. Most useful additions have been made under " Gold " by the clear and concise accounts of the cyanidation process, the all-slime process, fine-grinding and various methods of classification, and now this section which, in previous editions, has been inadequate, is greatly improved.

Much obsolete matter has been deleted, but it would have been an advantage if the abridgment of descriptions of many of the older processes had gone further. However, it is recognised that this might have involved much rewriting which perhaps would not have been justified ; but, on the other hand, it would have given more space for new matter, an important consideration in revising when the bulk of a book may not be unduly increased. A possible alternative would have been to have used smaller type for these older descriptions, and this would have helped the student to realise their true importance at the present time.

The volume, which has been enlarged by about forty-five pages, maintains the form and standard of excellence of the former editions ; for it has been carefully prepared, is well printed and illustrated, and deals with the general metallurgy of the above-mentioned four metals in a comprehensive manner.

Laboratories, Their Planning and Fitting. By ALAN E. MUNBY, M.A., F.R.I.B.A., with an historical introduction by SIR ARTHUR E. SHIPLEY, C.B.E., Sc.D., LL.D., F.R.S. [Pp. xix + 220, with 165 figures and a frontispiece.] (London: G. Bell and Sons, Ltd., 1921. Price 25*s.* net.)

ALL those who have at any time been concerned in the building, extension or remodelling of a science department will appreciate the usefulness of an authoritative book on the design and equipment of such buildings. The task demands the co-operation of the scientific staff and the architect for its success, and too often the first are quite ignorant of the difficulties and limitations imposed on the latter. The science man probably has very definite views on certain aspects of the work, and enters upon them with enthusiasm ; but he is likely to be very much worried by other equally important details which await on his decision and which he feels are quite outside his experience and, perhaps, his interest. Such points as the positions of benches, cupboards, radiators, gas taps and wall plugs, or the design of fume cupboards, drains, bench tops and lecture tables must be settled with an eye on the demands of the distant, as well as of the immediate, future. It is here that he will be grateful for the assistance of Mr. Munby's book ; it will probably help him more than many visits to the show departments in other towns and countries.

The arrangement of the book is as follows : First a chapter dealing with building schemes in general, with remarks on the functions of the building

committee and a method for making a rough estimate of costs: next, three chapters devoted to the special requirements of chemistry, physics and biology (including geology): then a chapter treating in some detail of laboratory services, such as gas, water, electricity, ventilation and drainage: and finally a set of detailed drawings of recent designs, both for schools and technical colleges at home and abroad. The style is restrained and undogmatic; it is perhaps too quiet. A few examples of bad design, with a critical commentary, might have added to the value. For example, it is pointed out quite casually (p. 9) that the laboratory and not the lecture theatre should be placed on the noisy side of the building, an obvious fact, indeed; but the reviewer has had a good deal of experience in lecturing to the strains of a barrel organ, and, consequently, feels that the matter might be put more strongly—the opportunity was provided by a certain design shown in the last chapter of the book.

So many neat ideas are brought to the reader's notice that to select two or three for special reference here is almost unfair to the author. We may, however, perhaps be permitted to draw attention to the lecture table "footlights" used in Prof. Kent's physiological theatre at Bristol, to the use of aniline black covered with a lead paint as a protection for the tops of ordinary pine-wood tables in certain schools in America, and to the V-shaped tables for the use of classes requiring microscopes, described by Sir Arthur Shipley in his witty introduction.

The book should find a place in the library of every growing institution, and should earn for its author the very sincere gratitude of all those who have occasion to make practical use of its contents. The reproduction of plans, printing, and general get-up of the work are excellent, and it has a sufficient index.

Critical Microscopy. By ALFRED C. COLES, M.D., D.Sc., M.R.C.P., F.R.S., (Pp. viii + 100, with 8 illustrations.) (London: J. and A. Churchill, 1921. Price 7*s*. 6*d*. net.)

THE author regards this book as a supplement to, rather than a substitute for, the standard works on the subject already in existence. From this standpoint it is possible to justify the production of such a work; otherwise it would be difficult, as there is little effort to explain even the elementary principles of microscopy. The book consists in large part of abstracts from published papers or personal communications from Mr. E. M. Nelson, to whom the well-known term "Critical Microscopy" is due.

The first part consists of a description of the Microscope and its Accessories. Then follows a section on Manipulation, such subjects as the adjustment of the sub-stage condenser, and dark-ground illumination rightly receiving a full share of attention. The selection of suitable objectives for bacteriology or pathology—and these branches are particularly, in fact almost exclusively, referred to in the book—is well treated. The author insists on the great advantage of using such a lens as an 8 mm. apochromatic for medium power work. He sets out in detail the features of such a lens, and mentions that for dark-ground work it will do all that is necessary, even in the identification of such objects as malarial parasites in blood films, a claim that is fully justified. Strong objection is taken to the use of objective changers in place of the more common, but less efficient, triple nose-piece. The only conclusion to be drawn is that this opinion has been formed without any, or sufficient, experience, as there is no comparison between the two appliances for accurate work. Preference is also given, even to the exclusion of all other light sources, to a paraffin lamp. Again, it can only be concluded that sufficiently exhaustive efforts have not been made with other illuminants.

The instructions for centreing the light and illuminating appliances follow along orthodox lines, and the question of tube-length adjustment is adequately dealt with, the methods being those described by other workers. The examination of stained preparations by a dark-ground method is described and recommended, but the errors of interpretation that can arise under such circumstances are apparently not realised.

The book has undoubtedly been compiled with the object of providing a guide for the uninitiated, and in many respects it will fulfil its purpose; but it suffers considerably from the constant recurrence of quotations from the writings of others and consequent lack of cohesion.

J. E. B.

Optical Theories. By D. N. MALLIK, B.A., Sc.D. Second Edition. (Pp. ii + 202). (Cambridge: at the University Press. 1921. Price 16*s.* net.)

THE fact that this book has reached a second edition in a comparatively short time is in itself sufficient evidence of its value. The treatment of the subject follows the same lines as in the earlier edition, with the important addition of a brief but lucid account of the theory of relativity and the quantum theory. The opening paragraph in the book indicates its scope and is as follows:

"A complete theory of optics has to furnish an adequate account, not merely of the nature of light, but also of the mode and mechanism of its propagation, as well as the nature of the medium in which the propagation takes place. And we shall see, as we pass in review, in historical order, the various theories that have been proposed, that our knowledge on these points is after all extremely limited."

The historical side is well treated, and consists not merely of a few isolated theories, but shows how these have been led up to on the basis of earlier work. The chapters on the Electro-magnetic Theory and the Electron Theory are especially interesting, the weak points in each case being clearly indicated. The book may be commended to those desirous of obtaining a clear conception of the relative value of the theories of light propagation at present available.

J. E. B.

First Course in General Science. By F. D. BARBER, M. S. FULLER, J. L. PRICER, and H. W. ADAMS. [Pp. vii + 608.] (New York: Henry Holt & Co.; London: G. Bell & Sons. Price 9*s.* net.)

"THIS book is written in the belief that science instruction in the first high-school year should not aim primarily to survey the entire field of nature and present scattered bits and choice morsels from every special science in order that the pupil may decide which of the special sciences he likes best and which he will omit. Nor should first-year general science be regarded primarily as an introduction to, or a foundation for, the special science he may later study. While it should, in a large measure, accomplish both these results, it has a vastly more important function to perform. The PRIMARY FUNCTION of first-year general science is to give, as far as possible, a rational, orderly, scientific understanding of the pupil's environment to the end that he may, to some extent, correctly interpret that environment and be master of it. IT MUST BE JUSTIFIED BY ITS OWN INTRINSIC VALUE AS A TRAINING FOR LIFE'S WORK."

The foregoing paragraph from the preface indicates the aim of the work. In most English schools the introduction to science is a course planned to lead up to more advanced work and is, in most cases, quite satisfactory for

the future science specialist. There is an increasing tendency, however, in many of the public schools at least, to replace this formal science by a wider and shallower course which is held to be more suitable for the majority of pupils, who thus acquire a working knowledge and understanding of natural phenomena and an appreciation of the value of scientific discovery in modern civilisation which they miss entirely under the deeper and narrower methods of the old system.

This American publication is far in advance of anything which has yet been published in England to cater for this modern development in scientific teaching, and merits attention on that account. Three university professors, of chemistry, physics, and biology respectively, and a lecturer on meteorology, have collaborated, each bringing his own expert knowledge to deal with the various branches of science, and consequently the book is almost entirely free from the misstatements and errors which are commonly found in text-books on general science compiled by a single author.

The book is a mine of information; over 100 experiments are outlined in its 600 pages, and many ingenious questions and problems are inserted in the various chapters. It is difficult, however, to visualise it as a first-year course, as it would appear that at least three years would be occupied in traversing the course in the time allotted to science in the English school curriculum. Heating, lighting, refrigeration, weather, seasons, climate, health, ventilation, food, nutrition, micro-organisms, soil physics, water supply, sewage disposal, machines, work, and energy are all dealt with in considerable detail, and the book strikes one as a fairly complete science course for non-specialists rather than as an introduction.

The printing, apart from the illustrations, is good; but the price seems prohibitive for general class use. It might be wise to issue it in sections in limp covers.

V. Seymour Bryant.

Le Movement Scientifique Contemporain en France : I. Les Sciences Naturelles : Par Georges Matisse, Docteur ès Sciences. [Pp. 160, with 25 illustrations.] (Paris: Payot et Cie, 1921. Price 4 francs.)

This little volume appears to be the first of a series intended to summarise French achievements in the domain of science during the last fifty years or so. The present instalment is devoted to the biological sciences. It opens with an account of the work of Lacaze-Duthiers, the founder of the first marine laboratory (at Roscoff in Brittany) and of the *Archives de Zoologie Experimentale*, both in or about 1870. The themes of the other narratives are as follows: Yves Delage: Sa Théorie de l'Hérédité; Polyzoisme des Organismes; Fécondation Chimique. La Fécondation Artificielle des Batraciens: E. Bataillon. La Morphologie Dynamique; La Forme des Poissons: Frédéric Houssay. La Théorie de la Préadaptation des Êtres au Milieu ou ils vivent: L. Cuénot. Les Tropismes; les Formes; la Chimie et la Vie: Georges Bohn. L'eau de Mer, Milieu Organique: Réné Quinton. L'Embryogénie des Plantes et les Tissus Transitoires; Théorie de la Constitution de la Plante: Gustave Chauveaud. La Physiologie Végétale: Marin Molliard. La Culture Artificielle des Végétaux Inférieurs: Louis Matruchot. In each case the account of the problem and of the achievements of the respective scientists is told in a concise and clear manner, and the literature on each subject treated is given at the end of each chapter. It seems somewhat strange to see science surveyed from the point of view of national boundaries, seeing that science is the most international of all human creations. And the period covered, 1870–1920, is also rather ominous, for it coincides with the Rise and Fall of German Imperialism, and, as some fear, with the Fall and Resurrection of French Imperialism. It is to be hoped that the great achieve-

ments of French scientists will not be used to feed the flames of Jingoism. Doctor Matisse, it is only fair to say, gives no occasion for complaint in this respect. One can only praise his lucid exposition and his wise choice of apt quotations.

A. WOLF.

Hyperacoustics. Division II, SUCCESSIVE TONALITY. By JOHN L. DUNK. [Pp. xi + 160.] (London: J. M. Dent & Sons, Ltd., 1921. Price 5s. net.)

THIS second volume of Hyperacoustics deals with its subject in the same strikingly original manner that the author evinced in his somewhat mystical treatment of simultaneous tonality given in the earlier volume. To form a bridge between the two divisions of the subject this second volume includes a 60-page summary of the contents of the first.

E. H. B.

The Electric Furnace. By J. N. PRING, M.B.E., D.Sc. Research Department, Royal Arsenal, Woolwich. (Monographs on Industrial Chemistry, edited by SIR EDWARD THORPE, C.B., F.R.S.) [Pp. xii + 485, with numerous plates and illustrations.] (London: Longmans, Green & Co. Price 32s. net.)

PRACTICALLY speaking, we may divide electro-thermal operations into two classes, those where a vigorous and fundamental reaction occurs, such as the formation of carborundum, calcium carbide, aluminium, etc., and those where the purely chemical changes are slight, such as the production of graphite, the smelting of iron and steel and other metals and alloys.

Hitherto it has been felt, rather than reasoned, that in countries such as Great Britain, where water-power is very scarce, we could only hope to utilise electricity for the first class of operations, whilst the use of electric furnaces for the heating of great masses of metal was thought to be economically impossible.

Dr. Pring is at great pains to show the fallacy of the argument, as in direct fuel heating the efficiency is probably well below 20 per cent. owing to flue losses, etc., whilst with internal electric heating an efficiency of 80 per cent. or more may be attained, which leaves a wide margin in favour of the electric furnace.

It is to be expected, therefore, that the electric furnace will come rapidly into wide use in all countries where reasonably cheap fuel is available, owing to its economy, cleanliness, and easy control, and for all those whose interests lie in these directions the present book should be invaluable.

It is impossible here to do more than indicate the scope of the book, which deals with the principles of furnace design, their cost and efficiency, the production of calcium carbide, cyanamide, synthetic ammonia and nitric acid, iron, steel, copper and tin smelting, carborundum, graphite, phosphorus, and so on.

The illustrations and diagrams are extremely clear and up to date, and the author's long experience as a teacher at Owen's College ensures a clearness of expression which is very welcome. Although one or two books on the same subject have been published since the last English edition of Moissan's classic work in 1904, it is doubtful whether any of them attain to quite the same high standard of clarity and conciseness as Dr. Pring's comprehensive work, and he has rendered no small service both to chemists and electricians in undertaking the production of the very excellent volume now published.

F. A. M.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Elementary Algebra. By C. V. Durell, M.A., Part II, and R. M. Wright, M.A. London: G. Bell & Sons, 1921. (Pp. xxiii + 551.) Price 5*s.* 6*d.* net.

This volume completes the excellent introduction to algebra of which the first part by Mr. Durell and Mr. Palmer was published in 1920. The same method of procedure is adopted: two editions are prepared, of which the one intended for the use of teachers contains a detailed introduction and answers. Lengthy discussions in the text are usually of little value to the ordinary boy, who needs oral instruction; they are therefore omitted in the edition which will be placed in his hands.

Algebra is interpreted, as it should be, in a wide sense; chapters are given on limits, differentiation, integration, and empirical formulas, and there is a welcome introductory account of nomography. A useful glossary and index is given at the end. We have no hesitation in highly recommending this book.

Examples in Optics. Compiled by T. J. I'A. Bromwich, Sc.D., F.R.S. Cambridge: Bowes & Bowes, 1921. (Pp. 16.) Price 2*s.* net.

Dr. Bromwich has here reprinted, primarily for the use of his classes at Cambridge, a useful collection of problems in Geometrical Optics; mostly taken from Mathematical Tripos Papers.

A Treatise on Probability. By John Maynard Keynes, Fellow of King's College, Cambridge. London: Macmillan & Co., St. Martin's Street. 1921. (Pp. xi + 466.) Price 18*s.* net.

Éléments de la Théorie des Vecteurs et de la Géometrie Analytique. By Paul Appell, Membre de l'Institut Recteur de l'Université de Paris. Paris: Payot et Cie, 106 Boulevard Saint-Germain, 1921. (Pp. 147.) Price 4 frs.

The Fourth Dimension. By E. H. Neville, late Fellow of Trinity College, Cambridge, Professor of Mathematics in University College, Reading. Cambridge: at the University Press, 1921. (Pp. 55.) Price 5*s.* net.

A Study of Mathematical Education, including the Teaching of Arithmetic. By Benchara Branford. New Edition, enlarged and revised. Oxford: at the Clarendon Press, 1921. (Pp. xii + 432.) Price 7*s.* 6*d.* net.

An Introduction to Projective Geometry. By L. N. G. Filon, M.A., D.Sc., F.R.S., Fellow of and Professor of Applied Mathematics at University College, London. Third Edition. London: Edward Arnold & Co. (Pp. viii + 253.) Price 7*s.* 6*d.* net.

The Fourth Dimension Simply Explained. A Collection of Essays Selected from those submitted in the Scientific American's Prize Competition. With an Introduction and Editorial Notes by Henry P. Manning, Ph.D., Associate Professor of Mathematics in Brown University. London: Methuen & Co., 36 Essex Street, W.C. (Pp. 251.) Price 7*s.* 6*d.* net.

Éléments d'Analyse Mathématique à l'Usage des Candidats au Certificats de Mathématiques Générales des Ingénieurs et des Physiciens. Cours Professé à l'Ecole Centrale des Arts et Manufactures par Paul Appell,

Membre de l'Institut Doyen Hongraire de la Faculté des Sciences Recteur de l'Université de Paris. Quatrième Édition. Entièrement refondue. Paris: Gauthier-Villars et Cie., 55 Quai des Grands-Augustins, 1921. (Pp. x + 715.)

Handbook of Meteorology. A Manual for Co-operative Observers and Students. By Jacques W. Redway, Fellow American Meteorological Society. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. 294.) Price 24*s*. net.

A Text-book of Physics. Edited by A. Wilmer Duff; Contributors: A. Wilmer Duff, E. Percival Lewis, Albert P. Carman, R. K. McClung, Charles E. Mendenhall. Fifth Edition, Revised. London: J. & A. Churchill, 7 Great Marlborough Street. (Pp. xi + 700, with 609 illustrations.) Price 16*s*. net.

L'Atome, sa Structure, sa Forme, avec des Dessins à la Plume de M. Raoul Leclerc, Architecte Diplomé du Gouvernement. Les Édifices Physico-Chimiques. Tome I. Dr. Achalme, Directeur de Laboratoire à l'École des Hautes Études. Paris: Payot et Cie., 106 Boulevard Saint-Germain, 1921. (Pp. 224.) Price 15 frs.

L'Œuvre Scientifique de Sadi Carnot. Introduction à l'Étude de la Thermodynamique. By E. Ariès, Correspondant de l'Institut. Paris: Payot et Cie., 106 Boulevard Saint-Germain, 1921. (Pp. 160.) Price 4 frs.

The Mechanism of Life in Relation to Modern Physical Theory. By James Johnstone, D.Sc., Professor of Oceanography in the University of Liverpool. London: Edward Arnold & Co., 1921. (Pp. xii + 248.) Price 15*s*. net.

Relativity and the Universe. A Popular Introduction into Einstein's Theory of Space and Time. By Dr. Harry Schmidt. Authorised Translation by Karl Wichmann, M.A., Ph.D. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xiii + 136, with five diagrams.) Price 5*s*. net.

Aggregation and Flow of Solids: Being the Records of an Experimental Study of the Micro-structure and Physical Properties of Solids in various States of Aggregation, 1920–1921. By Sir George Beilby, F.R.S. London: Macmillan & Co., St. Martin's Street, 1921. (Pp. xv + 256, with 34 plates.) Price 20*s*. net.

An Introduction to the Theory of Relativity. By L. Bolton, M.A. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xi + 177, with 38 diagrams.) Price 5*s*. net.

The Physical Properties of Colloidal Solutions. By E. F. Burton, B.A., Ph.D., Associate Professor of Physics, University of Toronto. London: Longmans, Green & Co., 39 Paternoster Row, 1921. (Pp. viii + 221.) Price 12*s*. 6*d*. net.

The Rudiments of Relativity. Lectures delivered under the auspices of the University College, Johannesburg, Scientific Society. By John P. Dalton, M.Al., D.Sc., Fellow of the Royal Society of South Africa. Johannesburg: published by the Council of Education, Witwatersrand, 1921. (Pp. 105.) Price 5*s*. net.

Relativity and Gravitation. Edited by J. Malcolm Bird, Associate Editor, *Scientific American*. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xiv + 345.) Price 8*s*. 6*d*. net.

Atomic Theories. By F. H. Loring. London: Methuen & Co., 36 Essex Street, W.C. (Pp. ix + 218, with 66 figures.) Price 12*s.* 6*d.* net.

General Physics and its Application to Industry and Everyday Life. By Ervin S. Ferry, Professor of Physics Purdue University, New York: John Wiley & Sons; London: Chapman and Hall, 1921. (Pp. xvi + 732, with 600 figures.) Price 24*s.* net.

The Elements of Physics and Chemistry. A Practical Course for Middle Forms. By Sir Richard Gregory and A. T. Simmons, B.Sc., with the assistance of F. W. Hodges, B.Sc. London: Macmillan & Co., 1921. (Pp. v + 299, with 186 figures.) Price 4*s.* 6*d.* net.

Les Combustibles Liquides et leurs Applications. Par le Syndicat d'Applications Industrielles des Combustibles Liquides. Paris: Gauthiers-Villars et Cie., 55 Quai des Grands-Augustins. (Pp. 621.)

Absolute Measurements in Electricity and Magnetism. By Andrew Gray, LL.D., F.R.S., M.I.E.E., Professor of Natural Philosophy in the University of Glasgow. Second Edition, rewritten and extended. London: Macmillan & Co., St. Martin's Street, 1921. (Pp. xix + 837.) Price 42*s.* net.

The Ideas of Einstein's Theory: the Theory of Relativity in Simple Language. By J. H. Thirring, Ph.D., Professor of Theoretical Physics in the University of Vienna. Translated by Rhoda A. B. Russell. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xiv + 167, with 7 diagrams and 1 chart.) Price 5*s.* net.

Handbook of Chemistry and Physics. By Charles D. Hodgman, M.S., assisted by M. F. Coolbaugh, M.A., and C. E. Senserman, M.A. Eighth Edition. London: Chapman & Hall, 1921. (Pp. 711.) Price 24*s.* net.

This little book of ready reference of which seven editions have already appeared, contains the great majority of really important tables and formulæ necessary to the chemist, physicist, and engineer. The most important additions are a new and enlarged numerical table, and a complete set of Metric-English and English-Metric Conversion Tables, besides which several new chemical and X-ray tables have been added, while many other tables, which have appeared in previous editions, have been entirely rewritten and greatly enlarged.

Chemical Disinfection and Sterilization. By Samuel Rideal, D.Sc., and Eric K. Rideal, D.Sc., M.A. London: Edward Arnold & Co., 1921. (Pp. vii + 313.) Price 21*s.* net.

Chemistry and Civilization. By Allerton S. Cushman, A.M., Ph.D., Director, Institute of Industrial Research, Inc., Washington, D.C. Edinburgh: E. & S. Livingstone, 17 Teviot Place, 1921. (Pp. 151.) Price 15*s.* net.

Early Science in Oxford, Part I: Chemistry. By R. T. Gunther, Magdalen College, Oxford. Printed for sale at the Oxford Science Laboratories by Hazell, Watson & Viney, London and Aylesbury, 1920. London Agents: Oxford University Press. (Pp. vi + 91.) Price 10*s.* net.

The Fixation of Atmospheric Nitrogen. By Joseph Knox, D.Sc., Lecturer on Chemistry, University of Glasgow. Second Edition. London: Gurney & Jackson, 33 Paternoster Row, E.C., 1921. (Pp. vii + 124.) Price 4*s.* net.

An Introduction to Biophysics. By David Burns, M.A., D.Sc., Grieve Lecturer on Physiological Chemistry in the University of Glasgow. With a Foreword by D. Noel Paton, M.D., LL.D., F.R.S. London: J. and A. Churchill, 7, Great Marlborough Street, 1921. (Pp. xiii + 435, with 85 illustrations.) Price 21*s.* net.

Biological Chemistry. By H. E. Roaf, M.D., D.Sc., M.R.C.S., L.R.C.P., Professor of Physiology at London Hospital Medical College, University of London. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xvi + 216, with 47 figures.) Price 10*s*. 6*d*. net.

A Text-book of Qualitative Analysis of Inorganic Substances. By Sydney Alexander Kay, D.Sc., Lecturer in Chemistry, University of Edinburgh. London: Burney & Jackson; Edinburgh: Oliver & Boyd, Tweeddale Court, 1921. (Pp. vii + 80.) Price 7*s*. 6*d*. net.

The Riddle of the Rhine. Chemical Strategy in Peace and War. By Victor Lefebure, Officer of the Order of the British Empire, with an Introduction by Field-Marshal Sir Henry Wilson, Bart., Chief of the Imperial Staff. London: W. Collins, Sons & Co., 48 Pall Mall. (Pp. 277.) Price 10*s*. 6*d*. net.

The Chemistry of Colloids and some Technical Applications. By W. W. Taylor, M.A., D.Sc., Lecturer in Chemical Physiology at the University of Edinburgh. London: Edward Arnold & Co., 1921. (Pp. viii + 332, Price 10*s*. 6*d*. net.

The Hot Springs of New Zealand. By Arthur Stanley Herbert, O.B.E., M.D., B.S., Consulting Balneologist to and late Government Balneologist to the Dominion of New Zealand. London: H. K. Lewis & Co., 1921. (Pp. xiv + 284, with 3 maps and 87 illustrations.) Price 15*s*. net. Also in Paper Covers, 14*s*. net.

Map Projections. By Arthur R. Kinks, C.B.E., M.A., F.R.S., Secretary of the Royal Geographical Society. Second Edition. Revised and enlarged. Cambridge: at the University Press, 1921. (Pp. xii + 158.) Price 12*s*. 6*d*. net.

Petroleum. Monographs of the Imperial Institute on Mineral Resources with special reference to the British Empire. Prepared jointly with H.M. Petroleum Department with the Co-operation of H. B. Cronshaw, B.A., Ph.D., A.R.S.M. London: John Murray, Albemarle Street, W., 1921. (Pp. x + 110.) Price 5*s*. net.

Silver Ores. Monographs of the Imperial Institute on Mineral Resources with Special Reference to the British Empire. Prepared under the Direction of the Mineral Resources Committee of the Imperial Institute with the Assistance of the Scientific and Technical Staff. By H. B. Cronshaw, B.A., Ph.D., A.R.S.M. London: John Murray, Albemarle Street, W., 1921. (Pp. vii + 152, with 2 diagrams and a map.) Price 6*s*. net.

La Constitution des Plantes Vasculaires Révélée par leur Ontogénie. Gustave Chauveaud, Directeur de Laboratoire à l'École des Hautes-Études. Paris: Payot et Cie., 106 Boulevard Saint-Germain, 1921. (Pp. xiii + 155, with 54 figures.) Price 10 frs.

Breeding Crop Plants. By Herbert Kendall Hayes, Professor of Plant Breeding, College of Agriculture, University of Minnesota, and Ralph John Garber, Associate Professor and Head of the Department of Agronomy, University of West Virginia. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. xvii + 328, with 66 figures.) Price 21*s*. net.

Strasburger's Text-book of Botany. Rewritten by Dr. Hans Fitting, Dr. Ludwig Jost, Dr. Heinrich Schenck, and Dr. George Karsten. Fifth

English Edition, by W. H. Lang, M.B., D.Sc., F.R.S. London: Macmillan & Co., St. Martin's Street, 1921. (Pp. xi + 799, with 833 figures.) Price 31*s.* 6*d.* net.

This text-book is too well known to need detailed description. The present edition, which has been revised from the 14th German Edition, is in the main similar to the 4th Edition published in 1912. In the section on Morphology we note a very inadequate attempt to deal with the relation of the plant to its habitat, otherwise ecology is practically ignored. There is also a meagre treatment of genetics.

In a work which has so many admirable features it is to be regretted that the fundamental changes necessary to bring that text into line with modern requirements have not been undertaken.

Origin and Evolution of the Human Race. By Albert Churchward, M.D., M.R.C.P., F.G.S., etc. London: George Allen & Unwin, 40 Museum Street, W.C.1. (Pp. xv + 511.) Price 45*s.* net.

An Introduction to Cytology. By Lester W. Sharp. Cornell University. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 and 8 Bouverie Street, E.C.4, 1921. (Pp. xiii + 452.) Price 24*s.* net.

Applied Entomology. An Introductory Text-book of Insects in their Relation to Man. By H. T. Fernald, Ph.S., Professor of Entomology, Massachusetts Agricultural College. New York: McGraw-Hill Book Company, 370 Seventh Avenue; London: 6 Bouverie Street, E.C.4, 1921.

Organic Evolution: Outstanding Difficulties and Possible Explanations. By Major Leonard Darwin, Hon. Sc.D., Cantab. Cambridge: at the University Press, 1921. (Pp. vii + 47.) Price 4*s.* net.

A History of the Whale Fisheries from the Basque Fisheries of the Tenth Century to the Hunting of the Finner Whale at the Present Date. By J. T. Jenkins, Ph.D. London: H. F. and G. Witherby, 326 High Holborn, W.C., 1921. (Pp. 336, with 12 illustrations.) Price 18*s.* net.

Hormones and Heredity: A Discussion of the Evolution of Adaptations and the Evolution of Species. By J. T. Cunningham, M.A., F.Z.S., Lecturer in Zoology at East London College, University of London. London: Constable & Co., 1921. (Pp. xx + 246.) Price 24*s.* net.

Typical Flies: A Photographic Atlas. By E. K. Pearce. Second Series. Cambridge: at the University Press, 1921. (Pp. xiv + 38, with 125 illustrations.) Price 15*s.* net.

Insect Transformation. By George H. Carpenter, D.Sc., Professor of Zoology in the Royal College of Science, Dublin, Secretary of the Royal Irish Academy. London: Methuen & Co., 36 Essex Street, W.C. (Pp. x + 282, with 4 plates and 124 figures.) Price 12*s.* 6*d.* net.

The Wisdom of the Beasts. By Charles Augustus Strong. London: Constable & Company, 1921. (Pp. ix + 76.)

Sur l'Origine dell'uomo Nuove Teorie e Documenti. By V. Giuffrida-Ruggeri, Prof. Ord. di Antropologia Generale nella R. Università di Napoli. Bologna: Nicola Zanichelli. (Pp. xiii + 264, with 24 figures.) Price 24 L.

A Text-book of European Archæology. By R. A. S. Macalister, Litt.D., F.S.A., Professor of Celtic Archæology, University College, Dublin. Volume I: The Palæolithic Period. Cambridge: at the University Press, 1921. (Pp. xv + 610, with 183 figures.) Price 50*s.* net.

Turbines. By Engineer-Captain A. E. Tompkins, C.B.E., Royal Navy (retired). Third Edition, entirely revised. London: Society for Promoting Christian Knowledge; New York: The Macmillan Company, 1921. (Pp. viii + 180, with 130 figures.) Price 8*s*. net.

Fifty Years of Electricity. The Memories of an Electrical Engineer. By J. A. Fleming, M.A., D.Sc., F.R.S., University Professor of Electrical Engineering in the University of London. London: The Wireless Press, Ltd., 12 Henrietta Street, Strand, W.C.2; New York: Wireless Press, Inc., 326 Broadway. (Pp. xi + 371, with 16 plates.) Price 20*s*. net.

Metric System for Engineers. By Charles B. Clapham, Hon. B.Sc., Eng., Lecturing in the Engineering Dept., University of London. London: Chapman & Hall, 11 Henrietta Street, W.C.2, 1921. (Pp. xii + 181.) Price 12*s*. 6*d*. net.

Modern Electrical Theory, Supplementary Chapters. Chapter XV: Series Spectra. By Norman Robert Campbell, Sc.D., a Member of the Staff of the Research Laboratories of the General Electric Company, Limited, London. Cambridge: at the University Press, 1921. (Pp. vii + 109.) Price 10*s*. 6*d*. net.

Hygiene for Health Visitors, School Nurses and Social Workers. By C. W. Hutt, M.A., M.D., D.P.H., Medical Officer of Health, Metropolitan Borough of Holborn. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xiii + 382, with 83 illustrations.) Price 12*s*. 6*d*. net.

Development Pathology: A Study in Degenerative Evolution. By Eugene S. Talbot, M.S., D.D.S., M.D., LL.D., Professor of Stomatology, Bennett Medical College, Edinburgh: E. and S. Livingstone, 17 Teviot Place, 1921. (Pp. xxii + 435, with 346 illustrations.) Price 25*s*. net.

Symptomatology, Psychognosis, and Diagnosis of Psychopathic Diseases. By Boris Sidis, A.M., Ph.D., M.D., Medical Director of the Sidis Psychotherapeutic Institute. Edinburgh: E. and S. Livingstone, 17 Teviot Place, 1921. (Pp. xxii + 448.) Price 21*s*. net.

Therapeutic Immunization in Asylum and General Practice. By W. Ford Robertson, M.D., Pathologist to the Scottish Asylums. Edinburgh: E. and S. Livingstone, 17 Teviot Place, 1921. (Pp. vii + 278.) Price 15*s*. net.

A Manual of Pharmacology. By Walter E. Dixon, M.A., M.D., B.S., B.Sc., D.P.H., F.R.S. Fifth Edition, completely revised. London: Edward Arnold & Co., 1921. (Pp. xii + 468.) Price 18*s*. net.

Hygiene of Town Planning and Vegetation. By P. S. G. Dubash, D.Sc., with Introduction by Hon. Sir John Cockburn, K.C.M.G., M.D. London: G. Bell & Sons, York House, Portugal Street. (Pp. 127.) Price 3*s*. net.

On the State of the Public Health. Annual Report of the Chief Medical Officer of the Ministry of Health for the Year 1920. London: Printed and published by His Majesty's Stationery Office, 1921. (Cmd. 1397.)

Laboratory Manual in General Microbiology. Prepared by the Laboratory of Bacteriology and Hygiene, Michigan Agricultural College, Second Edition. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. xxii + 472.) Price 21*s*. net.

The Dawn of Modern Medicine. An Account of the Revival of the Science and Art of Medicine which took place in Western Europe during the

latter half of the Eighteenth Century and the First Part of the Nineteenth. By Albert H. Buck, B.A., M.D. New Haven: Yale University Press; London: Oxford University Press, 1920. Price 25s. net.

A Manual of Practical Anatomy. A Guide to the Dissection of the Human Body. By Thomas Walmsley, Professor of Anatomy, Queen's University, Belfast. In Three Parts. Part II: The Thorax and Abdomen. London: Longmans, Green & Co., 1921. (Pp. v + 233, with 82 figures.) Price 10s. 6d. net.

Beowulf: An Introduction to the Study of the Poem, with a discussion of the Stories of Offa and Finn. By R. W. Chambers. Cambridge: at the University Press, 1921. (Pp. xii + 419.) Price 30s. net.

What is Science? By Norman Campbell, Sc.D., F.Inst.P. London: Methuen & Co., 36 Essex Street, W.C. (Pp. ix + 186.) Price 5s. net.

Life of Élie Metchinikoff, 1845–1916. By Olga Metchinikoff, with a Preface by Sir Ray Lankester, K.C.B., F.R.S. London: Constable & Company, Ltd., 1921. (Pp. xx + 297.) Price 21s. net.

The Care of the Adolescent Girl: A Book for Teachers, Parents, and Guardians. By Phyllis Blanchard, Ph.D., with Prefaces by Dr. Mary Scharlieb, C.B.E., M.S., and Professor G. Stanley Hall, Ph.D., LL.D., President of the Clark University, Worcester, Mass. London: Kegan Paul, Trench, Trübner & Co., 63 Carter Lane, E.C., 1921. (Pp. xxi + 201.) Price 7s. 6d. net.

Bleaching, being a Résumé of the Important Researches on the Industry published during the years 1908–20. By S. H. Higgins, M.Sc., Head of the Research Department, Bleachers' Association, etc. Manchester: at the University Press, 12 Lime Grove, Oxford Road; London: Longmans, Green & Co. (Pp. vii + 137.) Price 10s. 6d. net.

Elementary Statics of Two and Three Dimensions. By R. J. A. Barnard, M.A., Professor of Mathematics, Royal Military College of Australia. London: Macmillan & Co., St. Martin's Street, 1921. (Pp. iii + 254.) Price 7s. 6d. net.

The Psychology of Society: By Morris Ginsberg, M.A., Lecturer in Philosophy, University College, London. London: Methuen & Co., 36 Essex Street, W.C. (Pp. xvi + 174.) Price 5s. net.

Exploration of Air out of the World North of Nigeria. By Angus Buchanan, M.C. London: John Murray, Albemarle Street, W., 1921. (Pp. xxiii + 258, with photographs.) Price 16s. net.

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

APPLIED MATHEMATICS. By S. Brodetsky, M.A., Ph.D., F.Inst.P., etc., University, Leeds.

It is not an easy matter to decide what is, and what is not, to be included under the category of Applied Mathematics. If an attempt is made to interpret the term generously there is serious danger of overlapping with other branches of study like astronomy, physics, and engineering. On the other hand, a limited view of the subject is liable to lose sight of researches that affect the very fundamentals on which mechanics is based. This applies with particular force to the theory of relativity. Based on optical experiments, using the methods of pure mathematics, and seeking verification in the results of astronomical calculation and observation, the theory of relativity is at bottom an attempt to put the theoretical side of mechanics on consistent and logical foundations, and the applied mathematician is at least as concerned in the fortunes of the theory as the physicist or astronomer. No excuse need, therefore, be offered for beginning the present article with an account of the recent work on relativity.

Far more has been written about relativity during the past year than can be conveniently discussed in a single article. Mathematicians and physicists have been busy examining the theory in all its bearings. Perhaps the most useful papers to examine first are those that attempt a critique, favourable or unfavourable. Of such papers the most important are those of the French mathematicians. P. Painlevé devotes considerable space to a comparison of Einstein's treatment of gravitation with that of Newton, *Comptes Rendus*, **173**, 1921, 873–87. After a brief statement of the fundamental postulates of the classical mechanics, Painlevé considers the Principle of Invariance, which plays so leading a part in Einstein's work. He declares his assent to this principle, but in a different form: "It is possible from the laws of nature to derive consequences which are invariant for all changes of the space-time frame of reference, and which define these laws for any such change." Painlevé then proceeds to examine the way in which Einstein uses this principle, emphasising the fact that Einstein always tries to

derive his equations from those of classical mechanics, but makes them invariant in form, thus imitating Lagrange, who obtained invariant equations of motion in dynamics, but going further than Lagrange in extending the invariance to the space-time frame, whereas Lagrange only postulated invariance for the space co-ordinates. The writer then discusses the application to a symmetrical field, and the results that are thus obtained, concluding with the statement that the many speculations that form part of the theory of relativity may finally be discredited, but the positive theory of gravitation built up by Einstein will not be affected thereby.

What troubles Painlevé is the fact that in Einstein's results any function of the radius vector, within reason, can be substituted for the radius vector, *ibid.*, and 677–80; also J. Chazy, *ibid.*, 905–7. In truth it must be admitted that some relativists do far too much juggling with this arbitrary function, and the consequence is unnecessary bewilderment of the unsophisticated. As Painlevé points out, the arbitrariness in the choice of radius vector is due to the scanty astronomical data to which the theory can be at present applied. Some writers choose this function so as to make the velocity of light at any point the same in all directions, others so as to make the velocity of light constant along any radius vector. Einstein himself is fond of making the determinant $g = -1$, and so on. Perhaps a reasonable choice would be to make the periodic time in a circular orbit vanish when the radius is zero—but the writer of these notes must not join in this process of speculation, at least not here.

Other French writers on the theory are E. Borel, *ibid.* 189–91, who offers no opinion for or against; E. Picard, *ibid.*, 680–2, who discusses the implications of space-time considered as one organic conception; P. Langevin, *ibid.*, 831–4; and J. Le Roux, *ibid.*, 1074–7. Among general discussions of the theory must also be mentioned the paper by L. T. More, "On the Postulates and Conclusions of the Theory of Relativity," *Phil. Mag.* (vi) **42**, 1921, 841–52, whose decision is against the theory. Equally unfavourable is the view of O. Meiszner, *Phys. Zeit.*, xxii, 1921, 183–5; E. Reichenbächer, *ibid.*, 234–43, who declares that the theory does not explain the nature of weight; see also E. Guillaume and C. Willigens, *ibid.*, 109–14. Of great interest are the discussions opened by A. S. Eddington at the Physical Society, *Proc. Phys. Soc.*, xxxii, 1921, 245–51, and at the Mathematical Association, *Math. Gaz.*, x, 1921, 228–33.

Although the present article cannot enter into the optical consequences of the theory, mention must be made of the experiments by A. O. Rankine and L. Silberstein, who tried to measure the effect on the velocity of light produced by a gravi-

tational field, *Phil. Mag.* (vi), **39**, 1920, 586–91. They found that the ratio (vertical velocity minus horizontal velocity) velocity is less than 8×10^{-12}. But of more immediate interest to the applied mathematician is the question of the perihelion of Mercury. J. Le Roux, *Comptes Rendus*, **172**, 1921, 1227–30, 1467–9, does not think that Einstein's explanation of the outstanding discrepancy is really an argument in favour of relativity. G. Bertrand, *ibid.*, 1921, 438–40, finds yet another modification of Newton's law that will account for the discrepancy. W. M. Smart, *M.N.*, *R.A.S.*, lxxxii., 1921, 12–19, considers the possibility of the existence of an unknown planet, at the same mean distance from the sun as Mercury, and concludes that its stellar magnitude would have to be between 0·0 and 1·2.

The problem of rotation is one of the great difficulties in relativity mechanics. R. A. Sampson returns to this (see SCI. PROG., 1921, 525) *Phil. Mag.* (vi), **40**, 1920, 67–72, and expresses the opinion that rectilinear motion is a degenerate case, and so the conclusions derived from rectilinear motion cannot be applied to circular motion. An interesting paper was written by G. Lippmann, " Détermination de l'axe de rotation, de la vitesse de rotation d'un corps solide et réalisation d'un corps solide sans rotation," *Comptes Rendus*, **172**, 1921, 557–61 ; see also E. Picard, *ibid.*, 629–30, L. Lecornu, *ibid.*, 731. Other papers are F. Kottler, " Rotierende Bezugssysteme in einer Minkowskischen Welt," *Phys. Zeit.*, xxii, 1921, 274–80, 480–4, and J. Rey, " Sur l'expérience de Perrot relative au mouvement de rotation de la terre," *Comptes Rendus*, **171**, 1920, 343–4.

Concerning the foundations of relativity as contained in the Lorenz transformation, a very interesting paper is that by L. A. Pars, *Phil. Mag.* (vi.), **42**, 1921, 249–58, who shows that it is not necessary to *assume* the existence of an invariable velocity (that of light), but that this follows from the very principles of relativity. A. Righi, *Comptes Rendus*, **170**, 1920, 1550–4, suggests further decisive experiments (see also J. Villey, *ibid.*, **171**, 298–301), while a useful illustration of subjectivity in estimating speeds is given by C. Richet, *ibid.*, **173**, 1921, 805–6.

The relativity theory of gravitation naturally suggests further investigation of the properties of gravitational attraction, and considerable interest has been aroused by the experiments of A. Majorana. In *Phil. Mag.* (vi), **39**, 1920, 488–504 he gives an account of his experiments on the " quenching " of gravitation by matter. He surrounded a leaden ball weighing 1,274 grams with 104 kg. of mercury, and he found that the ball lost $7{\cdot}7 \times 10^{-10}$ of its weight. From this he deduced the quenching factor for matter of unit density to be $6{\cdot}73 \times 10^{-12}$, which means that gravitational force is reduced by this fraction of its value

when it passes through unit thickness of matter of unit density. Majorana holds that the absorption or quenching of gravitation is not similar to the phenomena of dielectrics or permeability, but that it takes place to an extent increasing exponentially with the amount of matter passed through: gravitation being a flux of energy, the absorption of which causes a rise in temperature. One interesting deduction is that the true mass of the sun is about three times as great as that calculated astronomically. In a later paper, *Comptes Rendus*, **173**, 1921, 478–9, Majorana states that a new experiment gave a value $2{\cdot}5 \times 10^{-12}$ for the quenching factor. H. N. Russell, *Astro. Jour.*, liv, 1921, 334–46, contends that these results are inconsistent with the phenomena of the tides. A revival of the theory that gravitation is a streaming in the ether is contained in a paper by H. Fricke, *Phys. Zeit.*, xxii, 1921, 636–9.

One of the most brilliant applications of Einstein's methods is Weyl's extension of the theory to include the phenomena of electro-magnetism, although the impression gathered by the present writer in a conversation with Einstein was that the latter is content to stop at gravitation and declines to reduce other forces to properties of the space-time continuum. The argument is that gravitation is unique in its universality, in being a property of all matter. A further generalisation of Weyl's work is made by A. S. Eddington, *Proc. Roy. Soc.*, **99A**, 1921, 104–22. After working out a pure geometry of a more generalised kind, the author shows that Einstein's mathematics represent the facts of gravitation exactly. In connection with the question of electro-magnetism reference should be made to a very interesting paper by E. T. Whittaker, *Proc. Roy. Soc. Edin.*, xlii, 1921, 1–23. Whittaker introduces a new kind of tube of force, which he calls a "calamoid": it is a generalisation of the Faraday tube, and reduces to the electric tube if there is no magnetic field present, to the magnetic tube if there is no electric field present. When both exist the quantity $((\text{electric force})^2-(\text{magnetic force})^2)^{\frac{1}{2}}$ is inversely proportional to the tube section.

Many investigations of a mathematical nature have been suggested by relativity. An obvious problem is to discuss the exact form of the path of a planet according to Einstein's theory. In addition to the paper by A. R. Forsyth (see SCI. PROG., 1921, 526), we now have investigations in terms of elliptic functions by G. Greenhill, *Phil. Mag.*, (vi), **41**, 1921, 143–8, and by F. Morley, *Amer. J. Math.*, xliii, 1921, 29–32. W. B. Morton discusses the parabolic and planetary orbits, *Phil. Mag.* (vi), **40**, 1920, 674–7, and *ibid.*, **42**, 1921, 511–22. Other papers are by A. Anderson, *ibid.*, **39**, 1920, 626–9; *ibid.*, **40**, 1920, 499–501; E. S. Pearson, *ibid.*, **40**, 1920, 342–4, who shows that Anderson

made a mistake in his approximation. A further problem is to find the field of force of an electron: this is carried out by J. B. Jeffery, *Proc. Roy. Soc.*, **99A**, 1921, 123–34, who gets a result agreeing with Nordström and Weyl and gives some applications; it is also discussed by K. Ogura, *Comptes Rendus*, **173**, 1921, 348–50, 407–8. Of purely mathematical interest is the paper by J. G. Campbell, "Einstein's Theory of Gravitation as a Hypothesis in Differential Geometry," *Proc. L.M.S.* (2), **20**, 1921, 1–14. E. Kasner, *Amer. J. Math.*, xliii, 1921, 126–9, 130–3, shows that an Einstein spread representing a permanent gravitational field can never be regarded as immersed in a five-flat, but that it can be represented in a flat space of six dimensions. Kasner also discusses, *ibid.*, 20–28, how the g's in ds^2 can be found if their ratios are given (by means of experiments with light-signals).

Other papers on relativity that should be of interest to the applied mathematician are:

WEYL, H., Über die physikalischen Grundlagen der erweiterten Relativitätstheorie, *Phys. Zeit.*, xxii, 1921, 473–80.

LODGE, O., Ether, Light, and Matter, *Phil. Mag.* (vi), **41**, 1921, 940–3.

EDDINGTON, A. S., The Relativity of Field and Matter, *ibid.*, **42**, 1921, 800–6.

SLATE, F., A New Reading of Relativity, and other papers, *ibid.*, **40**, 1920, 31–49; *ibid.*, **41**, 1921, 96–106, 652–64.

LIHOTZKY, E., Zur Frage der Verschiebung der scheinbaren Fixsternorte in Sonnennähe, *Phys. Zeit.*, xxii, 1921, 69–71.

BROMWICH, T. J. I'A., On Units and the Theory of Relativity, *Phil. Mag.* (vi), **42**, 1921, 431–2.

MORTON, W. B., Note on Einstein's Law for Addition of Velocities, *ibid.*, **40**, 1920, 771–4.

MCAULAY, A., Inertial Frames given by a Hyperbolic Space-time, *ibid.*, **41**, 1921, 141–3.

BUHL, A., Sur le rôle des symétries dans les théories relativistes, *Comptes Rendus*, **173**, 1921, 829–31.

BUHL, A., Sur la formule de Stokes dans l'espace-temps, *ibid.*, **171**, 1920, 547–9.

JUVET, M., Les formules de Frenet pour un espace de M. Weyl, *ibid.*, **172**, 1921, 1647–50.

WILSON, W., Space-time Manifolds and corresponding Gravitational Fields, *Phil. Mag.* (vi), **40**, 1920, 703–12.

ESCLANGON, E., Sur la relativité du temps, *Comptes Rendus*, **173**, 1921, 1340–2.

OGURA, K., Sur le champ statique de gravitation dans l'espace vide, *ibid.*, **173**, 1921, 521–3.

OGURA, K., Extension d'un théorème de Liouville au champ de gravitation, *ibid*, 766–8.

OGURA, K., Sur la théorie de gravitation dans l'espace de 3 dimensions, *ibid.*, 909–11.

EISENHART, L. P., The Permanent Gravitational Field in the Einstein Theory, *Proc. Nat. Acad. Sci., U.S.A.*, **6**, 1920, 678–82.

HILL, F. W., and JEFFERY, J. B., The Gravitational Field of a Particle in Einstein's Theory, *Phil. Mag.* (vi), **41**, 1921, 823–6.

WEDDERBURN, J. H. M., On the Equations of Motion of a Single Particle, *Proc. Roy. Soc. Edin.*, xli, 1921, 26–33, who modifies the classical equations of motion so that the time is also included in the Beltramic expressions.

JEFFERY, J. B., On the Path of a Ray of Light in the Gravitational Field of the Sun, *Phil. Mag.* (vi), **40**, 1920, 327–9.

OGURA, K., Sur la courbure du rayon lumineux dans le champ de gravitation, *Comptes Rendus*, **123**, 1921, 641–3.

FORSYTH, A. R., Note on the Path of a Ray of Light in the Einstein Relativity Theory of Gravitational Effect, *M.N., R.A.S.*, lxxxii, 1921, 2–11.

JEFFREYS, H., The Effect of Gravitation on Light, *Phil. Mag.* (vi), **42**, 1921, 470–1.

FLINT, H. T., On the Transformation of the Equations of Motion of the Dynamics of Continuous Media in the Restricted Theory of Relativity, *ibid.*, 794–99.

KOPFF, A., Bemerkung zur Rotationsbewegung im Gravitationsfeld der Sterne, *Phys. Zeit.*, xxii, 1921, 179–180, 309–10, who shows that the centrifugal and Coriolis forces also appear in Einstein fields.

A large output of work on hydrodynamics is one of the phenomena of the past year. The problem of flight makes it urgent to discover as much as possible about various kinds of fluid motion, while much of the modern theory of meteorology is based on the hydrodynamics of air. One of the classical problems of hydrodynamics is that concerned with finding the discontinuous stream-line motion past a barrier of more general form than that of a plane wing, and the applied mathematician will therefore welcome the series of volumes now being published by U. Cisotti under the title "Idromecanica Piana." Cisotti is himself responsible for very much of the recent work on two-dimensional hydrodynamics, and in the three volumes he intends to issue he will deal with the latest researches and methods. The first part has just been published (Milano, 1921). It contains a very valuable chapter on the various transformations of the complex variables used in two-dimensional hydrodynamical problems. It seems that very little attention has been paid in this country to the powerful ideas introduced by Levi-Civita and his followers during the past fifteen years, and that we are rather content to go on with the old methods. It is true that, even with the mathematics of the French and Italian writers, the direct solution of the problem of the curved barrier is yet quite remote; but Cisotti's book is nevertheless welcome, because it contains the solution of many problems of great interest and points the way to further useful and productive research. Two papers dealing with discontinuous motion are by W. B. Morton: "On the Discontinuous Flow of Liquid past a Wedge," *Phil. Mag.* (vi), **41**, 1921, 801–8, and by A. R. Richardson, "Stationary Waves in Water," *ibid.*, **40**, 97–110. The following papers on hydrodynamical problems, including the theory of the tides, should be noticed:

RIABOUCHINSKI, D., Equations du mouvement d'un fluide rapportées à des axes mobiles, *Comptes Rendus*, **173**, 1921, 698–701.

LECORNU, L., Sur le mouvement permanent des liquides, *ibid.*, **171**, 1920, 881–5.

VALCOVICI, V., Sur les forces hydrodynamiques dans les mouvements différant entre eux par une rotation uniforme de tout l'espace, *ibid.*, 619–21, who gives a theorem corresponding to the one that states that the pressure on a body moving in a liquid at rest is the same as when the solid is at rest and the liquid streams past it with the same velocity at infinity.

GRIALON, J., Sur le mouvement irrotationel et permanent d'un liquide, les trajectoires étant planes et verticales et le régime permanent, *ibid.*, **173**, 1921, 459–61.

RIABOUCHINSKI, D., Mouvement cyclique d'un liquide autour d'un solide qui se meut parallèlement à une paroi rectiligne, *ibid.* 25–6.

VILLAT, H., Sur les mouvements cycliques d'un fluide limité par un mur, et contenant un solide, *ibid.*, **172**, 1921, 359–61.

RIABOUCHINSKI, D., Mouvement initial d'un liquide en contact avec un obstacle à arètes vives, *ibid.*, 521–2.

VILLAT, H., Sur l'écoulement initial d'un liquide par un orifice brusquement ouvert, *ibid.*, 148–50.

RIABOUCHINSKI, D., Equations générales de mouvement de corps solides dans un fluid parfait incompressible, *ibid.*, **173**, 1921, 824–6.

OLSSON, O., Einige Anwendungen der hydrodynamischen Theorien von Kirchhoff-Clebsch, *Jour. für rein u. ang. Math.*, **150**, 1920, 113–56, dealing with solids moving in a perfect fluid under initial impulses but no further forces.

DATTA, B., On the Motion of Two Spheroids in an Infinite Liquid along their Common Axis of Rotation, *Amer. J. Math.*, xliii, 1921, 134–42, the spheroids having any ellipticities.

TAYLOR, G. I., Experiments with Rotating Fluids, *Proc. Camb. P.S.*, xx, 1921, 326–9.

TAYLOR, G. I., Experiments with Rotating Fluids, *Proc. Roy. Soc.*, **100A**, 1921, 114–21.

LECORNU, L., Sur le mouvement varié des fluides, *Comptes Rendus*, **172**, 1921, 350–3.

APPELL, P., Sur le mouvement périodique d'un fluide, *ibid.*, 885–6.

POCKLINGTON, H. C., Standing Wave Parallel to a Plane Beach, *Proc. Camb. P.S.*, xx, 1921, 308–10.

APPELL, P., Sur les oscillations ellipsoïdales d'une sphère, *Comptes Rendus*, **171**, 1920, 761–6.

RAV, J. C. K., On Ripples of Finite Magnitude, *Proc. Ind. Ass. Sci.*, vi, 1921, 175–93.

GHOSH, R. N., Some New Illustrations of Optical Theory by Ripple Motion, *ibid.*, 155–63.

HAVELOCK, T. H., The Stability of Fluid Motion, *Proc. Roy. Soc.*, **98A**, 1921, 428–37.

BURGERS, J. N., On the Resistance of Fluids and Vortex Motion, *Kon. Akad. Amst.*, xxiii, 1920, 774–782.

JAFFE, G., Uber den Transport von Vectorgrössen mit Anwendung auf Wirbelbewegung in reibenden Flüssigkeiten, *Phys. Zeit.*, xxii, 1921, 180–3.

DATTA, B., On the stability of two Rectilinear Vortices of Compressible Fluid moving in an Incompressible Liquid, *Phil. Mag.* (vi.), **40**, 1920, 138–48.

BERTRAND, G., L'équation de Fredholm et les masses statiques de la première sorte, *Comptes Rendus*, **173**, 1921, 1448–9.

TAYLOR, G. I., Tides in the Bristol Channel, *Proc. Camb. P.S.*, xx, 1921, 320–25, discussing the case of a channel in which both depth and breadth decrease uniformly to zero. The calculated results are compared with observation.

JEFFREYS, H., Tidal Friction in Shallow Seas, *Phil. Trans.*, **221A**, 1920, 239–64.

TAYLOR, G. I., Tidal Oscillations in Gulfs and Rectangular Basins, *Proc. L.M.S.* (2), **20**, 1921, 148–81.

STREET, R. O., The Tidal Motion in the Irish Sea, its Currents and its Energy, *Proc. Roy. Soc.*, **98A**, 1921, 329–44.

DOODSON, A., The Harmonic Development of the Tide-generating Potential, *ibid.*, **100A**, 1921, 305–29.

STREET, R. O., The Dissipation of Energy in Permanent Ocean Currents with some Relations between Salinites, Temperature, and Currents, *ibid.*, **99A**, 1921, 39–46.

MILNE, E. A., The Tensor Form of the Equations of Viscous Motion, *Proc. Camb. P.S.*, xx, 1921, 344–6.

HARRISON, W. J., On the Stability of the Steady Motion of a Viscous Liquid contained between two Rotating Coaxal Circular Cylinders, *Trans. Camb. P.S.*, xxii, 1920, 425–37. See also *Proc. Camb. P.S.*, xx, 1921, 455–9, for some corrections.

HARRISON, W. J., The Pressure in a Viscous Liquid moving through a Channel with Diverging Boundaries, *ibid.*, xix, 1920, 307–12.

HEMMY, A. S., The Flow of Viscous Liquids through slightly Conical Tubes, *Proc. Phys. Soc. Lond.*, xxxiv, 1921, 22–6.

WALKER, W. J., Fluid Discharge as affected by Resistance to Flow, *Phil. Mag.* (vi), **41**, 1921, 286–8; also *ibid.*, **42**, 1921, 138–9.

HAVELOCK, T. H., On the Decay of Oscillation of a Solid Body in a Viscous Fluid, *ibid.*, 628–34.

STANTON, T. E., MARSHALL, D., and BRYANT, C. N., On the Conditions at the Boundary of a Fluid in Turbulent Motion, *Proc. Roy. Soc.*, **97A**, 1920, 413–34, where it is shown experimentally that in turbulent motion there exists at the boundary a layer of fluid of finite thickness, which is in laminar motion, and which has zero velocity at the boundary.

JEFFREYS, H., On Turbulence in the Ocean, *Phil. Mag.* (vi), **39**, 1920, 578–86.

CARMICHEL, C., Sur les régime hydrauliques, *Comptes Rendus*, **173**, 1921, 630–2, 1061–3; see also D. Eydoux, *ibid.*, 701–3.

MESNAGER, M., Sur les applications du tube de Pitot, *ibid.*, **171**, 1920, 689–90; see also Y. Delage, *ibid.*, 646–51; M. Leaubeuf, *ibid.*, 1109–11.

DE SPARRE, M., Sur le rendement maximum des turbines, *ibid.*, **172**, 1921, 501–4, 896–9, 1561–4; **173**, 1921, 1045–9.

MIYAGI, O., Researches on the Theory of the Action of Centrifugal Pump Impellers, *Tech. Rep. Tôhoku*, i., 1920, 1–100.

In the problem of resisted motion a very important piece of work is that contained in a paper by R. H. Fowler, E. G. Gallop, C. N. H. Lock, and H. W. Richmond, entitled, "The Aerodynamics of a spinning Shell," *Phil. Trans.*, **221A**, 1920, 295–387, *Proc. Roy. Soc.*, **98A**, 1920, 199–205. This is an exhaustive experimental and mathematical investigation of practical as well as of theoretical importance, and the path of a spinning shell moving in air is calculated and plotted under various conditions. The resisted motion problem as applied to pilot balloons is discussed by C. A. Brazier, *Comptes Rendus*, **172**, 1921, 644–6, 756–8, M. Alayrac examines the motion of the centre of gravity of a body symmetrical about a vertical plane and moving in a resisting medium, *ibid.*, **172**, 1921, 1089–92. The equilibrium of particles in an ascending current is discussed by R. Feret, *ibid.*, 575–8; while A. Véronnet writes on the variation of a conic path in a resisting medium, *ibid.*, 267–9. The application to pendulum motion is the topic of a paper by

J. Andrade, *ibid.*, **171**, 1920, 664, who examines the case where the friction is proportional to the displacement, and by R. Serville, *ibid.*, **172**, 1921, 404–7, 470–2.

The study of aeronautics is proceeding apace. The first question that engages the attention of researchers is that of air-resistance. Theoretical methods are at present insufficient for practical needs, and aviation is based principally on experimental researches. A valuable paper is that by J. R. Pannell on "Fluid Resistance on Bodies of Approximately Streamline Form," *Aer. Jour.*, xxiv, 1920, 498–504. There is also a paper on the laws of fluid-resistance by E. Jouguet, *Comptes Rendus*, **171**, 1920, 96–9, while on resistance in viscous fluids there are papers by D. Riabouchinski, *ibid.*, **173**, 1921, 967–9, and by C. Wisselsberger. Experimental methods are discussed by W. Margoulis, *Comptes Rendus*, **171**, 1920, 997–9, and by J. Villey, *ibid.*, **172**, 1921, 270–2.

Aeroplane wings and surfaces have been the subject of much discussion, principally the Handley-Page wing, which is in the form of a slotted aerofoil, *Aer. Jour*, xxv, 1921, 263–89. Much is claimed for this new type of wing, and its introduction will no doubt give rise to further research on this most important subject. There is also a paper by H. B. Irving on "The Design of Aeroplane Control Surfaces, with Special Reference to Balancing," *ibid.*, 537–55.

Next comes the propeller, an ever-fruitful source of investigation. G. de Bothezat, whose work has already been mentioned (see SCI. PROG., 1921, 526) contributes a short paper on the fundamentals of the theory of blade-screws, *Aer. Jour.*, xxiv, 1920, 595–600, while notes on the subject are given by M. A. S. Riach, *ibid.*, 20–4, and H. C. Watts, *ibid.*, 406–12, both well-known workers on the theory of screw propellers.

The design of spars is the subject of papers by H. Booth, *ibid.*, xxiv, 1920, 563–74, and by H. P. Hudson, *ibid.*, xxv, 1921, 556–8. Miss Hudson also discusses the important question of "Incidence Wires," *ibid.*, xxiv, 1920, 505–16.

Helicopters are now a prominent subject of research, chiefly experimental; the heliocopter, however, also offers scope for mathematical treatment, A useful account of the state of the subject is given by L. Damblanc, *ibid.*, xxv, 1921, 3–19, and experimental flights are described by H. Pescara, *Comptes Rendus*, **172**, 1921, 845–7, by P. Painlevé, *ibid.*, 847–8, and by E. Oehmichen, *ibid.*, 366–8. H. Chatley writes a paper on "The Problem of Flapping Flight," *Aer. Jour.*, xxv, 1921, 492–503, a subject in which he has been interested for many years. An account of rigid airships is given by J. L. Bartlett, *ibid.*, 357–77.

The flight of birds is naturally a matter of interest to all who

pay any attention to the study of aeronautics, and the most mystifying phenomenon in this connection is the problem of soaring flight. Many explanations have been attempted of a matter which seems to defy the principles of mechanics. Recently a further attempt at an explanation was made by R. de Villamil in a pamphlet published in London in 1920 (Spon). His explanation, however, is not satisfactory, and the mystery remains unsolved. E. H. Hankin has been studying bird-flight for a considerable number of years, and he discusses the problem of soaring flight in two papers, *Proc. Camb. P.S.*, xx, 1921, 219–27, 460–5. He comes to the conclusion that this form of flight is not due to undiscovered wing movements, to lateral gusts of wind, to ascending gusts, or to convection currents produced by the sun's heat. He thinks that the problem is at present inexplicable, and suggests experimental investigation of the phenomenon. Soaring flight is known in connection with such differently constituted bodies as birds, flying-fishes, and dragon-flies, and Hankin thinks that low speeds are to be attributed to sunshine, while high speeds are to be attributed to wind.

A mild sensation has been caused by G. Brewer's "The Langley Machine and the Hammondsport Trials," *Aer. Jour.*, xxv, 1921, 620–64, wherein the author contends that the credit for having been the first to fly in an aeroplane is due entirely to the Wright brothers, and contests the claim that has been raised on behalf of Langley. Brewer brings evidence to show that the successful flights of the Langley machine carried out in 1914 were due to the very considerable modifications made in the original machine of 1903. There are replies by supporters of the Langley claim to priority.

Of great interest to the applied mathematician is the dynamical problem of the actual behaviour of an aeroplane in the air, including such questions as the greatest and least possible velocities of horizontal flight, the angle of glide for various configurations of the elevator, the rate of climb at different levels, the height the aeroplane can reach, or "ceiling," etc. To the student of dynamics the subject affords an excellent opportunity of useful research, of all grades of difficulty. The general problem is of great complexity, but the simpler questions that the practical designer is content to ask can be answered at the expense of quite elementary mathematics. A very useful and easy account of such mathematical processes has been written by H. Booth, entitled, "Aeroplane Performance Calculations" (Pp. xvi + 207, Chapman & Hall, London, 1921, 21*s*. net.). Mr. Booth's main object is to supply the designer with methods of calculating beforehand what the machine he is designing will do when it is taken up into the air, and in carrying

out this object he remembers that one must not expect too much mathematical knowledge of men engaged in what is a practical art based on technological science. The book thus makes easy reading.

Having set out the theory of the subject, with special reference to performance in the air, including cruising for long distances, on the ground and on the water (for a hydroplane), the author gives the exact procedure of carrying out these calculations in practice, and finally he shows the processes as actually applied to a definite machine.

The most interesting point in the theoretical portion is the author's introduction of the parameter λ which represents the ratio of the lift coefficient to the maximum value of the same coefficient. He is thus enabled to tabulate graphically the necessary corrections for aspect ratio, gap chord, stagger, dimensions, etc., in a very simple manner.

Recent papers also include the following:

Taylor, G. I., Scientific Methods in Aeronautics, *Aer. Jour.*, xxv, 1921, 474–91.

Glazebrook, R. T., Some Points of Importance in the Work of the Advisory Committee for Aeronautics, *ibid.*, xxiv, 1920, 479–95.

Richard, P. and M., Sur le problème général de l'aviation, *Comptes Rendus*, **173**, 1921, 758–60.

Brodetsky, S., Aeroplane Mathematics, *Math. Gas.*, x, 1921, 257–81, giving performance calculations in a very simplified form.

Hill, R. M., The Manœuvres of Getting Off and Landing, *Aer. Jour.*, xxv, 1921, 510–36, 665–71.

Hunsaker, J. C., Naval Architecture in Aeronautics, *ibid.*, xxiv, 1920, 321–405.

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Chief Assistant, Royal Observatory, Greenwich.

The Scale of the Universe. *Bulletin No.* 11 of the *National Research Council* of the National Academy of Sciences, Washington, contains two articles of great interest dealing with this subject by Harlow Shapley, formerly of the Mount Wilson Observatory and recently appointed Director of the Harvard Observatory, and Heber D. Curtis, Director of the Allegheny Observatory, respectively. These two writers are protagonists of two opposite views as to the scale of our Galactic System. Dependent upon the view taken as to the scale of this system is the further question as to whether the spiral nebulæ are "island universes" in space or are common members of one large system.

One of the main points upon which the discussion turns is the order of distance to be assigned to globular clusters. It is assumed that these belong to the Galactic System; their marked

concentration towards the galactic plane seems to indicate this, and both Shapley and Curtis accept the assumption. Shapley then uses the distances which he found for the globular clusters by various indirect methods to define the extent of the galactic system, and obtains a diameter for the system of the order of 300,000 light-years. Curtis denies the reliability of this figure, and argues in favour of a diameter of only about one-tenth the amount.

The process by which Shapley deduced the distances of the globular clusters may be briefly recalled. In the Lesser Magellanic Cloud a number of Cepheid variables were discovered by Miss Leavitt. Variables of this type are characterised by a light curve by means of which they may easily be identified, and it is believed that the light variation is due to pulsations in the atmosphere of the star. Miss Leavitt found that there was a definite relationship between the period and apparent magnitude of these stars. But this implies a direct relationship between period and absolute magnitude, since the Magellanic Cloud is so distant that the differences between the distances of individual variables may be neglected. Under such conditions absolute magnitude differs from apparent magnitude by a constant, which cannot be determined, however, unless the distance of the cloud can be determined.

Shapley found that in many globular clusters Cepheid variables occur, and investigation of their variation showed that in each case their periods and magnitudes fell on a curve which, by adjustment of its zero, could be fitted to the curve obtained from the variables in the Magellanic Clouds. These results served to confirm and extend the original curve. To determine the zero, Shapley discussed the near Cepheids in the Galactic System, and from their proper motions determined their mean parallax and, therefore, their mean absolute magnitude. From this curve, so standardised, the absolute magnitude and thence the distance of any Cepheid variable can be deduced, when its period of variation has been determined. In this way, and by certain extensions of the method to which it is not necessary now to refer, Shapley showed that the distances of the globular clusters range up to about 200,000 light-years.

Curtis accepts the relative distances determined by Shapley as correct, but criticises the method by which the zero is fixed, and argues in favour of a much smaller scale. The differences in the consequences resulting from the two view-points may be illustrated for the case of the Hercules cluster (Messier 13), for which Shapley finds a distance of 36,000 light-years, but for which Curtis would assign one of the order of 3,600 light-years.

—	36,000 light-years.	3,600 light-years or less.
(a) Mean absolute photographic magnitude of blue stars (C.I. < 0.0)	0	+ 5 or fainter
(b) Maximum absolute photographic magnitude of cluster stars	Between − 1·0 and − 2·0	+ 3·2 or fainter
(c) Median absolute photovisual magnitude of long-period Cepheids	− 2	+ 3 or fainter
(d) Hypothetical annual proper motion . .	0″·004	0″·04 or greater

As regards (*a*), stars whose colour-index is negative are blue stars of spectral type B. Such stars are all giants, of relatively large mass and high intrinsic luminosity. An absolute magnitude fainter than $+3^M$ is exceptional for a B-type star, and an average star of this type would only appear of the fifteenth magnitude, as in the Hercules cluster, if more than 30,000 light-years away. This seems decisive against the smaller distance assigned by Curtis.

As regards (*b*), Shapley compares the maximum absolute photographic magnitude of stars in various near-by groups and clusters: in no case were the brightest stars fainter than $+1^M{\cdot}0$. They are therefore giant stars. The distance assigned by Curtis to the globular clusters would imply that the brightest stars in clusters are dwarf stars; this is improbable when comparison is made with the near groups. But the matter has been directly tested at Mount Wilson by photographing the clusters through a thin prism on special plates sensitive in the red and blue regions, and relatively insensitive in the green-yellow. In this way small spectra divided in the middle are obtained, and the relative intensities of the red and blue regions can be compared. A comparison with spectra of known giants and dwarfs enables the conclusion to be made that in absolute brightness the cluster stars equal, or even exceed, the average giants.

Curtis has criticised the distances deduced by Shapley from the parallactic motions. But, as seen from (*c*) above, Curtis would make the Cepheids in Messier 13 five magnitudes fainter than Shapley; this would correspond to a mean annual parallactic motion of the order of 0″·160, which is much too great to be admissible.

As far as (*d*) is concerned, it may be deduced from the average radial velocity of the clusters that the mean proper motion of a cluster at a distance of 3,600 light-years would be of the order of 0″·04 per year. No cluster is known with so large a proper motion; in fact, the motion can hardly exceed 0″·01.

These four lines of evidence, therefore, concur in supporting distances of the order found by Shapley, and there would seem

to be little doubt but that the system of globular clusters is of the wide extent which he attributes to it. What, then, are the arguments of Curtis in favour of the smaller scale of distance?

Curtis denies, in the first place, the validity of the assumption that the Cepheid variables in clusters and in the immediate neighbourhood of our stellar system must obey the same luminosity-period relationship as the Cepheids in the Smaller Magellanic Cloud, and he further asserts that the method of fixing the zero of this relationship, by means of the parallactic motion of the nearer Cepheids, is based upon inadequate material—the small proper motions being uncertain and the number of stars too few to be reliable. But unless the Magellanic Cloud is a unique region of space, it is difficult to believe that the cause of Cepheid variation is other than an intrinsic property of the star itself, conditioned by its temperature and density. If a luminosity-period relationship holds for any one part of space, there seems no escape from the conclusion that it will also hold universally. Further, as already mentioned, the distances assigned by Curtis would require an impossibly large parallactic motion for the Cepheids.

Curtis further denies that it is legitimate to use the value $-1^{\mathrm{M}}.5$, found by Shapley as the mean luminosity of the twenty-five brightest stars in Messier 3, to deduce the distances of other clusters. He considers that the Cepheid variables are a small and possibly exceptional class, and that they are not typical of the stars in general. He discusses the brightness of the stars in several nearer open clusters, and obtains a mean value of about $+2^{\mathrm{M}}$. Assuming this value to hold for the globular clusters, he deduces a much smaller distance than Shapley. But this assumption is open to objection, for the number of stars in a globular cluster is much greater than in the open clusters, and amongst the relatively small number of stars in the latter it is hardly surprising that the mean magnitude of the brightest stars should be fainter than for the globular clusters. Moreover, Shapley uses the argument based upon the brightest stars in the cluster mainly to corroborate the distances found from the Cepheid variables, and the results obtained in the two ways support one another. The various lines of argument developed by Shapley are so concordant in their conclusions that there seems little room to doubt their validity.

Both authors discuss the status of the spiral nebulæ. Curtis is strongly in favour of the view that these constitute island universes, and obviously the large scale found by Shapley for our Galactic System would militate against this view. There is very little reliable evidence as to the distances of the spiral nebulæ. From the radial velocities found for spirals, which are of the order of 700 kms. per second, it may be deduced that

spirals at distances of 1,000 and 10,000 light-years respectively would have annual proper motions of $0''\cdot48$ and $0''\cdot048$ respectively. The former value is certainly too great to be admissible; the latter could scarcely be detected at present. It is therefore only possible to conclude from this argument that the distances are probably equal to, or greater than, 10,000 light-years. A second line of evidence, and perhaps the most promising at present available, depends upon a comparison of the linear velocities of rotation of spirals seen edgewise with the angular velocities of rotation found by van Maauen in the case of a few spirals observed broadside on. If the two rotations are assumed comparable, the distances deduced are of the order of 25,000 light-years. But this evidence is at present very slender, and it is doubtful to what extent the results can be regarded as typical of the large numbers of spirals in general.

Another argument has been based upon a comparison of the magnitudes at maximum of the galactic novæ and of novæ observed in the Andromeda nebula. Curtis gives the following figures:

—	Apparent magnitudes.	
	Thirty galactic novæ.	Sixteen novæ in Andromeda nebula.
At maximum . .	+ 5	+ 17
At minimum . .	+ 15 or fainter	+ 27 (conjectured)

—	Absolute magnitudes.		
	Four galactic novæ of known distance.	Sixteen novæ in Andromeda if at distances of	
		500,000 l.y.	20,000 l.y.
At maximum . .	− 3	− 4	+ 3
At minimum . .	+ 7	+ 6 ?	+ 13 ?

Curtis argues that these figures support a distance of 500,000 light-years rather than of 20,000 light-years. But it should be emphasised that the distances of the four galactic novæ have only been determined with a relatively large uncertainty; and, further, these four cannot be accepted as generally typical of galactic novæ. Novæ which at maximum only attain a low apparent magnitude may easily be missed, whilst probably all novæ which have recently appeared in the Andromeda nebula have been detected. The result would be to increase the discrepancy in the two sets of figures. Whilst, therefore, it appears probable that the distance of the Andromeda nebula exceeds 20,000 light-years, it appears very doubtful whether the distance can approach 500,000 light-years. But if not, the nebula would come within the limits of the Galactic System

as determined by Shapley, and could not be regarded as an island universe.

Curtis further argues that if the Andromeda nebula is at a distance of 500,000 light-years, its diameter would be 17,000 light-years, and therefore, in his opinion, of the same order of size as our Galaxy. Adopting, however, Shapley's dimensions for the size to be comparable, the distance assigned for the Andromeda nebula would be much greater than is admissible.

As an argument against the spirals being members of our Galactic System, Curtis supposes that all the spirals are of approximately equal size, the differences in angular diameter being due to differences in distance. He then concludes that even if the distance of the Andromeda nebulæ is only 20,000 light-years, the distances of the fainter nebulæ must extend to 10,000,000 light-years, or far outside our Galactic System. There is at present no evidence to support this assumption, and it seems strange that Curtis should deny that Cepheid variables in different parts of the Galactic System obey the same luminosity-period relationship, but should accept the hypothesis that the spiral nebulæ are of the same size wherever they occur.

The balance of evidence seems to be decidedly against the hypothesis that the spirals are comparable in size with our Galactic System. It may further be mentioned that Seares's measurements of the surface brightness of several spirals show that they are much brighter than that of the Milky Way, and Reynolds, from a study of the distribution of light and colour in typical spirals, has concluded that they cannot be stellar systems. Shapley considers the spirals to be nebulous bodies of great dimensions and distances, but not island universes. It will be possible to form some more definite conclusions as to their distances in the course of years, when, by the accumulation of material, it will become possible to determine with some accuracy the proper motion, or at least to assign an upper limit for the motions.

In conclusion, Shapley's summary of the results of accepting the restricted scale of the Galactic System may be quoted :

" If the distance of globular clusters must be decreased to one-tenth, the light-emitting power of their stars can be only a hundredth that of local cluster stars of the same spectral and photometric types. As a consequence, I believe Russell's illuminative theory of spectral evolution would have to be largely abandoned, and Eddington's brilliant theory of gaseous giant stars would need to be greatly modified or given up entirely. Now both of these modern theories have their justification : first, in the fundamental nature of their concepts and postulates ; and second, in their great success in fitting observational facts.

"Similarly, the period-luminosity law of Cepheid variation would be meaningless; Kapteyn's researches on the structure of the local cluster would need new interpretation, because his luminosity laws could be applied locally but not generally; and a very serious loss to astronomy would be that of the generality of spectroscopic methods of determining star distances, for it would mean that identical spectral characteristics indicate stars differing in brightness by 100 to 1, depending only upon whether the star is in the solar neighbourhood or in a distant cluster."

PHYSICS. By James Rice, M.A., University, Liverpool.

In a former number of Science Progress (**54**, Oct. 1919) an account was given in this section of experiments which had been carried out by Prof. Rutherford and his co-workers which gave considerable reason for the view that the atom of the element nitrogen could be disintegrated by bombardment with the very swift α-particles emitted by radium C. This work has been continued at the Cavendish Laboratory during the two years intervening, and further interesting and extremely important information is to hand showing that certain other atoms can be similarly disintegrated, and providing us with some data bearing on hypotheses which we may frame concerning the structure of an atomic nucleus.

Thus in No. 240 of the *Phil. Mag.* (Dec. 1920) Mr. Chadwick, at the suggestion of Prof. Rutherford, has performed a series of experiments to test more closely than had been possible the well-known hypothesis of Van der Broek that the charge on the nucleus of an atom bears to the electron charge a ratio equal to the atomic number of the element, *i.e.* the number of the element when all are arranged in order of increasing atomic weight. It is now some years since Barkla first showed by experiments on the scattering of X-rays by the light elements that the number of electrons in a light atom agreed approximately with half the atomic weight. Further evidence in support of this was forthcoming when Geiger and Marsden published their paper [*Phil. Mag.*, **25**, p. 604 (1913)] on the scattering of α-particles by matter, and showed that if one adopted the nuclear structure theory (which had just been put forward by Rutherford), the nuclear charge was approximately $\frac{1}{2} Ae$, where A is the atomic weight and e the electronic (positive) charge. The order of approximation was about 20 per cent. It was, however, the work of Moseley on X-ray spectra [*Phil. Mag.*, **26**, p. 1024 (1913), and **27**, p. 703 (1914)] which clearly indicated that the important number for an atom was not so much its atomic weight as the number indicating its place in a periodic classification. He showed experimentally

that the frequency of the lines of the X-ray spectra varied as $(N - a)^2$ where N is the atomic number of an element and a is a constant for the series. For the K lines a is about 1 and for the L lines about 7·4. Adopting the Bohr model of an atom and his hypothesis that radiation is emitted in quanta by electrons leaping from one steady orbit to another in the atom, Moseley's experimental law can be justified, provided, as stated above, N is an *integral* number which increases by unity as we proceed step by step along a complete periodic table of elements. As it happens N itself is about half the atomic weight of an element, and this fact brought the earlier work of Barkla and Geiger and Marsden into line with Moseley's results obtained by a different method. Mr. Chadwick, in view of the importance of obtaining fresh evidence on this point, undertook to repeat the experiments on the scattering of α-particles, which probably of all the methods gives a result most directly. The essentially new feature of his apparatus is the arrangement by which the scintillations produced by the α-particles in the scattered beam and those in the direct are counted on the same zinc sulphide screen and under the same conditions. This involves a very considerable increase in the accuracy of comparison of the two beams. His work has been carried out on platinum, silver and copper. For platinum (atomic number 78) he finds for N the value 77·4 ; for silver (atomic number 47) he finds N to be 46·3 ; for copper N is 29·3 (atomic number 29).

As regards the experiments concerned with the disintegration of atoms, reference can be made to the Bakerian Lecture of 1920 reprinted in the *P.R.S.*, A 686, 1920 (July), and to a lecture to the London Physical Society, reprinted in their *Proceedings* for August 1921. Prof. Rutherford's most recent work, however, is summarised in a paper contributed to the *Phil. Mag.* of last November. It is stated that in the earlier experiments (described in the *Phil. Mag.* of 1919 (June) and summarised in the number of SCIENCE PROGRESS referred to above) the scintillations due to the H atoms ejected from nitrogen were so few in number and feeble in intensity that it was difficult to decide with certainty whether the maximum range of the H atoms from nitrogen differed from that for the corresponding H atoms which were set in motion by recoil when α-particles pass through hydrogen.

The apparatus has been considerably improved in the meantime. A better form of microscope which aims at a larger field of view than before at the expense of magnifying power (a relatively unimportant feature) and protects the eyes of the counter from injury by γ-radiation, was designed ; special zinc-sulphide screens with a thin and finely powdered layer of the sulphide were used so that the corresponding reduction in

the luminosity due to γ-rays had a smaller effect in masking the α-ray scintillations which were to be counted. Using the usual device for preventing the β-particles striking the screen, that is deflecting them away by a magnetic field, it was possible to count the scintillations with the screen only 2·5 cms. away from a source of radium C, equivalent in γ-ray activity to 20 mgms. of radium. So concordant were the results that observations taken after an interval of six months were found to be in good agreement with those taken previously. A further advantage ensued from the good fortune of a gift of some radio-thorium of unusual activity. With this, deposits of thorium C on a nickel disk were obtained which ejected α-particles with a range of 8·6 cms., decidedly a more vigorous projectile than even the α-particle from radium C with its range of 7 cms.

With these appliances it has been decided beyond doubt that, whereas the H atoms due to recoil when α-ray particles from radium C pass through hydrogen have a maximum range of 29 cms. in air, *i.e.* about four times the range of the α-particles themselves, the particles from nitrogen bombarded by these same α-particles have a range of 40 cms. in air. This definitely shows that these particles are not due to the presence of free hydrogen or hydrogen in combination as a contamination. The bending of these particles by a magnetic field agrees with the bending which can be theoretically calculated for particles which have unit charge and the mass of an H atom and move with a velocity corresponding to this range of 40 cms., using Bragg's rule connecting speed and range.

It has further been discovered that in addition to nitrogen, particles whose ranges vary from 40 to 90 cms. of air are ejected from boron, fluorine, sodium, aluminium, and phosphorus when these are exposed to intense α-rays. Out of the elements between lithium and sulphur which have been examined the six mentioned above show the effect, the others do not. Some other elements of higher atomic weight than sulphur, *viz.* chlorine, magnesium, calcium, titanium, manganese, iron, copper, tin, silver, and gold, have also been examined without result; *i.e.* no particles whose length of range clearly decided that theywere not due to hydrogen contamination were observed.

Of the substances observed aluminium gave particles of a very considerable range, *viz.* 90 cms., phosphorus coming next with 65 cms.; boron gives particles with a range about 45 cms., while fluorine and sodium are much the same as nitrogen.

A notable feature of the particles from aluminium was the fact that the direction of their escape was largely independent of the direction of the bombarding stream of α-particles; in fact nearly as many were expelled in the backward as in the forward direction; but the range of the particles shot back was found to be

decidedly smaller, 67 cms. as against 90 cms. In the case of nitrogen this backward ejection was scarcely observable, being certainly less than 2 per cent. of the forward. Similar experiments on the remaining four elements have not yet been made.

In considering why some of the elements should show this effect and others in neighbouring places of the periodic table should not, it is a striking fact, as Rutherford points out, that of the elements so far examined only those whose atomic mass is given by $4n + 2$ or $4n + 3$, where n is a whole number, give rise to ejected H atoms. The explanation offered is that the nuclei of these elements are built up of helium nuclei of mass 4 and hydrogen nuclei. The study of radio-active transformations has already brought forward powerful evidence for the view that the helium nucleus is an important unit of atomic structure, in the case of the very heavy elements at all events. In the case of elements of atomic mass $4n$, there are no extra hydrogen nuclei present in the nucleus of the element, unless the helium nuclei themselves are composed of 4 hydrogen nuclei and 2 electrons; but if so, the structure of the helium nucleus is so stable that even the intense bombardment by swift α-particles is unable to break it up, and so no H atoms are ejected from such atoms. Where, on the other hand, there are present extra hydrogen nuclei, these can be broken away from the main nucleus and endowed with a considerable portion of the momentum and energy of the impinging α-particles. Of course, statistically very few indeed of the atoms of the bombarded substance suffer such a direct blow as to have their structure so shattered. Thus it appears that two α-particles only in one million are able to liberate a swift H atom. Rutherford regards the H atoms as being "satellites" to the main part of the nucleus consisting of a whole number of helium nuclei. We are, of course (as reference to the earlier work mentioned in the first paragraph will show), concerned with distances of separation of the order of the size of an electron or a nucleus itself, *viz.* 10^{-12} cm.; at such distances the ordinary law of force no longer holds; and these positively-charged nuclei are actually assumed to attract, so that the H satellites execute orbits around the main nucleus from which they are ejected by an α-particle which chances to come near enough, and for which the direction of ejection is in consequence somewhat fortuitous. In a former number of SCIENCE PROGRESS (50, Oct. 1918) will be found in this section a reference to some work of Prof. Nicholson on the modification of the law of force between two minute electric charges which, while conforming to the law of inverse squares for large enough distances, would give an actual inversion of sign followed, perhaps, by complete coalescence of two similar charges if brought sufficiently near.

PHYSICAL CHEMISTRY. By W. E. GARNER, M.Sc., University College, London.

Thermochemistry.—Notable advances have been made in recent years in the determination of the heats of formation of chemical compounds from the elements in the gaseous and monatomic state. Ultimately it will be possible to refer all heats of formation to this new datum-line, and to evaluate the heats of formation of the individual chemical linkings of any given chemical compound.

In the case of a few simple gaseous molecules, at comparatively high temperatures, it is possible to determine the heat of formation of the chemical linking, directly from the degree of dissociation. The heats of dissociation of the halogens and hydrogen have been obtained in this manner fairly accurately (Pier, *Zeit. Elektrochem.*, 1908, **62**, 417; Bodenstein, *Zeit. Elektrochem.*, 1910, **16**, 961 and 1916, **22**, 327; and Langmuir, *J.A.C.S.*, 1915, **37**, 417). The extension of our knowledge of crystal structure, ionisation potentials, and spectroscopy has made possible other methods of estimation of the energy of formation of chemical linkages (see Lewis, *Annual Reports*, *Chem. Soc.*, 1921). It is thus possible to deduce values for the heat of dissociation and ionisation of hydrogen, oxygen, and nitrogen molecules from the ionisation and resonance potentials of these gases. $Q = 23 \times E$, where E is the corresponding potential (. . .). Franck, Knipping, and Krüger find that the heat of dissociation of hydrogen determined in this manner is 81,300 calories, which is in agreement with the value obtained by Langmuir. Diatomic gases, such as hydrochloric acid, are more complex in their behaviour on ionisation; Knipping (*Zeit. für Phys.*, 1921, **7**, 328) following a method of formulation due to Haber, shows that hydriodic acid can undergo the following changes:

$$\begin{tikzcd}
HI & \frac{H_2}{2}, \frac{I_2}{2} \arrow[l, "Q_{HI}"'] & H, I. \arrow[l, "D_H + D_I"'] \\
H^+, I^- \arrow[u, "X"] & & H^+, I, e. \arrow[ll, "E_I"] \arrow[u, "I_H"']
\end{tikzcd}$$

where Q_{HI} = the heat of formation of HI. from H_2 and I_2,
where D_H and D_I = the work of dissociation of hydrogen and iodine respectively,

I_H = the heat of combination of the hydrogen ion with an electron,

E_I = the heat of formation of an iodine ion from the atom,

and X = the heat of formation of hydriodic acid from hydrogen and iodine ions.

Each of these quantities, which can be derived separately from experimental data, are connected by the relation :

$$-E_I = X - Q_{HI} - D_I - D_H - I_H.$$

The heat of formation of hydriodic acid from hydrogen and iodine atoms will be given by $Q_{HI} + D_H + D_I$ calories.

Calculations of the ionisation potentials made by Born and Fajans from their lattice theory of crystal structure, and by Franck from spectroscopic data are in good agreement with the values obtained experimentally for hydrochloric, hydrobromic, and hydriodic acids. This concordance proves the soundness of the theoretical basis on which these heats of dissociation and ionisation have been calculated.

It is necessary to adopt other methods of determination in the cases of the $-C-C-$ and $-C-H$ linkings, on account of the complexity of the reactions between carbon and hydrogen at high temperatures. Fajans (*Zeit. Phys.*, 1920, **1**, 101) shows that it is possible to derive the values for these linkings from the heats of combustion of the hydrocarbons provided that the heat of sublimation of carbon could be deduced.

The dissolution of the carbon-carbon and the carbon-hydrogen linkings, in the combustion of organic compounds, is accompanied by the oxidation of the carbon and hydrogen, to carbon dioxide and water. Thus the combustion of ethane can be analysed into four separate processes, which can be represented as follows :

$$(1)\quad -6x + 6v - y + 2z = 370{\cdot}9 \text{ Kcal},$$

where x represents the heat of dissociation of $C-H$ linking and y the value of the $C-C$ linking, whereas v and z represent the heats of oxidation of monatomic hydrogen and oxygen respectively.

Thomsen (*Zeit. Phys. Chem.*, 1887, **1**, 369) showed that the heat of combustion of the CH_2 group in a homologous series of carbon compounds was 156 Kcal. Thus :

(1) $-2x + 2v - y + z = 155{\cdot}8$ Kcal, and from (1) and (2) it follows that :

(3) $z - 2y = 96{\cdot}5$ Kcal (later values 101–105 Kcal).

The investigations of W. A. and W. L. Bragg on the X-ray analysis of the diamond supply data for the evaluation of z and y, since the carbon atoms in the diamond are arranged as in the aliphatic carbon compounds. If y^1 is the heat of formation of the $-C-C-$ linking in the diamond we can write for the heat of combustion of the diamond.

(4) $z - 2y^1 = 94{\cdot}4$ calories. This value differs by only two

calories from that given in (3); thus it may be concluded that the heat of formation of the – C — C – linking in the diamond is nearly identical with that of the same grouping in aliphatic compounds.

From the measurements of Franck, Knipping, and Krüger on the ionisation potential of hydrogen it can be shown that

$$H + \tfrac{1}{4}O_2 = \tfrac{1}{2}H_2O_{(Liq.)} + 74 \text{ Kcal}$$
$$\therefore\ v = 74 \pm 3 \text{ Kcal.}$$

Therefore a knowledge of the heat of sublimation of the diamond is all that is necessary for the complete solution of equation (1).

Up to the present only approximate values have been deduced for the heat of sublimation of carbon. Gruneisen has calculated, from the relationship between the atomic heat Cv and the coefficient of expansion of the diamond, that $\lambda_0 = 274$ Kcal. Fajans obtains a similar value $\lambda = 287$ Kcal from a consideration of the work of Lummer on the variation with pressure of the temperature of the positive crater of the carbon arc (3940–4200° abs.). Assuming that these temperatures represent the sublimation temperatures of carbon under the corresponding pressures, and applying the Clausius Clapyron equation to the vapour pressure curves which are obtained, the heat of sublimation has been deduced. Kohn (*Zeit. Phys.*, 1920, **3**, 143), while confirming the general conclusions of Fajans, finds that Lummer has made a miscalculation of the temperatures of the carbon arc. After correcting these errors the heat of sublimation is 168 Kcal, a value agreeing fairly well with that deduced from the Nernst heat theorem and the chemical constant of carbon (140 Kcal). These values are, however, too uncertain to make it possible to give the heats of formation of organic compounds from the gaseous elements in the monatomic state with any accuracy.

That this line of attack is fruitful has been demonstrated by Fajans (*Zeit. Phys. Chem.*, 1921, **99**, 395) in an investigation into the anomalies of the heats of formation of aliphatic hydrocarbons. He finds it necessary to reject the idea that each carbon and hydrogen atom is associated with only 4 and 1 valencies respectively. The more important subsidiary valencies which are exhibited in carbon compounds are those along the edges of the van t' Hoff tetrahedron. These " edge " valencies play a considerable part in the internal energies of the compounds. He points out that, whereas pentane and isopentane contain an equal number of principal – C – C – and – C – H linkings, the number of " edge " valencies is dissimilar. The energy in the – C – C – " edge " linking is quite appreci-

able, this being responsible for the low heat of combustion of the diamond,

$$z - 2y^1 = 94 \cdot 4 \text{ Kcal},$$

as compared with that of the carbon in aliphatic compounds viz. 101–105 Kcal. Hydrocarbons with side-chains contain more of the subsidiary – C – C – linkings, and it would be expected that their heats of formation would be higher and their heats of combustion lower than those of the isomeric straight-chain compounds. These and other similar relationships are found to hold.

Oxidation of Phosphorus.—Weiser and Garrison (*J. Phys. Chem.*, 1921, **25**, 473) have shown that the first step in the oxidation of phosphorus is the formation of phosphorus trioxide, and that this reaction takes place at a lower temperature than that of the further oxidation to the pentoxide. The formation of the pentoxide is accompanied by the emission of light, and the action of inhibitors destroying this luminescence is due to a retardation of this reaction. Positive catalysts like ozone, nitrobenzene, etc., react energetically with the trioxide, giving rise to an increased luminosity, while negative catalysts, being adsorbed on the surface of the trioxide, have a reverse effect. A stream of pure oxygen at 27° C. converts the vaporised phosphorus to the trioxide, which may be made to oxidise with luminescence by raising the temperature of the mixture. Ozone is formed during the latter change. Lord Rayleigh (*Proc. Roy. Soc.*, 1921, A. 99, 372), in a series of striking experiments, shows that the percentage of water present in the oxidation of phosphorus is important; air dried over suphuric acid does not inhibit the glow of phosphorus, but the presence of greater amounts of water produces a periodic luminosity. The glow travels like an explosion wave through the air saturated with phosphorus when sufficient water vapour is present. Small quantities of other inhibitors, such as turpentine, produce similar effects. When these are present in excess, the glow is completely extinguished.

Isotopes.—Bronsted and Hevesy (*Phil. Mag.*, 1922, **43**, 31) have succeeded in partially separating mercury into its isotopes by evaporation and effusion methods. A separation corresponding to a difference of 0·1 unit in the element weight of mercury has been obtained. Harkins and Hayes (*J.A.C.S.*, 1921, **43**, 1803) give an account of further experimental work on the separation of chlorine into its isotopes by the diffusion of hydrochloric acid gas. An increase in the atomic weight of chlorine of approximately one part in a thousand has been obtained.

Law of Distribution of Colloidal Particles in Colloidal Solu-

tion.—An interesting contribution to this subject has been made by Burton and Bishop (*Proc. Roy. Soc.*, 1921, A 100, 414), who have shown that the equation derived by Perrin for the distribution of gamboge particles under the action of gravity,

$$\frac{RT}{N} \operatorname{Log} \frac{n}{n_0} = V(\rho_1 - \rho_2)g(h - h_0),$$

does not hold for colloidal solutions of copper, where the particles are much smaller. They find that the colloidal particles are uniformly distributed throughout a column of liquid 90 cm. in height after standing for fifty days. From Perrin's equation it may be deduced that the ratio of the concentrations in such a solution at any two levels 1 cm. apart would be given by the equation—

$$\operatorname{Log} \frac{n_1}{n_0} = 5{,}000.$$

The authors point out that this equation can only hold over an extremely small depth of liquid, below which a uniform distribution of the particles occurs. The concentration of colloidal solutions of the metals rarely exceeds 10–20 mgrm. per 100 cc. Attempts to increase the concentration of such colloidal solutions by slow evaporation causes settling of the metal.

Two papers on the catalytic oxidation of carbon monoxide at ordinary temperatures have appeared (*J.A.C.S.*, 1921, **43**, 1973 and 1982). It is shown that mixtures of manganese dioxide with other oxides are the best catalysts. The best promoters appear to be the oxides of copper, cobalt, and silver. For use in gas-masks it is, however, necessary to protect the catalyst from moisture by means of anhydrous calcium chloride.

Bone and Haward (*Proc. Roy. Soc.*, 1921, [A] 100, 67) have made accurate measurements of the rate of explosion of mixtures of hydrogen and oxygen and carbon monoxide and oxygen at high pressure. Striking differences between the behaviour of the two gaseous explosives are observed. The rise in pressure in the hydrogen and oxygen mixture is practically instantaneous, and the reaction is rapidly completed. On the other hand, the explosion of carbon monoxide and oxygen takes place more slowly, and the reaction continues after the maximum pressure has been reached. The presence of 1 per cent. of hydrogen in the carbon monoxide mixture, however, catalyses the rate of explosion, giving an entirely different form to the explosion curve.

ORGANIC CHEMISTRY. By O. L. BRADY, D.Sc., F.I.C., University College, London.

Organo-metallic Compounds.—(1) Chromium. Mendeléeff at one time considered that it was unlikely that the metals of the

even series of the Periodic Table would give organo-metallic derivatives, while Zeltner, in 1908, thought that the metals of the sixth group gave no indication of the formation of compounds of this type. Considerable interest attaches, therefore, to the preparation of organo-derivatives of chromium by Hein (*Ber.*, 1921, **54**, 1905). Some years ago Bennett and Turner (*Trans. Chem. Soc.*, 1914, **105**, 1057) attempted to prepare organo-derivatives of chromium by the action of phenyl magnesium bromide on chromic chloride, but without success, the main product of the reaction being diphenyl.

$$2CrCl_3 + 2C_6H_5MgBr \rightarrow C_6H_5.C_6H_5 + 2CrCl_2 + MgCl_2 + MgBr_2.$$

In a subsequent paper (*J. Roy. Soc. New South Wales*, 1919, **52**, 100) they pointed out that evidence had been obtained of the formation of organo-metallic derivatives of chromium, tungsten, and iron, but apparently did not succeed in isolating any individual. Hein has been more successful, and, using Bennett and Turner's method, but without heating the reaction mixture, has obtained chromium pentaphenyl bromide, $Cr(C_6H_5)_5Br$. This compound is described as an orange-brown amorphous powder difficult to purify on account of its instability; the mercuri-chloride $Cr(C_6H_5)_5Br.HgCl_2$ can, however, be obtained reasonably pure. From the crude bromide there has been prepared a crystalline hydrated chromium pentaphenyl hydroxide, which conductivity measurements show to be a strong base.

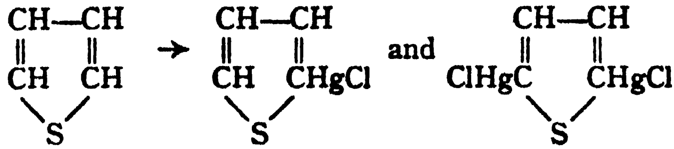
(2) Lead. Krause and Schmitz (*Ber.*, 1919, **52**, 2165) obtained evidence of the existence of organo-derivatives of lead analogous to triphenyl methyl, in which the metal is attached to but three hydrocarbon radicles. When magnesium *p*–2–xylyl bromide acts upon lead dichloride, instead of lead tetraxylyl, a crystalline compound is obtained which analysis and molecular weight determinations indicate to be $[(C_8H_9)_3Pb]_2$. The properties of this compound, notably the ease with which it reacts with halogens at low temperatures, afford some evidence that the union between the lead atoms is very feeble, and that it might be regarded as lead tri-*p*-2-xylyl. It reacts with bromine in pyridine solution at −40° to give lead tri-*p*-2-xylyl bromide, which with magnesium *p*-2-xylyl bromide yields lead tetra-*p*-2-xylyl, $(C_8H_9)_4Pb$. Krause (*Ber.*, 1921, **54**, 2060) has now obtained important confirmation of these views by the preparation of lead tricyclohexyl, $Pb(C_6H_{11})_3$, from lead dichloride and magnesium cyclohexyl bromide. Lead tricyclohexyl, which is obtained in 50 per cent. yield, is a yellow crystalline compound decomposing at 195°. The pure substance is

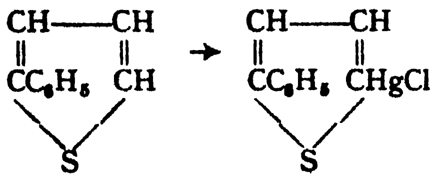
stable in the dark, but decomposes on exposure to light; its solution in benzene is unstable even in the dark. It dissolves in benzene to a yellow solution, and molecular weight determinations in this solvent show that it exists as the simple molecule. The solution reacts at ordinary temperatures quantitatively with iodine, with the formation of lead tricyclohexyl iodide, $Pb(C_6H_{11})_3I$, which with potassium hydroxide gives the hydroxide $Pb(C_6H_{11})_3OH$. This is converted into the chloride and bromide by the corresponding halogen acids. On warming lead tricyclohexyl iodide in benzene with iodine, lead dicyclohexyl di-iodide, $Pb(C_6H_{11})_2I_2$, is formed, and the corresponding dibromide is quantitatively produced by the action of bromine in chloroform solution. By the action of magnesium cyclohexyl bromide on lead tricyclohexyl bromide, lead tetracyclohexyl is obtained.

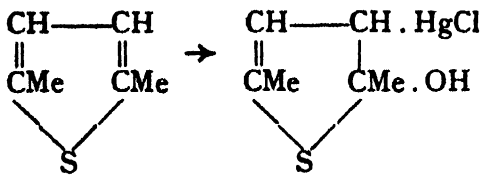
(3) Bismuth. Challenger and Allpress (*Trans. Chem. Soc.*, 1921, **119**, 913) have published a further contribution to the study of the tertiary aromatic bismuthines. In consequence of the failures which accompanied many attempts to prepare mixed aromatic bismuthines of the type $BiR'R''_2$ and bismuthonium haloids, $BiR'_3R''X$, by the action of magnesium aryl haloids, the reaction of this reagent on halogen derivatives of the type BiR_2X, $BiRX_2$, and BiR_3X_2 has been more thoroughly investigated. The action of magnesium-4-*m*-xylyl iodide on diphenyl-chlorobismuthine and di-*p*-tolyl-chlorobismuthine gave tri-phenyl- and tri-*p*-tolyl-bismuthines respectively. Similar results were obtained in a number of other cases, the triarylbismuthine produced corresponding to the halogen bismuthine employed. When, however, a large excess of the Grignard reagent is employed, interchange of groups seems to take place. *a*-naphthyldibromobismuthine with six molecular proportions of magnesium phenyl bromide gives triphenyl-bismuthine and diphenyl-*a*-naphthyl-bismuthine in almost equal amounts; and phenyl-dibromo-bismuthine with five and a half molecular proportions of magnesium *a*-naphthyl bromide gives much tri-*a*-naphthyl-bismuthine.

(4) Mercury. An increasing amount of attention is being paid to organo-mercury compounds, partly on account of their pharmacological importance, and a number of papers have been published recently, though not many are of much theoretical interest. In a long paper (*Annalen*, 1921, **424**, 23) Steinkopf continues his work on the mercury compounds of thiophen. In a previous paper (*Annalen*, 1914, **403**, 50) he came to the conclusion that thiophens in which the 2 and 5 positions were unsubstituted gave mono- and di-mercurichlorides, those in which one of these positions was substituted, mono-mercurichlorides, and those in which both positions were

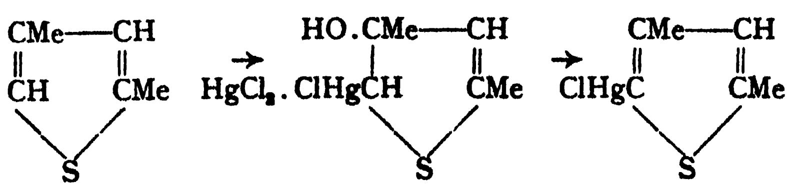
substituted did not react, or formed additive compounds containing a mercurichloride group in position 3.



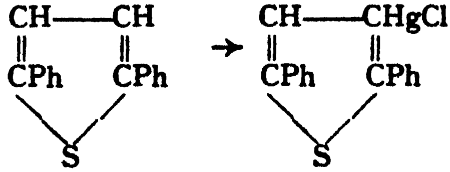




He pointed out, however, that certain irregularities occurred. Further work has emphasised the fact that the above conclusions do not invariably hold. For example, 3:4 diphenylthiophen does not react with mercuric chloride in the cold, and behaves abnormally when heated with this compound ; again, 2:4-dimethylthiophen [i] and 2:5-diphenylthiophen [ii] both behave in an unusual way.



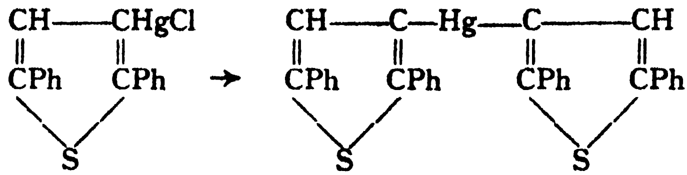
[i]



[ii]

In addition, the conversion of the mercurichlorides into the mercury dithienyls by the action of sodium iodide is not, as was formerly thought, a reaction characteristic of mercurichlorides of thiophen in which the mercurichloro group is

attached to the 1 or 5 carbon atoms, as 2:5-diphenylthiophen-3-mercurichloride [iii] readily yields 3-mercury-di-2:5-diphenylthiophen.



[iii]

White (*J. Amer. Chem. Soc.*, 1920, **42**, 2355) has described a number of mercury derivatives of the phthaleins. The mercury enters in the ortho position to the hydroxyl group when this is free, and may be introduced by the action of yellow mercuric oxide on a solution of the phthalein salt, when phthalein mercurihydroxides are formed, or by means of mercuric acetate, which gives acetoxymercury derivatives, or a mixture of these with the hydroxides owing to partial hydrolysis. Hart and Hirschfelder (*J. Amer. Chem. Soc.*, 1920, **42**, 2678) have obtained mercurihydroxides from phenylcarbinols such as saligenin and *m*-nitro-*p*-hydroxybenzyl alcohol. These have an antiseptic action of the order of mercuric chloride, but are far less irritant.

Whitmore and Middleton (*J. Amer. Chem. Soc.*, 1921, **43**, 619) have found that mercuric acetate reacts with excess of phenol in the absence of a solvent to form the *o*- and *p*- mercuriacetates, the disubstituted product not being produced as in aqueous solution. The phenol-mercurichlorides, on boiling with a solution of potassium iodide, are decomposed, giving phenol, mercuric iodide, and potassium hydroxide; potassium bromide is less effective, while the chloride is without action. When, however, the phenolic hydroxyl group is acetylated, this reaction does not take place, the mercuriiodide being formed, together with a small amount of the mercury diphenyl derivative. The action of haloid salts in eliminating mercury from organo-mercury compounds is not common where the Hg–C linkage is concerned, particularly in the aromatic series, although a few cases have been reported. This reaction is of considerable importance in connection with the therapeutic use of these compounds.

Other papers are those of Vecchiotti (*Gazzetta*, 1921, **51**, ii, 208) on the position of the mercury group in *p*-toluidine-mercuriacetate; Oliveri-Mandalà (*Gazzetta*, 1921, **51**, i, 125) on derivatives from antipyrine and pyramidone; Paolini (*Gazzetta*, 1921, **51**, ii, 188) on mercuriacetates of thymol, phenol, β-naphthol and vanillin; Tiffeneau and Gannagé

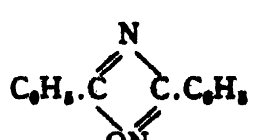
(*Bull. Sci. Pharmacol.*, 1921, **28**, 7) on mercury cyclohexyl, mercury methyl cyclohexyl, and their derivatives; Binz and Bauer (*Zeitsch. angew. Chem.*, 1921, **34**, 261) on the action of mercuric chloride on salvarsan and neosalvarsan; Stieglitz, Kharasch, and Hanke (*J. Amer. Chem. Soc.*, 1921, **43**, 1185) on 5:5′-mercuri-bis-3-nitro-4-hydroxyphenylarsinic acid; Kharasch and Chalkley (*J. Amer. Chem. Soc.*, 1921, **43**, 607) on nitrobenzene mercury compounds.

Dimroth (*Ber.*, 1921, **54**, 1504) defends his view that in the mercuration of phenolic ethers the reaction consists in the replacement of a hydrogen atom by the –HgX group against the criticism of Manchot and Bössenecker (*Annalen*, 1920, **421**, 331). These authors suggest that the primary product of the action of mercuric acetate is a hydrolysed additive compound; for example, [OMe.C_6H_5, Hg.OH]OH, which on treatment with alkali haloids gives the nuclear mercurated compound. Dimroth comes to the conclusion that Manchot and Bössenecker's compounds were mixtures. In the case of the substance they obtained from anisol, for example, it could be converted by the action of iodine in potassium iodide into a mixture of *p*-iodi-anisol and 2:4-di-iodoanisol, and was therefore a mixture of *p*-anisylmercuriacetate and 2:4-anisylenedimercuriacetate, which could be actually separated.

Oxidation products of the Oximes.—Some confusion has previously existed with regard to the oxidation products of benzaldoxime, and a reinvestigation of this problem by Robin (*Annales de Chimie*, 1921, **16**, 75) seems to have cleared up many of the doubtful points. Previous investigators had used amyl nitrite, nitrogen peroxide, or potassium ferricyanide as oxidising agents, but Robin makes use of iodine in the presence of sodium carbonate. Ketoximes resist the action of this reagent, and aldoximes of the type phenylacetaldoxime are decomposed with the formation of the corresponding aldehyde; an interesting exception, however, is provided by cinnamic aldoxime, which behaves in a similar manner to benzaldoxime, thus bearing out its other resemblances to this compound. Benzaldoxime is oxidised by iodine and sodium carbonate to the peroxide [iv], together with small quantities of dibenzenylazoxime [v], benzoylbenzaldoxime [vi], and dibenzenyloxoazoxime [vii].

$C_6H_5.CH : N.O$
$C_6H_5.CH : N.O$

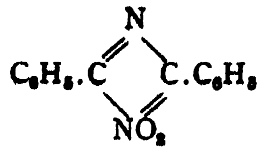
[iv]



[v]

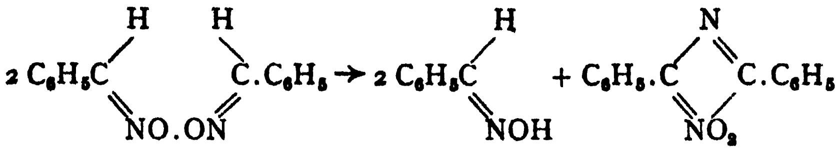
$C_6H_5.CH : NO.COC_6H_5$

[vi]



[vii]

On keeping in a closed vessel benzaldoxime peroxide was found to decompose, giving benzaldoxime, benzoic acid, and dibenzenylazoxime, but no dibenzenyloxoazoxime nor benzoylbenzaldoxime. On the other hand, the peroxide, on boiling with benzene, gave benzaldoxime and dibenzenyloxoazoxime.



This is not in keeping with the results of Beckmann (*Ber.*, 1889, **22**, 1588) and of Wieldand and Semper (*Ber.*, 1906, **39**, 2522), who state that dibenzenylazoxime is formed under these conditions. Robin also failed to obtain, by the oxidation of benzaldoxime peroxide with iodine and sodium carbonate, the peroxide of benzil dioxime which Beckmann obtained by the use of nitrogen peroxide as the oxidising agent.

The action of iodine and alkali on benzsynaldoxime has not been studied, but Robin erroneously supposes that benzaldoxime obtained in the usual manner and distilled *in vacuo* consists of a mixture of the two isomerides.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

Petrology.—A valuable summary of the physical chemistry of the crystallisation and magmatic differentiation of igneous rocks is proceeding in the *Journal of Geology* (**29**, 1921, 318–50 ; 426–43 ; 515–39 ; 627–49) from the pen of the veteran J. H. L. Vogt. The review of mineral relations during crystallisation follows both synthetic and analytic methods ; the former being based on the experimental investigation of silicate melts by the Geophysical Institute, and on Vogt's own work on slags ; the latter based on the study of the texture and structure of the rocks. In this way a mass of observations is being brought together elucidating the order of crystallisation, the individualisation-fields, the mix-crystal systems, eutectic intergrowths, etc., of the rock-forming minerals.

Consideration of the Raana norite intrusion in the Ofoten Fiord of Northern Norway has yielded to S. Foslie a remarkable

confirmation and extension of Bowen's theory of differentiation by the straining off of residual liquid from a crystallising mass (*Journ. Geol.*, **29**, 1921, 701–19). The intrusion consists of a central mass of quartz-norite surrounded by a marginal zone of normal norite, in which there are smaller bodies of olivine-rich types. The crystallisation of the intrusion went on under directed pressure due to Caledonian folding. As crystallisation proceeded inward from the margins, and the crystal mesh became strong enough to take up the stress, the more acid residual liquid was expressed towards the intérior, thus contributing to the formation of a quartz-rich norite in that position. In Foslie's words, "the wave of crystallisation is followed by a wave of squeezing." This conception makes a valuable addition to Bowen's general theory, and may, in the writer's opinion, throw light on the origin of schlieren in igneous rocks.

N. L. Bowen has experimentally determined the rate of diffusion in certain silicate melts by permitting diffusion against gravity of a heavy liquid into a lighter one (*Journ. Geol.*, **29**, 1921, 295–307). The value of ·25 is taken as near the average diffusivity in igneous magmas measured in grams passing through unit area per day. On this basis it is shown that the formation of border phases in igneous intrusions by diffusion towards the cooling margins cannot take place in the time available. It is shown, for example, that after sixty-four years the precipitating effect would be felt only at a distance of 3·3 metres from the margin, the rest of the magma being unaffected. After 256 years a layer only 8 cms. thick of the first-crystallised mineral would be formed, all the material being taken from a border portion less than 7 metres wide.

In discussing the origin of the alkalic igneous rocks, Dr. J. W. Evans (*Report Brit. Assoc., Cardiff*, 1920, p. 354) points out that they occur mainly where the earth's crust is thick, the heat-gradient low, and where there has been no folding since remote times. Magmas under these conditions of high pressure and comparatively low temperature would crystallise with the formation of minerals with small molecular volumes, garnets, kyanite, epidote, and zoisite, all rich in lime and alumina. Hence there should be a residual magma rich in alkalies which would be able to reach the surface where faulting and fissuring occurred.

An intrusion of alnoitic rocks at Isle Cadieux, near Montreal, has been studied by N. L. Bowen (*Amer. Journ. Sci.* (v), **3**, 1922, 1–34). This rock contains monticellite (lime-olivine), and it is believed to have consisted originally of augite and chrysolite. With lowering of temperature these minerals were attacked probably by their own interstitial liquid, and were replaced by melilite, monticellite, and biotite, with marialite,

perofskite, and titaniferous magnetite, as minor products of the reaction. An associated melilite-biotite rock is believed to be the result of the further replacement of monticellite by these minerals. The residual liquid was alkalic; and Bowen has shown experimentally that nepheline reacts with diopside to form melilite, a reaction analogous to that taking place in the natural rock. The formation of melilite and other lime-rich minerals may therefore be due to normal equilibrium processes in the magma, and not to the assimilation of limestones.

The veteran petrologist Prof. W. C. Brögger has now added a fourth volume to his classic work on the alkaline igneous rocks of the Kristiania region of Norway (*Die Eruptivgesteine des Kristianiagebietes*, IV, Das Fengebiet in Telemark, Norwegen. *Vidensk. Skr.*, I, Math.-Nat. Klasse, No. 9, Kristiania, 1920, pp. 408). The area dealt with occupies only 4 square kilometres, but is nevertheless replete with the most vivid petrographical interest. No less than 13 new rock types are described, the greatest novelty being the demonstration of the existence of a series of magmatic carbonate rocks, which are associated with an ijolite series containing a variable amount of primary calcite. There are also nepheline-syenites and alkali-syenites, with numerous hybrid types. The carbonate rocks are believed to have been derived from a mass of sedimentary limestone melted in depth; and the central mass of carbonatite is regarded as having floated upon the heavy silicate magma of the region. The age of this series, first regarded as an outlier of the Devonian Kristiania petrographic province, is now believed to be late Pre-Cambrian, a view which brings its petrographical characters and age relations into line with those of numerous other scattered provinces throughout Scandinavia (*e.g.* Alnö). The enormous wealth of observation in this great memoir makes it a veritable mine of interest for petrographers, and quite precludes an adequate review in this place.

By a curious coincidence Prof. S. J. Shand has just described an extremely similar occurrence to that of the Fen district in Sekukuniland (Transvaal), within the Bushveldt granite (*Trans. Geol. Soc. South Africa*, **24**, 1921, 111–49.) This forms a roughly oval area of 7 sq. miles consisting of differentiated nepheline syenites and ijolites surrounding a block of limestone, the ijolite being found in a belt adjacent to the limestone. The latter is regarded as a block of the Dolomite group of the Transvaal Series brought up by the rising magma. The formation of the ijolite between nepheline-syenite and limestone is shown to be due to the assimilation of limestone by the original nepheline-syenite magma, roughly one-third of its own weight of limestone being absorbed.

C. H. Clapp's study of the igneous rocks of Essex Co.,

Massachusetts (*Bull.* 704, *U.S. Geol. Surv.*, 1921, pp. 132), throws much light on such topics as the sequence of volcanic, batholithic, and dyke phases in an igneous episode ; the differentiation of subalkalic and alkalic magmas ; origin of shatter-breccias along irruptive contacts ; and the formation of hybrid rocks. Probably the most startling conclusion in the work is that the type *essexite* of Salem Neck is a hybrid rock due to interaction between an older subalkalic gabbro and a younger nepheline-syenite. It is shown, however, that this hybrid differs in no essential chemical or mineralogical characters from essexites which have certainly resulted from magmatic differentiation ; and that hybrid rocks may therefore be identical in some cases with normal igneous types.

A very detailed study of the igneous geology of the well-known Karroo dolerites of South Africa has been made by Dr. A. L. du Toit (*Trans. Geol. Soc. South Africa*, **23**, 1921, 1–42). He shows that a kaleidoscopic repetition of sheets and dykes of dolerite occurs intruding the Karroo and older rocks over an area of at least 220,000 sq. miles. The intrusions are dyke-like in the difficultly-penetrable basement of ancient rocks, but are spread out into concordant forms within the horizontal Karroo sediments. From various considerations it follows that the relative order of injection was from the summit down to the base of the sedimentary block. There is a very interesting discussion of the geophysical aspect of the Karroo eruptions. From a study of this and similar areas " it would appear that the prolonged accumulation of sedimentary infillings of this type (Karroo) led to ultimate folding, upheaval, and penetration by basic eruptives on an extensive scale."

Kinkell Ness, " the largest, best exposed, and most interesting volcanic vent along the northern shores of Fife," part of which has been eroded into the famous Rock and Spindle near St. Andrews, has been fully described by D. Balsillie (*Geol. Mag.*, 1919, 498–506). Like many of the larger Scottish vents Kinkell Ness has a peripheral zone of non-volcanic débris, suggesting that the funnel shape of these vents is of late origin, appearing when the sedimentary walls began to disintegrate and slump into the central pipe.

The large double volcanic vent of the Heads of Ayr, finely exposed in both cliff and shore section, has been described by the writer (*Trans. Geol. Soc. Glasgow*, **16**, pt. 3, 1920, 339–63). The infilling material is mainly the subjacent Old Red Sandstone andesite lavas, although the vent itself is of Early Carboniferous age. It also contained numerous boulders of lherzolite. The agglomerate is tumultuous and unstratified towards the centre of the vent, but is well stratified, and in vertical beds, at the western margin. The agglomerate is pierced by several

dykes and bosses of analcite-nepheline-basalt, which are believed to be due to a reawakening of the focus in Late Carboniferous or Permian times.

The classification and nomenclature of igneous rocks has been recently discussed in the following papers :

L. V. Pirsson, The Classification of Igneous Rocks—A Study for Students, *Amer. Journ. Sci.* (v), **2**, 1921, 265–84.
Dixey, F., The Magnesian Group of Igneous Rocks, *Geol. Mag.*, 1921, 485–93.
Tyrrell, G. W., Some Points in Petrographic Nomenclature, *ibid.*, 494–502.
Hackmann, V., Some Criticisms of Idding's Classification of Igneous Rocks, *Bull. Comm. Geol. Finlande*, **53**, 1920.

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., University College, London.

Plant Distribution.—Willis, in 1919 (*Ann. Bot.*, July), restated his " age and area " hypothesis in the following terms : " The area occupied at any given time, in any given country, by any group of allied species, at least ten in number, depends chiefly, so long as conditions remain reasonably constant, upon the ages of the species of that group in that country. . . ." The positive action of age and the profoundly modifying action of geographical, climatic, and ecological barriers, which exert a negative influence, were at the same time emphasised.

Later, Willis dealt with the plant invasions of New Zealand from this point of view (*Ann. Bot.*, October 1920), and it was shown that the present distribution of the genera, when classified according to the density of species in the different parts of New Zealand, is indicative of several invasions by land, viz. a western, probably by the Lord Howe Island ridge ; a Kermadec ; a northern and a southern. The northern invaders are represented by genera of *Proteaceæ*, *Lauraceæ*, *Myrtaceæ*, etc. ; the southern by *Cardamine*, *Veronica* ; and the western by *Clematis*, *Senecio*, etc. The Kermadecs, Norfolk, and Lord Howe Islands contain a number of endemics which would naturally be expected to be more numerous in the early arrivals than in genera whose arrival was comparatively recent. It is, therefore, very significant from the standpoint of " age and area " that most of these endemic species belong to genera which have reached New Zealand.

Quite recently Willis has given a summary of his views (*Ann. Bot.*, October), and incidentally offers several warnings with respect to their applications. He regards plant distribution in the present era, with respect to its broad outline, as governed mainly by the time factor, whilst the detailed distribution is chiefly dependent on ecological factors.

The subject of endemics is here dealt with, and these are shown for the Pacific Islands to be most numerous in the larger, and hence presumably older, genera ; also in islands

which have the greatest number of widespread species, and which were therefore presumably colonised at a relatively early date. Such facts, and the limited average range of endemics as compared with non-endemics, point clearly to the influence of the time factor. That is, the endemic species is, in most cases, one which has not had time to spread, or alternatively has been hindered from so doing by geographical or biological barriers.

All the 1582 endemic genera of the world's islands are considered in this paper, and it is shown that the first forty families, in order of the number of endemic island genera which they embrace, include no fewer than thirty-one of the first forty largest families in the world. Herbaceous families tend to present few endemics, which is consistent with the probably more recent origin of the herbaceous type.

Of the 145 non-endemic families which have endemic genera on islands, 90 per cent. reach both the Old and New Worlds, while only 52 per cent. do so in those families which contain no endemic genera.

The facts, then, appear to indicate that the endemics of tropical islands are almost entirely the more recently produced species, and probably but rarely unsuccessful survivals.

Norman Taylor, in the same journal, discusses the endemic flora of the Bahamas. This consists of 185 species, embracing 132 flowering plants out of a total of 894 species. Analysis shows that the percentages of the species found on one, two, three, or many islands do not differ appreciably as between the endemics and non-endemics, and it is suggested that the age of these Bahama endemics cannot be measured either by their area or frequency. It is, further, in contrast to Willis's results that the percentage of herbaceous species is here greater for the non-endemics (59.3 per cent.) than for the endemics (39.3 per cent.). The facts appear to suggest that the numerous endemics are related to the peculiar climatic conditions rather than to age.

Genetics, etc.—Ostenfeld has shown that in the section *Pilosella* of the genus *Hieracium*, new forms have arisen in consequence of hybridisation, and are then kept pure by apogamy. In the section *Archieracium* very few species are sexual, amongst the exceptions being *H. umbellatum* and *H. virga-aurea*. Recently, Ostenfeld has described (*Genetics*, September) the origin of two apogamic mutants from a parent which was itself apogamous, viz. *Hieracium rigidum*. It is suggested that in both sub-genera the real cause of the appearance of new forms is hybridisation in the past, whilst apogamy is responsible for their constancy.

Lloyd Williams, in a preliminary account (*Ann. Bot.*, October), describes the gametophytes of *Laminaria* and *Chorda*.

There is no appreciable difference in the early stages of the two types of gametophyte, which develop from the pear-shaped zoospores as a germination-tube with a terminal swelling into which one of the two daughter nuclei of the original nucleus passes.

In *Laminaria* the male gametophyte is much smaller than the female, and consists of smaller more numerous cells, any one of which may function as an antheridium producing a single antherozoid. The female gametophyte is often only one-celled, and this functions as an oogonium, producing a single egg which is fertilised after emergence.

The gametophytes of *Chorda*, whilst similar in essentials to those of *Laminaria*, are much larger and irregularly branched.

Morphology and Anatomy.—A study of the floral anatomy of the Urticales carried out by Bechtel (*Amer. Journ. Bot.*, October) has led him to conclude that the flowers are typically zygomorphic and that the bicarpellate condition has been derived by reduction. The latter is indicated by the presence of what are regarded as the vascular strands of suppressed carpels. In *Ulmus* one outer whorl of stamens and one inner perianth whorl are held to have been suppressed, and the basal orthotropous ovule is regarded as derived from a primitively anatropous condition.

The study of *Phaseolus* seedlings with three cotyledons and two primordial leaves by Harris and his colleagues (*Ann. Journ. Bot.*, October) shows that anatomically, as well as morphologically, these are intermediate between typically dimerous and typically trimerous seedlings. In a later paper (November) it is shown that there is a negative correlation in the hypocotyl of trimerous seedlings between the number of double bundles and the number of accessory bundles characterised by the absence of protoxylem elements.

From a study of various species, especially *Vitis vulpina* (New York Agr. Exp. Sta., 1915), Benedict concluded that senescence in the individual was accompanied by a decrease in size of the vein islets of the adult leaf. Ensign, who has recently investigated the point on six species, including *Vitis vulpina* (*Amer. Journ. Bot.*, November), finds no evidence of any such correlation.

In an interesting paper dealing with the Norfolk species of *Utricularia* (*Trans. Norfolk and Norwich Nat. Hist. Soc.*, vol. ix, 1921), Clarke and Gurney state that stomata are apparently absent from the submerged leaves of *U. vulgaris*, but occur regularly on those of *U. intermedia*, and less frequently on the leaves of *Utricularia minor f. platyloba.*

The occurrence of seasonal growth-rings in a monocotyledon

Aloe ferox, is described by Professor Chamberlain (*Bot. Gaz.*, November).

Ecology.—The destruction of Mosses by Lichens is described by McWhorter (*Bot. Gaz.*, November). This is usually due to smothering, but sometimes apparently to actual parasitism, *Amphitoma* being cited as an example.

In an extensive and fascinating paper Erdtman (*Arkiv. für Botanik*, March 17, 1921) describes the occurrence of pollen of various trees in the successive layers of peat in a number of moors and fens of Sweden. Both the statistical results with respect to pollen and the distribution of vegetative organs on the peat seem to indicate a succession from below upwards, through Fen with Sedges, *Menyanthes*, *Cladium*, etc.; Fen Carr; Pine; and ultimately *Sphagnum*.

Although there is some irregularity in the percentage curves for the different species of pollen, most of these seem to indicate an earlier phase in which deciduous trees were more abundant; whilst the recent peat tends to show an increase of *Pinus* and *Picea* corresponding to a diminution or total absence of the deciduous species, especially *Corylus* and others generally associated with less acid conditions.

PLANT PHYSIOLOGY. By PROF. WALTER STILES, M.A., Sc.D., University College, Reading (Plant Physiology Committee).

Osmotic Pressure in Plants.—Considerable additions to our knowledge of osmotic pressure and allied phenomena in plants have been made during the last few years by J. A. Harris and a number of co-workers in America, and by Ursprung and Blum in Switzerland. As is well known, there are two chief methods available for the determination of osmotic pressures in plants—the plasmolytic method, which gives a measurement of the osmotic pressures of individual cells, and the method which consists in expressing the sap from tissues or organs and measuring the freezing-point lowering of the fluid so obtained. The determinations of Harris and his collaborators have been made by the latter method, those of Ursprung and Blum by the former.

With regard to the magnitude of osmotic pressures in plant cells, many observations have indicated that a mean value would be somewhere in the neighbourhood of 10 to 15 atmospheres. While values considerably higher than the latter number have been recorded from time to time, Harris, Gortner, Hofman, and Valentine ("Maximum Values of Osmotic Concentration in Plant-tissue Fluids," *Proc. Soc. Exper. Biol. and Med.*, **18**, 106–109, 1921) have found exceptionally high values in plants growing in the neighbourhood of the Great Salt Lake, the value of 153·1 atmospheres found in the case of the sap of

a plant of *Atriplex confertifolia* being the highest so far recorded for any flowering plant.

Considerable attention has been devoted to the possible relation between osmotic pressure of cells and their position in the plant, this question being especially of importance in regard to the ascent of water in plants. While earlier observations had been rather inconclusive, Harris, Gortner, and Lawrence (" The Relation between the Osmotic Concentration of Leaf Sap and Height of Leaf Insertion in Trees," *Bull. Torrey Bot. Club*, **44**, 267–286, 1917) found, in a series of measurements made on material of twelve species of trees, that almost without exception the osmotic pressure of the sap extracted from leaf cells is greater the higher the level of insertion of the leaf. No such relation between height of leaf insertion and osmotic pressure of the leaf cells was found by Ursprung and Blum (" Über die Verteilung des osmotischen Wertes in der Pflanze," *Ber. deut. bot. Ges.*, **34**, 88–104, 1916). In the species examined by these latter authors, namely, *Helleborus fœtidus*, *Urtica dioica*, *Sedum acre*, *Fagus sylvatica*, and *Funaria hygrometrica*, it was found that although cells of the same layer at the same height from the ground have approximately the same osmotic pressure if they are not too far apart, yet neighbouring cells may differ considerably in their osmotic pressure if they are constituents of different layers. The osmotic pressure of the leaf cells appears to increase with age of the leaf. No particular tissue in the leaf appears to be characterised by the possession of the highest osmotic pressure.

There appears to be a certain amount of correlation between habit and osmotic pressure. Thus Harris, Lawrence, and Gortner (" The Cryoscopic Constants of Expressed Vegetable Saps as related to Local Environmental Conditions in the Arizona Deserts," *Physiol. Res.*, **2**, 1–49, 1916) had found considerable variation in the osmotic pressure of the sap expressed from plants growing in the deserts of Arizona, the different groups, in increasing order of osmotic pressure, being (1) winter annuals, (2) perennial shrubs, (3) dwarf shrubs and half shrubs, and (4) shrubs and trees. In a similar investigation on the plants of the Blue Mountains of Jamaica, Harris and Lawrence (" The Osmotic Concentration of the Tissue Fluids of Jamaican Rain-forest Vegetation," *Amer. Journ. Bot.*, **4**, 268–298, 1917) found that the osmotic concentration of the sap expressed from the leaves of woody plants was greater than that from the leaves of herbaceous plants, while this same result has been found by Harris, Gortner, and Lawrence (" On the Differentiation of the Leaf-tissue Fluids of Ligneous and Herbaceous Plants with Respect to Osmotic Concentration and Electrical Conductivity," *Journ. Gen. Physiol.*, **3**, 343–345, 1921 ; " The Osmotic Con-

centration and Electrical Conductivity of the Tissue Fluids of Ligneous and Herbaceous Plants," *Journ. Phys. Chem.*, **25**, 122–146, 1921) in the case of plants of the Arizona deserts and of the north shore of Long Island.

About twelve years ago it was suggested by MacDougal and Cannon that there might be a fundamental relation between the osmotic pressure of the cells of parasites and that of the cells of the host plants. That this is indeed the case has been shown by Harris and Lawrence (" On the Osmotic Pressure of the Tissue Fluids of Jamaican Loranthaceæ parasitic on various Hosts," *Amer. Journ. Bot.*, **3**, 438–455, 1916) for various Loranthaceæ of montane rain-forest, and by Harris and Valentine (" The Specific Electric Conductivity of the Tissue Fluids of Desert Loranthaceæ," *Proc. Soc. Exper. Biol. Med.*, **18**, 95–97, 1920) for desert species of the same family. Their results indicate that the osmotic concentration of the cells of the parasite is in general higher than that of the leaf cells of the host, the difference being of the order of from 5 to 20 per cent. in the cases examined.

Non-parasitic epiphytes, on the other hand, appear to exhibit unusually low osmotic pressures, to judge from the values obtained by Harris (" On the Osmotic Concentration of the Tissue Fluids of Phanerogamic Epiphytes," *Amer. Journ. Bot.*, **5**, 490–506, 1918). In the epiphytic species examined by him the osmotic pressure of the expressed sap appeared to be from 37 to 60 per cent. lower than that of the sap of herbaceous plants and from 28 to 45 per cent. lower than that of ligneous species.

Bound up with the question discussed in the preceding paragraphs is that of the possible effects of external conditions on the osmotic pressure of plants. The high values found by Harris, Gortner, Hofman, and Valentine for the osmotic pressure of the expressed sap of plants growing in the neighbourhood of the Great Salt Lake, to which reference has been made earlier, is probably connected with the high content of salt in the soil. Differences in the osmotic pressure of the sap of ligneous plants growing in different habitats have been observed by Harris and his co-workers, the average osmotic pressure of the sap of such plants in Jamaican montane rain-forest being 11·44 atmospheres, in Long Island 14·40 atmospheres, and in the deserts of southern Arizona 24·97 atmospheres.

Some direct observations on the influence of external conditions on the osmotic pressure of plant cells have been made by Ursprung and Blum (" Über den Einfluss der Aussenbedingungen auf den osmotischen Wert," *Ber. deut. bot. Ges.*, **34**, 123–142, 1916). From their results, as well as from those of earlier investigators, it appears clear that a higher water-con-

tent of the environment, either soil or atmosphere, induces a lower osmotic concentration of the cell sap. Wind brings about an increase in the osmotic concentration of the sap, no doubt on account of increase of transpiration and consequent removal of water from the plant. Temperature of the environment appears to be not without influence on the osmotic pressure of the cell sap. The osmotic pressure of the cells examined appeared to have a minimum value in the region between 10° and 20° C., increasing with change of temperature in either direction. Such a state of affairs is very difficult to explain satisfactorily, and it does not appear that any effect due simply to temperature has been entirely disentangled from that of other factors. As might be expected, illumination, no doubt on account of the production of carbohydrates, brings about an increase in the osmotic concentration of the cells of green leaves.

As it is clear then that the factors of the environment influence the osmotic pressure of plant cells, it is to be expected that in plants growing in a varying climate there should be a variation in osmotic pressure corresponding to the changes in climate. This has been shown by Ursprung and Blum ("Über die periodischen Schwankungen des osmotischen Wertes," *Ber. deut. bot. Ges.*, **34**, 105–123, 1916) to be the case, both a daily and an annual periodicity being clearly discernible. With regard to the former, the osmotic pressure increases from early morning to the afternoon, and then falls off again until early the next morning; as regards the annual periodicity, minimum values are the rule in the summer and maximum in the winter.

Some contributions to the question of the substances in the cell sap responsible for the osmotic pressure have been made by Harris, Gortner, and Lawrence ("On the Relationship between Freezing-point Lowering, Δ, and Specific Conductivity, K, of Plant Fluids," *Science*, **52**, 494–495, 1920, and other papers cited above). They measured not only the osmotic pressure of the expressed sap, but also the electrical conductivity which should give an approximate measure of the content of the sap in electrolytes, though no more than an approximate measure for a variety of reasons. They found, however, that there was no constant relation between the osmotic pressure and electrical conductivity in different species, so that it appears that the ratio of electrolytic and non-electrolytic osmotically active constituents of the cell sap varies greatly from plant to plant.

In a further series of papers Ursprung and Blum have devoted attention to what they call the "Saugkraft" of the cell ("Zur Methode der Saugkraftmessung," *Ber. deut. bot. Ges.*, **34**, 525–539, 1916; "Zur Kenntnis der Saugkraft," *Ber.*

deut. bot. Ges., **34**, 539–554, 1916). For this quantity the present writer is suggesting the English equivalent of "suction pressure," as by it is meant the net pressure forcing water into the cell, which, when no external forces are operative, is equal to the osmotic pressure of the cell sap less the inwardly directed pressure of the cell wall resulting from its tendency to contract when the cell is turgid. An English writer has recently suggested the term "water-absorbing power of the cell" for this quantity (Thoday, "On Turgescence and the Absorption of Water by the Cells of Plants," *New Phyt.*, **17**, 108–113, 1918), but this loose use of the term "power" for a quantity which is undoubtedly a pressure is to be deprecated.

Ursprung and Blum describe two methods for measuring the suction pressure of cells, the simpler of which, and the one which is likely to come into general use, consists in determining the concentration of sucrose in which the cell neither loses nor gains in volume. The osmotic pressure of this solution is equal to the suction pressure.

In a series of determinations of the suction pressures of cells in the beech, Ursprung and Blum obtained definite evidence of a gradient in the suction pressure in proceeding from lower to higher levels. Later their observations were extended to other plants ("Zur Kenntnis der Saugkraft, II.," *Ber. deut. bot. Ges.*, **36**, 577–599, 1918), as, for example, the ivy, in which they showed that the suction pressure increases with the distance from the absorbing zone of the root in any particular tissue, while in a cross section through any particular organ the suction pressure increases with distance from the water-conducting tissue. These facts are very significant from the point of view of the translocation of water through plants. The only exception to the rule was found in the absorbing region of the root, where the suction pressure is greater in the cortex than in the piliferous layer; this is to be expected.

In their latest paper ("Zur Kenntnis der Saugkraft, IV., Die Absorptionszone der Wurzel. Der Endodermissprung," *Ber. deut. bot. Ges.*, **39**, 70–79, 1921) the same writers discuss in particular the absorbing zone of the root. They find in this organ in the broad and runner bean that the suction pressure increases from the piliferous layer inwards as far as the layer of the cortex next within the endodermis, but that the suction pressure falls very considerably in the latter layer itself, the pressure of the cells within the endodermis being even less. This is very difficult to explain, the explanation offered by Ursprung and Blum being that the suction pressure varies at different parts of the surface of the same endodermal cell, so that it is high on the side towards the cortex, but low on the side towards the pericycle. In this way the endodermis acts

as a sort of valve by means of which the passage of water into the central cylinder is enabled to take place in spite of the sudden drop in the *mean* suction pressure in passing from the cortex to the endodermal layer.

PALÆOBOTANY IN 1921. By MARIE CARMICHAEL STOPES, D.Sc., Ph.D., F.L.S., Fellow of University College, London.

An interesting and attractive *Life* of the greatest palæobotanist of his day, Prof. A. G. Nathorst, appeared in April 1921 in Swedish (by T. G. Halle, *Geol. Fören. Förhandl.*, pp. 241–80, illustr.), with a number of portraits of Nathorst at different ages. Were there such a body of persons as there should be in this country taking a serious interest in Palæobotany, the pamphlet should be translated into English. The complete list of Nathorst's publications which follows (same journal, pp. 281–311) accentuates the exceptional width and range of his contributions to various branches of learning. An appreciation also appeared in the *Botanical Gazette* for June 1921, pp. 462–5, by Seward.

In *General Palæobotany* the year's most interesting paper is the reprint (Sect. K., *Brit. Assoc. Sci., Edin.*, 1920, pp. 1–17) of the Presidential address on "The Present Position of the Theory of Descent in Relation to the Early History of Plants" to the Botanical Section, by Dr. D. H. Scott. Here he links up and discusses the more recent results of palæobotany and genetics, and makes many suggestive deductions. One is glad to see the statement: "The evidence from the older Devonian flora, so far as it goes, materially supports the opinion that the seed plants cannot have arisen from Ferns, for the line of the Spermophyta seems to have been already distinct at a time when true ferns had not yet appeared." After indicating many lines of recent development in Palæobotany, Dr. Scott concludes justly that: "We have indeed a wealth of accumulated facts, but from the point of view of the Theory of Descent they raise more questions than they solve."

Palæobotany as Viewed by two Geologists (by G. R. Wieland, *Science*, vol. liii, pp. 437–9), is based on brief references to the recent papers of Professors A. C. Coleman and C. Schuchert, and the bearing of fossil plant records on climate. Microscopic work on the stomates and cuticles of various plants, chiefly *Thinnfeldia rhomboidalis*, is described by R. Potonié ("Mitteilungen über mazerierte kohlige Pflanzenfossilen," *Zeits. Bot.*, vol. xiii, pp. 79–89, 12 figs.). The return of the name Potonié in records of active palæobotanical work is of special interest, as the present Robert Potonié is the young son of the famous Berlin professor who died some years ago.

Memoirs on Special Floras this year include two further handsome quartos on Devonian plants by R. Kidston and W. H. Lang, continuing their series on the Rhynie Chert, viz. part 4, "Restorations of the Vascular Cryptogams"; and part 5, "The Thallophyta occurring in the Peat-bed," *Trans. Roy. Soc. Edin.*, vol. lii, pt. 4, Nos. 32 & 33, pp. 831–54, and 5 pls., and pp. 856–902, and 10 pls. The first of these papers will be widely consulted, as it gives clear and explicit restorations of a life-like character of all four of the forms, and discusses the morphological significance of their very primitive structure. The second contains full descriptions of many well-preserved hyphal fungi; these are numbered, and to some of them definite (new) specific names are given. It is concluded that the majority are best described as belonging to the non-septate genus *Palæomyces*. The micro-photographs illustrating these forms are of notable quality and beauty. The concluding division of the memoir consists of a careful description of the horizontal sequence of the plants in the chert beds and the conditions of deposition of the plant material and the penetrating silica. It is concluded that the plants represent a true *land* vegetation, and the infiltrating mineral was possibly supplied by volcanic fumaroles.

Among studies of the carboniferous floras should come first that of H. Yabe and S. Endô, "Discovery of Stems of a Calamites from the Palæozoic of Japan" (*Sci. Rep. Tohoku Imper. Univ.*, Ser. 2, vol. v, No. 3, pp. 93–6, 1 pl.), because though the paper is short, it contains an account of a unique specimen, viz. the only Upper Palæozoic species known from its anatomical structure from Japan: the only other Japanese carboniferous plant being a very doubtful impression. Although many coal-measure plants are known from China, none other is recorded from Japan, where the deposits appear to have been entirely marine and without any coal-beds. The specimen is not well preserved, but seems to be undoubtedly a *Calamites* of the *Arthropitys* type.

From Belgium come once more further contributions to the detailed consideration of the coal-bearing strata of that country by A. Renier, "Les Gisements Houillers de la Belgique," *Ann. des Mines Belg.*, 1921, vol. xxii, 1re & 2me liv. A contribution of considerable novelty and value is found in D. Davies's "Ecology of the Westphalian and the Lower Part of the Staffordian series of Clydach Vale and Gilfach Goch" (*Quart. Journ. Geol. Soc.*, vol. lxxvii, pt. 1, pp. 30–74, pl. ii. The work is based on over 45,000 field records and many thousands of collected specimens, and gives a thoroughly detailed account of the conditions at the time of deposition and the plants contributing to the accumulations. Once such careful records

are prepared for all standard horizons, real comparative palæobotany can begin on its true task of analytically examining the progress of evolution and dispersal of species. *E. M. Round* reports and figures *Odontopteris genuina* in Rhode Island (*Bot. Gaz.*, vol. lxxii, pp. 397–403).

Special work on Coal has been followed out in this country by chemists investigating further the palæobotanical zones in coal described by Stopes in 1919. See R. Lessing (" The Mineral Constituents of Banded Bituminous Coal," *Trans. Chem. Soc.*, vol. cxvii, pp. 256–65; and " Studies in the Distribution of Mineral Matter in Coal," *Trans. Inst. Mining Engin.*, vol. lx, pt. 3, pp. 288–309, and vol. lxi, pt. 1, pp. 36–41), and F. S. Sinnatt (" The Constitution of Coal," *Trans. Inst. Mining Engin.*, vol. lxii), and others who are working on the detailed structure of Fusain, Durain, Clarain, and Vitrain. Work on Brown Coal has appeared in Germany by W. Gothan (" Neue Arten der Braunkohlenuntersuchung," in *Braunkohle*, 1921, No. 27), and both by B. Kubart and R. Kräusel under the same title (" Ist *Taxodium distichum* oder *Sequoia sempervirens* Charakterbaum der deutschen Braunkohle ? " *Ber. deuts. Bot. Gesell. Jahrg.*, 1921, vol. xxxix), depending largely on microscopically identified tissue-structure. It is evident that palæobotanical work and results will form the basis for advance in almost all aspects of coal research.

The Mesozoic floras of New South Wales were further dealt with by A. B. Walkom in his memoir, " Fossil Plants from Cockabutta Mountain and Talbragar " (*Mem. Geol. Surv., N.S. Wales*, No. 12, pp. 1–21, pls. i–vi). Eleven species of Ferns and Gymnosperms are there described, including some new species of *Thinnfeldia*, and the age of the beds is determined as being Jurassic. G. Erdtman described a new species of *Equisetites* from Sweden and *Neocalamites Nathorstii* n. sp. from Yorkshire, in *Arkiv. Bot.*, vol. xvii, No. 3; and in the same journal, No. 1, T. G. Halle described and figured some beautifully preserved sporangia of Mesozoic Ferns, particularly of *Danæopsis fecunda* n. sp. W. N. Edwards described a new Wealden fructification in his paper " On a small Bennettitalean Flower from the Wealden of Sussex " (with a " Note on *Parka decipiens*" in the *Ann. Mag. Nat. Hist.*, vol. vii, ser. 9, pp. 440–4, pl. xii), obtaining masses of spore-remains, which indicate a *Williamsoniella* affinity. " The Missing Link in Osmundites " was described by M. C. Stopes (*Annals Bot.*, vol. xxxv, No. 137, pp. 55–61, pl. ii, fig.), from a small petrified specimen with a segment of the main axis and surrounding leaf bases. The latter were normal Osmundaceous meristeles, but the main axis had a solid central protostele with a beginning of secondary thickening, features of theoretical interest in the series of

Osmundaceous and Botryopteridean forms. In his paper on "Monocarpy and Pseudo-monocarpy in the Cycadeoids" (*Amer. Journ. Bot.*, vol. lxxxi, pp. 218–30, pls. ix–xii), G. R. Wieland discussed Monocarpy in a few recent examples, and developed the interest of the character in relation to the Cycadeoidea, with their immensely numerous and complex fructifications.

From Tertiary beds C. Reid and J. Groves described a large number of beautifully preserved Chara fruits, and suggested that the group might supply the zonal types which are so much wanted for fresh-water strata (see "The Charophyta of the Lower Headon Beds of Hordle Cliff," *Quart. Journ. Geol. Soc.*, vol. lxxvii, pt. 3, pp. 175–92, pls. iv–vi. Probably of Tertiary age were the numbers of specimens brought back from Christmas Harbour in 1840 and now in the British Museum; detailed examination of their structure, however, has only revealed two species (see W. N. Edwards on "Fossil Coniferous Wood from Kerguelen Island," *Annals Bot.*, vol. xxxv, No. 109, pp. 609–617, pl. xxiii); namely, *Cupressinoxylon Antarcticum* Beust and *Dadoxylon kerguelense* Seward.

In general one must report that the year 1921 marks a low ebb of the tide of palæobotanical production.

ENTOMOLOGY. By A. D. Imms, M.A., D.Sc., Institute of Plant Pathology, Rothamsted Experimental Station, Harpenden.

Since the last report on Entomology (Science Progress, October 1921) the following are among the more important contributions that have come to notice.

General Entomology.—R. A. Muttkowski (*Ann. Ent. Soc. Am.*, **14**, 150–56) has published the first paper of a series of studies on respiration in insects. As at present understood, atmospheric oxygen is led directly to the tissues by the tracheæ, while the blood acts as the carrier of food and metabolic products. Some workers, however, have maintained that the blood is not concerned with the transportation of oxygen at all. This latter statement has proved difficult to reconcile with facts, more especially in insects that are devoid of tracheæ. Muttkowski finds that both O and CO_2 are present in insect blood, the major portion of these gases being held by a respiratory protein, and the species examined show the presence of copper, which is interpreted as forming the basis of a respiratory pigment-hæmocyanin. The only exception is found in certain Chironomid larvæ where the respiratory pigment has long been known to be hæmoglobin. In addition to its recognised function of circulating food and metabolic products, the blood, therefore, aids the tracheal system in the distribution of O and the removal of CO_2.

G. H. D. Carpenter (*Trans. Ent. Soc.*, 1921, 1–105) records a number of experiments on the relative edibility of insects, using a single monkey (*Cercopithecus*) as the judge of the question. As the result of nearly 1,000 experiments, the behaviour of this monkey has led the author to the conclusion that conspicuous insects are definitely distasteful, and make the most of their conspicuousness to advertise the fact. Highly edible species endeavour to elude their enemies by concealment. Edibility and distastefulness, it is pointed out, are not absolute but relative qualities, and a hungry monkey will eat an insect which he would pass over when not very hungry. G. C. Crampton has published a further series of papers on general insect morphology. In *Journ. New York Ent. Soc.* (**29**, 63–100) he discusses the phylogenetic origin of the mandibles of Arthropods, and points out, as others have done before him, how close a similarity is exhibited in the mandibles of the Machilidæ with those of certain of the higher Crustacea. In a larger contribution (*Ann. Ent. Soc. Am.*, **14**, 65–103) he enters into a detailed morphological discussion of the head-sclerites and mouth-parts of various orders of insects. It is too elaborate for a brief abstract in these pages, but is a paper which concerns all morphologists. Reference has already been made in my previous article on " Recent Advances " to this author's preliminary announcement of his rejection of the view that the maxillulæ of insects are a true pair of head appendages. In *Psyche*, **28**, 84–92, he has stated his views in greater detail and concludes that the work of Folsom is not to be relied upon, mainly, it appears, for the reason that it is not in accordance with the findings of another observer—Philiptschenko, who was unable to find appendicular rudiments of maxillulæ in the embryos of an allied insect. As the matter stands at present, it is scarcely possible to say whether it is Folsom or Philiptschenko that has committed an error of observation. It must also be pointed out that, in certain of the Thysanura, each maxillula exhibits an *apparent* differentiation into galea, lacinia, and palpus. This differentiation is not regarded by Crampton as a real one ; nevertheless, he does not completely succeed in explaining it away. H. Onslow (*Phil. Trans.*, **211**, 1–74) has a valuable contribution on the causes of the irridescent colours in various insects and on a periodic structure in the scales. Irridescence is found to depend very largely upon interference and not upon diffraction. In the case of Lepidoptera, the interference theory is greatly strengthened by the discovery of a regular periodic structure, of the correct magnitude, in certain irridescent scales. The elytra of nearly all scaleless irridescent Coleoptera form a group whose colour appears to be due to selective metallic reflection.

The recent text-book of G. H. Carpenter, *Insect Transformation* (London, 1921, pp. xi + 282, 4 pls.), is a useful and clearly written discussion of metamorphosis in insects.

Hymenoptera.—M. Haviland (*Q.J.M.S.*, **65**, 451–78) contributes a careful piece of research work on the biology of certain Cynipidæ, those of the genus *Charips* (*Allotria*) in particular. They are parasites of *Aphidius*, and, through the latter insects, hyperparasites of aphides. The newly hatched larva of the Cynipid is of a remarkable form, it being armed with darkly coloured segmental chitinous plates, and terminated by a caudal prolongation of the body. In the second instar the chitinous plates are lost, although the "tail" is still evident, while the fully grown larva is of the usual maggot-like hymenopterous type, but has only six pairs of open spiracles. The heavily armoured first larva is difficult to explain in an endoparasite. It closely resembles the active, roaming, planidium which occurs as the primary larva in certain other parasitic Hymenoptera, and its retention in the Cynipid may, perhaps, be a case of the survival of a former life-cycle not unlike that of *Perilampus*. C. Morley (*Entom.*, **55**, 1–3) contributes the first of a series of articles on the British Proctotrypidæ (Oxyura), which have, so far, been greatly neglected by entomologists in this country. C. H. Mortimer (*Ent. Month. Mag.*, **58**, 16–17) records the occurrence of males of *Bombus sylvarum* var. *nigrescens* (Perez) from the Newhaven district in August last. This form has not previously been recorded in the British Isles, and the record is of interest from the faunistic standpoint. In the same journal (p. 19) this author also records the very rare humble bee, *Bombus cullumanus* (Kirby), from Sussex. Under the title of *More Hunting Wasps*, the late A. de Mattos (London, n.d.) has brought together a series of translations of Fabre's remarkable observations on the solitary Vespidæ. H. Hacker (*Mem. Queensland Mus.*, vii., pt. 3) catalogues the bees of Australia. The fauna is evidently very rich in these insects, for no less than 50 genera and 872 species are listed. The primitive bees of the family Prosopidæ are the best represented, while there are no Bombidæ and only one species of Nomadidæ.

Diptera.—R. Frey (*Acta. Soc. Fauna et Flora Fennica*, **48** [3], 245 pp.) has written an important and comprehensive treatise on the structure of the mouth-parts in the lower Diptera Schizophora. These organs have been examined in ninety genera of flies, representative of forty-four groups, or families, although it is doubtful whether all merit the latter rank. The work has been carried out by means of potash preparations and stained sections. It is accompanied by a general discussion of the function and homologies of the mouth-parts, and there are

tables of the families based upon characters afforded by the latter. Appended to it are ten plates, containing 126 figs. and a very full bibliography. C. G. Lamb (*Proc. Camb. Phil. Soc.*, xx [3], 293–7) describes some venational aberrations and comments upon their rarity in Diptera. The same author (*ibid.* [4], 475–7) describes an unusual type of male secondary sexual characters in a new genus of Dolichopodidæ from the Seychelles. The male has shorter and differently shaped wings from the female, and also possesses a pair of remarkable spheroidal bodies, at the base of the abdomen on its dorsal surface. The function of these organs is highly problematical. The ovary and ovarian eggs of *Anopheles maculipennis* form the subject of a detailed histological study by A. J. Nicholson (*Q.J.M.S.*, **65**, 395–448), and in the same journal (611–23) D. Keilin writes on the presence of $CaCO_3$ in the Malpighian tubes, or in cells connected with the fat-body, in many dipterous larvæ. During the pupal stage the $CaCO_3$ completely dissolves in the perivisceral fluid, and then passes through the newly formed pupal cuticle into the ecdysial fluid. When the latter is absorbed the $CaCO_3$ remains as a deposit upon the inner surface of the puparium. It is evidently an excretory process, and the $CaCO_3$ may possibly be derived from the neutralisation of the CO_2 of respiration. H. M. Morris (*Bull. Ent. Res.*, **12** [3], 221–32) gives a useful description of the larval and pupal stages of certain of the Bibionidæ, with particular reference to *Bibio marci* L. He points out that the larvæ of different, although often closely allied, species may be readily separated by the form and structure of the cuticular spines and nodules distributed over the general surface of the body. In the same journal (263–362) F. W. Edwards contributes an important revisionary paper on the mosquitoes of the Palæarctic region. It is based upon not only adult characters, but also on those of the larvæ wherever known. Metcalf (*Ann. Ent. Soc. Am.*, **14** [3], 169–214), discusses the morphology and taxonomic significance of the male genitalia in Syrphidæ, and his paper is accompanied by many illustrations. E. W. Laake (*Journ. Agric. Res.*, **21** [7], 439–57) has a paper on the larval stages of the Ox-Warble flies, *Hypoderma bovis* and *H. lineata*. He finds that good distinguishing characters are afforded by the structure of the posterior spiracles and other parts. It is, therefore, possible to determine with certainty which of these two species is infecting a given animal. In addition to the four instars already known, he finds that an additional one is passed through and occurs in the œsophagus.

Hemiptera.—R. J. Tillyard (*Proc. Linn. Soc., N.S.W.*, **46**, 270–87) establishes a new family—the Mesogereonidæ—for a large Cicada-like insect, examples of whose wings have

been found in the Mesozoic rocks of Queensland. L. Lloyd (*Bull. Ent. Res.*, **12** [3], 355–9) brings to notice one of the most clearly proved cases of colour response yet discovered in any insect. By means of critical experiments he was able to show that the Greenhouse White Fly (*Asterochiton vaporariorum*) is powerfully attracted to a yellow colour, and that literally enormous numbers of that species can be caught by shaking infested plants near a yellow screen, coated with a transparent adhesive substance. Uichanco (*Psyche*, **28**, 95–109) contributes a critical paper on our present knowledge of the influence of environmental factors on reproduction in aphides. This same question is dealt with more fully by Davidson (*Sci. Proc. Roy. Dublin Soc.*, **16**, n.s., 304–22), who shows that cytological investigations on aphides and breeding experiments render it highly probable that the sequence of winged and apterous individuals is largely due to inherent (internal) factors. Similar investigations show that the appearance of sexual forms is associated with changes in the chromosome complex. F. Muir (*Psyche*, 116–119), criticises some recent work by Crampton, and discusses the phylogeny of the Homoptera.

Lepidoptera.—The question of the occurrence of any indigenous species of butterfly in Iceland has been for a long time under discussion. J. J. Walker (*Ent. Month. Mag.*, 58, 1–7) contributes some observations on this topic, and considers that there are no true native species in that island, but admits the possibility that such insects, if present, may perhaps turn up in one or other of its sequestered valleys. The individuals of *Vanessa cardui* and *V. atalanta* that have been taken are to be looked upon as stray examples of species well known to possess strong migratory instincts. L. Bordas (*Ann. Sci. Nat. Zool.*, **8**, 175–250) contributes an important treatise on the structure and histology of the digestive system in adult Lepidoptera. Students of genetics will be interested in two papers by Onslow (*Journ. Genetics*, **11**, 3), on the inheritance of colour and pattern in *Diaphora mendica* and its var. *rustica*, and in *Hemerophila abruptaria* and the var. *furcata*. J. H. Gerould (*Journ. Exp. Zool.*, **34** [3], 385–412) describes a blue-green mutation in the hæmolymph colour of caterpillars of *Colias philodice*. This colour is a mendelian recessive, the normal grass-green colour being dominant. The adults from two blue-green larvæ breed true. The author concludes that the hereditary nuclear enzyme (or recessive gene) involved in the case of blue-green larvæ is a decoloriser (inhibitor) of the yellow pigment. The eye colour of the adult is affected by the mutation probably through the action of the blood. It is interesting to note that parasites (*Apanteles*) emerging from grass-green larvæ spin bright golden-yellow cocoons, while those emerging from blue-green cater-

pillars spin white cocoons. Presumably the yellow-inhibiting enzyme in the blood of the blue-green host changes the colour of the secretion of the silk glands of the parasite from yellow to white.

Coleoptera.—Scott (*Bull. Ent. Res.*, **12** [2], 133–4) records a Ptinid beetle, *Trigonogenius globulum* Sol. breeding in argol, which is the deposit that separates out in barrels of new wine, and contains a high percentage of potassium bitartrate. A. W. R. Roberts (*Ann. App. Biol.*, **8** [3 and 4], 193–215) has a second instalment of his work on wireworms of the genus *Agriotes.* He deals mainly with *Agriotes obscurus*, and with more especial reference to the morphology of the mouth-parts and spiracles. A. G. Boving (*Proc. Ent. Soc. Washington*, **23** [3], 51–62) describes the larva of the Scarabæid beetle, *Popilla japonica*, in great detail, and provides a useful key to the identification of the larvæ of different genera in the subfamily Rutelinæ. The same author, in collaboration with J. H. Wade (*Journ. Agric. Res.*, **22** [6], 323–34), deals with the biology and structure of the larva and pupa of the Tenebrionid beetle, *Embaphion muricatum.* The great care and detail with which these descriptions are drawn up will serve as a model for future work of this kind. A. d'Orchymont (*Ann. Soc. Ent. Fr.*, **89**, 1–50) discusses the wing-venation in Coleoptera.

Orthoptera.—F. Carpenter (*Ann. Soc. Ent. Belg.*, **61**, 337–43) has a contribution on the structure of the thorax in Orthoptera. Ubarov (*Bull. Ent. Res.*, **12** [2], 135–63) contributes an important article on the genus *Locusta*, and concludes that, owing to their great variability, only two species can be distinguished, viz. *L. migratoria* L. and *L. pardalina* Walk. The latter he regards as being sufficiently different as to require a new genus, *Locustana*, for its reception. *L. migratorioides* and *L. danica* are phases of *L. migratoria*, and all three are connected by transitional forms, and are even able to undergo a transformation into each other. *L. migratorioides* and *L. migratoria* are swarming phases and *L. danica* is a solitary one. In the same way *L. padalina* is the swarming destructive phase of that species, and its form *solitaria* is the solitary harmless phase. The causes of migration of larval and adult swarms of *migratoria* are discussed, and the conclusion is arrived at that lack of food has nothing to do with larval swarms, which are chiefly guided by the instinct of gregariousness and thermotropism. The flying swarms also are not driven to wander by hunger, and do not feed much during migration.

Protura.—This primitive order of insects was discovered by Silvestri in 1907, and has been largely overlooked on account of the small size of its members. The most recent contribution towards a knowledge of these forms is by H. E. Ewing (*Proc.*

Ent. Soc. Washington, **23**, 193–202), who describes twelve new species and three new genera, all of which were collected from a deposit of dead leaves in Maryland on a single occasion.

Applied Entomology.—There is no falling off in the number of new text-books that continue to appear and deal with various aspects of the subject. H. T. Fernald's *Applied Entomology* (New York, 386 pp.) is an all-round first-rate elementary book. B. M. Underhill's *Parasites and Parasitosis of Domestic Animals* (New York) devotes 70 pages out of 379 to insects. D. Rivas has brought out a comprehensive work, entitled *Human Parasitology* (Philadelphia and London, 715 pp.), and the insects concerned are dealt with in about 100 pages. F. W. Dry (*Bull. Ent. Res.*, **12** [3], 233–8) contributes a note on a trypanosomiasis in cattle, possibly transmitted by some insect other than the Tsetse, since the latter was absent from the district in the Kenya Colony of Africa under observation. In a second note he calls attention to an obscure human disease in the same country, which has a distribution showing a coincidence with that of a small blood-sucking fly, *Simulium neavei* Roub. W. S. Patton (*ibid.*, 239–61) writes on dipterous larvæ which produce myiasis in man and domestic animals. By means of characters afforded by the cephalo-pharyngeal skeleton and posterior spiracles, a table is provided for the identification of the myiasis-producing larvæ, other than those of the Œstridæ. The Imperial Bureau of Entomology have recently issued a brochure of 65 pages entitled, *An Abstract of the Legislation in force in the British Empire dealing with Plant and Diseases up to the Year* 1920. R. J. Tillyard (*New Zealand Journ. of Agric.*, **23** [1], 1–15) describes the introduction of a minute Chalcid, *Aphelinus mali*, from the United States into New Zealand, for the purpose of controlling the Woolly Aphis. The parasite is now regarded as successfully introduced, and its economic value in reducing the number of its host is a matter which the future can alone decide. K. M. Smith (*Fruit Grower*, December 15 and 22, 1921) contributes a very useful and well-illustrated article on the biology and control of the Carrot Fly. It is remarkable that so prevalent a species should have hitherto received so little investigation. The introduction of the European Corn Borer into North America is attracting the attention of the entomologists of that continent. In a recent conference (*Journ. Econ. Entom.*, **14**, 453–5) the matter was fully discussed and the seriousness of the problem emphasised. The spread of the insect constitutes a menace to the agriculture of North America, and its recent establishment over large areas renders extermination wellnigh impossible.

ANTHROPOLOGY. By A. G. Thacker, A.R.C.S.

The "Broken Hill Skull" has now taken its place as one of the dozen most important human crania thus far discovered. As is well known, the skull and other human remains were found last summer by Mr. W. E. Barren in a cave in the Broken Hill Mine, in North-Western Rhodesia, and the relics have been brought to England and confided to the care of the British Museum. A preliminary account of the skull was contributed to *Nature*, November 17, by Dr. A. Smith Woodward, and a few days later it was exhibited to the Zoological Society; but at the time of writing it has not been subjected to a minute anatomical examination. Dr. Smith Woodward has, however, had no hesitation in constituting the skull as the type of a new species of the Hominidæ, which he has designated *Homo rhodesiensis*. Dr. Woodward says that "the length of the skull from the middle of the glabella to the inion is about 210 mm., while its maximum width at the parietal bosses is 145 mm. The skull is, therefore, dolichocephalic, with a cephalic index of 69. Its greatest height (measured from the basion to the bregma) is 131 mm. In general shape the brain-case is much more ordinarily human than that of the La Chapelle Neandertal skull, which differs in the expansion and bun-shaped depression of its hinder region. The mastoid process, though human, is comparatively small. The supra-mastoid ridge is very prominent and broad. The tympanic meatus is short and broad, as always in man. The foramen magnum occupies its normal forward position, so that the skull would be perfectly poised on an erect trunk."

The skull is very platycephalic, but, apart from this, its most striking characteristic is the association of a large and human brain-case with a monstrously simian face. It was at first alleged by some writers to represent a type intermediate between the Javan *Pithecanthropus* and Neandertal man, but this was evidently a hasty judgment, and the supposed resemblance to *Pithecanthropus* is clearly illusory. The superficial likeness to the La Chapelle-aux-Saints skull of *Homo neandertalensis* is, however, striking. This is chiefly due to the enormous brow-ridges, the receding forehead, and the large palate; but the likeness may not, and probably does not, extend to details. The teeth are large, but quite human. An extraordinary feature is that the third molars are much smaller than the second molars, the length of the third molars being 9·5 mm. and that of the second molars being 13·5 mm. This is a form of degeneracy which is not even found in the early representatives of the surviving species of man. The teeth are also affected with caries, which is another somewhat sur-

prising feature in a primitive skull. The canines, unlike those of the Piltdown skull, are not proportionately large. The lower jaw was not found. One of the most important characters is that mentioned by Dr. Woodward in the quotation given above, namely, the forward position—what seems to us the normal position—of the foramen magnum. This is a most important difference from *Homo neandertalensis*, and an equally important resemblance to *Homo sapiens*. Parts of a femur and a tibia were also found, and these relics are said to give confirmatory evidence to the view that the individual walked erect ; but there is necessarily some doubt as to whether these bones belonged to the same skeleton, for there were masses of mammalian bones in the same cave, and, in addition to the fragments already mentioned, part of a human sacrum, and part of another upper jaw were unearthed.

The skull was found at about 80 feet below the surface, but this depth has little or no definite significance, since it is more than likely that the skull had fallen down a crevice, near the bottom of which it lay. The great accumulation of mammalian bones was obviously due, in the main, to the action of man ; and in the heaps a few scattered stone implements, resembling those of Bushmen, were found. The mammalian bones are those of Recent species, most of them actually still living in Rhodesia.

The human skull is extremely well preserved, and most fresh in appearance. This is, indeed, the first thing which strikes the observer. It is misleading to speak of it as a "fossil skull." It is sub-fossil. The suggestion was, indeed, made in some quarters that it was Pliocene, but it is difficult to understand on what grounds. It seems to me unlikely that it is even as old as the end of the Pleistocene.

It is, of course, impossible to give a final opinion on the skull until we have more minute details, relating to such matters as the pulp-cavities of the teeth, the inner markings of the brain-case, and so forth. Dr. Woodward is, however, clearly justified in making it the type of a new species. But it seems to me to be indicated that the nearest known allies of the new *H. rhodesiensis* are to be found, not in any of the extinct species of mankind, but among the lowest and most platycephalic races of the surviving species.

The chief interest of the specimen is perhaps to be found in the fact that it is almost certainly of quite recent date. We are thus faced with the probability that only a few thousand years ago a species of mankind other than our own was living in South Africa—long after Neandertal Man had become extinct in Europe. In this connection it is interesting to record that the Bantu is a recent arrival in Southern Africa—he

reached there, in fact, after the white man. And even the Hottentots, who are believed to be a cross between Negroes and Bushmen, are not ancient. The real "aboriginals" of Southern Africa are the Bushmen. And it is therefore not difficult to imagine that if these weak and lowly Bushmen were, three or four thousand years ago, the sole representatives of *H. sapiens* in Southern Africa, a remnant of this strange humanoid species might well have been able to maintain their existence in the recesses of the jungle.

MEDICINE. By R. M. Wilson, M.B., Ch.B.

By far the most important event in the past quarter has been the influenza epidemic. This epidemic, though widespread, belongs undoubtedly to the so-called "minor" type of visitations. At no period was the scale of the great outbreak of 1918–1919 reached, or even approached.

It is worthy of note that Brownlee, in 1919, published in *The Lancet* a paper on the periodicity of influenza, in which he suggested that minor recurrences take place at intervals of thirty-three weeks. Those recurrences, however, which fall due in summer or autumn do not materialise, and, consequently, intervals of about three years separate the epidemics. Brownlee, as a result of his study, foretold the present epidemic, saying that it might be expected in February of 1922.

This is the more remarkable when we remember that the prophecy was made three years ago. It points to a definite advance in the study of epidemiology, and opens up a wide vista of future work; for if it should be found possible to foresee a visitation such as that we have just endured, it may, later on, be possible to determine the nature of the factors producing the recurrence.

If we except this one gleam of light, the darkness which surrounds influenza and all its protean manifestations is unrelieved. No conclusive evidence has been forthcoming in regard to the causative organism. There are still those who believe in the Pfeiffer bacillus. There are others who think that a filter-passer is the first cause, and that its presence prepares the way for the secondary infections which are so common. In the absence of proof one way or the other, judgment must needs be suspended.

The outbreak began in November, and was said to have been first observed in Nottinghamshire, whence it spread, according to the Ministry of Health, to towns in the south of the West Riding, where Leeds, Sheffield, and Rotherham were principally affected. It spread also westward to the Potteries. The disease did not become prevalent in London until the middle

of December, though a few cases were observed at an earlier period. The northern, southern, and eastern districts of the Metropolis were chiefly affected. In January the disease moved to the west and also to the north, so that its maximum intensity in Wales, Northumberland, and Scotland followed that in London by about two weeks.

How rapid was the progress of the wave may be gathered from the following figures, which refer to deaths in the London area.

Week ending:

Dec. 3.	Dec. 10.	Dec. 17.	Dec. 24.	Dec. 31.	Jan. 7.	Jan. 14.
29	14	43	54	151	353	551

The last figure marked the height of the wave. During the period under review the number of deaths from bronchitis and broncho-pneumonia increased rapidly, but the cases of lobar-pneumonia were comparatively few.

The epidemic was not confined to this country. It appeared also in Scandinavia, Germany, France, and Italy. It was later reported in America. The symptoms in all these areas appear to have been similar: headache, pain in the back and legs, and congestion of the throat with some bronchial catarrh and an irritating and very persistent cough. The gastric form was frequent, and in some cases skin rashes of various kinds were met with. As is usual, most of the deaths were due to complications of one kind or another. In the epidemic of 1918–19 the young adult was the chief victim. On this occasion, however, the victims were mostly very young children and old persons. No satisfactory explanation of this phenomenon has been offered.

The Annual Report of the Medical Research Council for 1920–21 contains an account of an interesting experimental study of epidemiology being carried out at Charing Cross Hospital by Dr. Topley. He has introduced into a relatively large population of mice a virus affecting these animals and tending to assume epidemic strength. The conclusions already reached are that:

1. An epidemic among a population which is increasing at a constant rate always progresses in a series of waves, indicating some periodic fluctuation of the conditions upon which the spread of infection depends.

2. The survival of susceptible individuals coming among the infected population has been found to vary widely in relation to the phase of the epidemic during which they are first exposed to the risk of infection.

3. An infected population succumbs far more rapidly to a severe epidemic if fresh susceptible individuals gain access to it in considerable numbers than if it remains isolated.

It is anticipated that these findings may be applied to the solution of disease problems in the cattle industry at an early date. Another interesting piece of work which is discussed in the report is that of Haldane and Collis on rock-dust in relation to tuberculosis. The work of Gye in this connection appears to leave little doubt that silica plays a very big part in the incidence of tubercle, for he found that if silica was added to injections of tubercle bacilli given to insusceptible animals acute tuberculosis often resulted. The work of Holford Ross on printers' phthisis deserves mention in this connection. He has recently shown that dust is carried in printing offices on the fine particles of cotton fluff which are to be found in the air.

ARTICLES

THE PROBLEM OF THE RHODESIAN FOSSIL MAN

By A. SMITH WOODWARD, LL.D., F.R.S.

COMPARED with the discoveries in caves of the temperate regions, those in the corresponding shelters of the tropics have hitherto proved disappointing. In Western Europe we find remains of extinct races of primitive men associated with the skeletons of mammals which are either extinct or no longer living in the same country. In the tropics nearly all the caves hitherto explored have yielded evidence only of existing races of men associated with remains of animals which are identical—or nearly so—with those still living in the same district. An important exception to this general rule was reported in 1920 by Prof. Eugène Dubois, when he described his discovery of skulls and other remains of a primitive Australoid race in rock-shelters at Wadjak in Java.[1] A still more important exception was the finding last autumn of a primitive human skull, with other human remains, in a cave at Broken Hill in Northern Rhodesia.

Until last year the Rhodesian cave had proved as disappointing as most of the others in the tropics. It was first discovered in 1907, during mining operations, by the Rhodesian Broken Hill Development Co.,[2] and since that time enormous numbers of more or less fragmentary bones have been removed from it. At first, these remains were carefully preserved and submitted for examination to Dr. C. W. Andrews, of the British Museum, and to Mr. E. C. Chubb, of the Bulawayo Museum. So far as they could be named, however, they all appeared to belong to species still living in Rhodesia, or to others only slightly different from these.[3] They therefore excited little

[1] E. Dubois, "The Proto-Australian Fossil Man of Wadjak, Java," *Proc. Roy. Acad. Sci. Amsterdam*, vol. xxiii (1921), pp. 1013–1051, with 2 plates.

[2] Franklin White, "Notes on a Cave containing Fossilised Bones, etc., at Broken Hill, North-Western Rhodesia," *Proc. Rhodesia Sci. Assoc.*, vol. vii (1908), pp. 13–23.

[3] F. P. Mennell and E. C. Chubb, "On an African Occurrence of Fossil Mammalia associated with Stone Implements," *Geol. Mag.* [5], vol. iv (1907), pp. 443–8.

interest, and, as they are encrusted with the ores of zinc and lead, all such bones have in later years been thrown into the smelting furnace with the ores from the surrounding rock. The smaller bones were probably introduced by owls, but most of the larger specimens are the broken remains of the food of hyænas and man.

Man clearly occupied the cave at times, for from the beginning of the exploration rude implements both of stone and bone have been continually met with. Among these are pieces of vein-quartz and close-grained rock chipped for cutting and scraping; large round pebbles of quartzite used for crushing seeds and breaking marrow-bones; and pieces of bone and ivory conveniently shaped for digging roots. Some of the implements are peculiar, but most of them might have been made by the existing Bushmen. Their discovery therefore at first excited little more interest than that of the bones, and it was not until the remains of the cave-man himself were found in 1921 that the importance of the accumulation was realised.

The human remains occurred in the deepest and remotest part of the cave which had been reached at the time, but there was nothing in their situation to suggest that they were older than the broken bones and implements which had already been found. According to Mr. Franklin White, indeed, there was originally a vertical fissure above the spot where the skeletons lay, and they may not even have been introduced through the cave itself, but may have accidentally fallen from above. The discovery affords an interesting illustration of the difficulties with which geologists are often confronted when they attempt to determine the age of a fossil.

Two individuals at least are represented by the remains recovered. One nearly complete skull may be assigned to the same individual as a shin-bone (tibia), and the two ends of a thigh-bone (femur). Part of the upper jaw of a slightly smaller individual may be associated with the shaft of a more slender thigh-bone. A sacrum may belong to either of the two. All the bones are remarkably fresh in appearance, and when the skull was first cleaned it adhered to the tongue in the same way as a buried bone which had only lost its animal matter without becoming mineralised. While they lay buried in the cave-earth most of the bones became encrusted with a layer of minute crystals, chiefly of hemimorphite (silicate of zinc), and some of their cavities are partially filled with the same mineral.

The general appearance of the skull is now familiar from the many photographs of it which have appeared in the newspapers. The brain-case shows very little that is unusual, but

the face is probably the largest ever seen in man, and at first sight appears astonishingly ape-like. The inflation of the bone of the brow produces supraorbital ridges almost as stout as those of a gorilla, only partly divided in the middle of the forehead and somewhat less produced at the lateral angles. The ridges overhang immense quadrangular orbits, and the ape-like aspect is further increased below the eyes by the inflation of the maxillary bones, which in ordinary man are indented. The great length (or depth) of the face below the eyes is also striking, for it is nearly as long as that of a gorilla. The opening of the nose, however, is placed and shaped exactly as in man, and the nasal bones roofing the cavity are entirely human. Except that there is no indent at the upper end of the nasal bones where their gently curved surface passes gradually into that of the brow-ridges, the whole arrangement is indeed very similar to the corresponding region of an ordinary modern Australian skull. The essentially human inferior nasal spine occurs at the middle of the lower border of the nasal opening, and the floor of the nasal cavity passes uninterruptedly outwards into the plane of the face, while the sharp lateral edges of the opening are continued slightly downwards on the face, exactly as in the Australian.

The large face of Rhodesian Man, as thus described, finds no parallel even in the lowest existing races of men. It is only comparable with the face of the extinct Neanderthal or Mousterian Man, which is also remarkably large and retains the inflated brow-ridges and maxillary bones as an inheritance from ape-like ancestors. The geological age of Neanderthal Man is known, for his remains are found in the caves of Western Europe definitely associated with those of the mammoth, reindeer, woolly rhinoceros, and other animals which lived in this part of the world during the middle of the Pleistocene period. If, therefore, we can determine whether the face of Rhodesian Man is intermediate between that of Neanderthal Man and the ancestral apes, or whether it is of a more advanced form, we find compensation for the lack of clear geological evidence in the Broken Hill cave and are able to infer the relative age of the new Rhodesian species.

A student of fossils turning to this problem will bear in mind two general principles which seem to be now well established. He will remember that, when a race of animals begins to develop skeletal excrescences, it has reached the end of its course and will not give rise to any higher race. He will also remember that when, during the progress of evolution, a part becomes reduced in size and then enlarges again, this secondary enlargement will generally be on a new plan, not a mere reversion to the old pattern. A student of apes and monkeys will also

observe that the extent of the elongation (or deepening) of the face in these animals invariably depends on the length (or depth) of the bones between the orbits and the nasal opening, not on the length of the premaxillæ, which are always short.

If, then, the bony face of Rhodesian Man be compared with that of Neanderthal Man as known in skulls from the Forbes Cave, Gibraltar,[1] and the cave named La Chapelle-aux-Saints in the Corrèze, France,[2] it will be noted that the form and proportions of the nasal region are almost the same. The nasal opening in the Rhodesian skull only differs from that in the Neanderthal skull in lacking the sharp edge which, in man, usually separates the floor of the nasal cavity from the subnasal region of the face. The essential differences between the two types of skull are in the periphery of the face, where the Rhodesian is much the larger. In the latter the brow-ridges are especially overgrown, and extend much further at the external lateral angles than in the Neanderthal skull. They are thus more gorilla-like. Now, there is not much doubt, both on one general principle already quoted, and on account of the characters of the very ancient Piltdown skull, that when the skulls of the early ancestral apes are discovered they will prove to have no brow-ridges, only a tendency to their development. Most of the existing apes, through successive ancestors, have acquired increasingly massive brows; and at least some of the extinct races of men may be assumed to have shown the same development. In that case, the weaker brow-ridges of Neanderthal Man are nearer the condition in the ape-ancestor than the stronger brow-ridges of Rhodesian Man. From this point of view Rhodesian Man is a later development than Neanderthal Man, and it is interesting to consider the astonishing length (or depth) of his face. Measurement shows that this is entirely due to the great length (or depth) of the premaxillo-maxillary region below the nose: the length (or depth) from the upper end of the nasal bones to the lower border of the nasal opening is the same in the two forms of skull. On general grounds already stated, therefore, the bony face of Rhodesian Man is secondarily enlarged, and much further from that of the ape-ancestor than the bony face of Neanderthal Man. In other words, the Rhodesian is the later of the two races.

This conclusion is supported by a study of the palate, which is absolutely human—may indeed be described as ultra-

[1] W. J. Sollas, "On the Cranial and Facial Characters of the Neanderthal Race," *Phil. Trans. Roy. Soc.*, vol. 199B (1908), pp. 281–339, pl. xxix. A. Keith, *The Antiquity of Man* (London, 1915).

[2] M. Boule, "L'Homme fossile de La Chapelle-aux-Saints," *Annales de Paléontologie*, vols. vi–viii (1911–13).

human. The bony palate is beautifully domed, and the typically human teeth are arranged round it in a horseshoe-shaped row. The molar-premolar series of the two sides show less approach to the ape-like parallelism than those even in some existing races of men; and the third molar, or wisdom tooth, is considerably smaller than the other molars, thus differing from that in all the lowest known races and resembling that of the higher types of man. The front teeth are worn down to stumps by the primitive edge-to-edge bite, but the whole dentition has a very modern aspect from its partial destruction by typical caries. No such decay has hitherto been seen in the teeth of a prehistoric man. The only noteworthy feature of the palate is its immense size, which is not inferior to that of Neanderthal Man. The outside measurement of the dentition across the second molars is 78 mm., and the length from the socket of the median incisor to a line drawn across the back of the third molars is 51 mm. This seems, therefore, to be almost the largest known human palate, being only slightly exceeded by the palate of the Wadjak Man described by Dubois from Java. The lower jaw of Rhodesian Man unfortunately remains unknown.

If the great face with the brow-ridges were removed, the brain-case would scarcely attract attention, except perhaps for the unusual width of the flattened occipital surface for the insertion of the neck-muscles. The thickness of the bone at the accidentally broken edges in the lower part of the right side is no greater than in ordinary European skulls; but a cast of the brain cavity proves that in most parts the bone is as thick as in the average Australian skull. The total length of the skull is about 210 mm., and its width at the parietal bosses is about 145 mm.; so that it is dolichocephalic, with a cephalic index of 69. The capacity of the brain-cavity is about 1,280 c.c., and according to Prof. Elliot Smith the brain must have been of a very primitive type.

There is a slight median ridge along the frontals, and the skull rises to its maximum height just about the coronal suture. This height (from the basion to the bregma) is 131 mm., the same as in the Neanderthal skull from La Chapelle-aux-Saints; but the hinder occipital portion of the skull does not show the bun-shaped expansion and depression which is so characteristic of the skull just compared. The mastoid processes, though small, are typically human. All the other parts of the temporal bone are also essentially human, though the downward extension of the tympanic at the back of the glenoid cavity and the tightly-wedged petrous portion are a little unusual. Most interesting of all, the rather large foramen magnum occupies its ordinary human position, and if its plane

be continued forwards this passes through the narial opening as in the highest races of men. The basicranial axis, which has not yet been thoroughly examined, is only slightly modified in relation to the large size of the face, and there can be no doubt that the skull was poised on an erect skeleton.

This conclusion is especially interesting because the two ends of the thigh-bone and the nearly complete shin-bone found with the skull do not differ in any essential respect from the corresponding parts of a tall and robust modern man. They are totally different from the thigh-bone and shin-bone of Neanderthal Man found in the caves of Belgium and France. The Rhodesian cave man, therefore, represents a distinct species, *Homo rhodesiensis*,[1] which differs from *H. neanderthalensis* in his erect skeleton as well as his relatively large face. This additional feature again suggests that among extinct races the South African is later than the European just compared.

The lands of the Southern Hemisphere have indeed always been the refuges in which old types have survived long after they became out of date and displaced in the more progressive Northern Hemisphere. They are still the refuges of many antique forms of life which we otherwise know only by fossils. The discovery in the Rhodesian cave now seems to show that races of unfinished men were among the latest refugees in the south. The new race in question does not fill precisely any gap in a direct series uniting modern man with his ape-like ancestry. It merely represents one of the latest variants among the multitude which will eventually be discovered to have passed away as failures during the progress of man in the making. It is an advanced stage in which arrested brain-development accompanies enlargement instead of refinement of the face.

[1] A. Smith Woodward, " A New Cave-man from Rhodesia, South Africa," *Nature*, November 17, 1921.

THE SCOPE AND METHOD OF MENTAL ANTHROPOLOGY[1]

By SIR JAMES GEORGE FRAZER, D.C.L., LL.D., Litt.D., F.R.S.

The lectures which I have the honour to deliver in this place deal with a branch of savage society and religion—the Belief in Immortality and the Worship of the Dead in Polynesia. As the subject may be novel, and the reasons for studying it obscure to some of my hearers, I propose to devote the first lecture to a general introduction, in which I will endeavour to explain why savage society is worth studying and how we should study it.

The study of savage society forms part of the general science of man or anthropology. That science is one of the latest born in the sisterhood of the sciences, being hardly older than about the middle of the nineteenth century; in fact, the science is contemporary with not a few of its exponents who have not yet reached the extreme limit of old age. Not very many years have elapsed since two of its founders in England, Lord Avebury and Sir Edward Tylor, passed away. But, though young in years, the science has grown so rapidly that already it is hardly possible for any one man to embrace the whole of it. The principle of the division of labour, which is essential to economic progress, is no less essential to scientific progress. The time has gone by when the comprehensive intellect of an Aristotle or a Bacon could take all knowledge for its province. More and more each inquirer has to limit his investigations to a small patch of the field, to concentrate the glow-worm lamp of his intelligence on a tiny circle, almost a speck, in the vast expanse, which we dimly perceive stretching out to infinity on every side of us. Only by multiplying these glow-worm lamps, glimmering side by side, can we hope, step by step, to diffuse the light of knowledge through the boundless region of the unknown.

In our particular science the first broad and sharp division is between the study of man's body and the study of his mind. The one is known as physical anthropology; the other is now, at least in this country, commonly called social anthropology,

[1] An introductory lecture delivered at Trinity College, Cambridge, November 4, 1921.

but I should prefer to call it by the more general name of mental anthropology. For though man is no doubt pre-eminently a social being and probably owes a large part of his superiority as an animal to the strength of his gregarious instincts, these instincts are only part of his mental endowment, and even when we have abstracted them from our consideration, there still remains in the human mind much that deserves to be carefully studied and that naturally falls under the science of man. It is with mental, as distinguished from physical, anthropology that I shall be exclusively occupied in these lectures.

But even when, in anthropology, we have limited our inquiries to the mind of man, the subject is still so vast that, if progress is to be made, some further subdivision of it becomes necessary. For the mind of man has for ages been investigated by a whole series of special studies, which, under the various names of psychology, logic, metaphysics, and ethics, sometimes summed up under the general title of philosophy, have made great and noble contributions to a science of man. What place, then, is there for the new study of mental anthropology beside these ancient studies? Is there room for her in the venerable college? Can she discharge a function which was not previously performed by her older sisters? We think that she can, and to determine what that function is, we need only perhaps consider the date at which the modern science of anthropology as a whole was first taken up seriously and systematically. The birth of anthropology followed almost immediately the promulgation of the evolution theory by Darwin and Wallace in 1859. I think I am right in saying that the foundation of anthropological societies at home and abroad has everywhere been subsequent to that date and has followed it often at very short intervals. Be that as it may, the theory of the gradual evolution of man out of a long series of inferior forms of animal life is now generally accepted, though diversity of opinion still prevails as to the precise mode in which the evolution has been brought about. It is this conception of evolution which supplies a basis for the modern science of anthropology.

On the physical side human anatomy had been studied for centuries and was, I take it, firmly established on its main lines long before the appearance of Darwin; the new idea imported into the science was that the human body, like the bodies of all animals, is not a finished product, a fixed type, struck out by nature or created by God at a blow, but that it is rather a merely temporary effect, the result of a long process of what resembles growth rather than construction or creation, a growth which we have no reason to suppose has been arrested, but is probably still going on and may cause our descendants to differ as far from us as we now differ from our remotest ancestors in the scale

of animated being. It is only the slowness of the process that hides the movement from our eyes and suggests the conclusion, so flattering to human vanity, that nature has reached her consummation in us and can no farther go. An immediate result of the promulgation of the evolution theory was thus to give an immense impulse to comparative anatomy; for it was now recognised that man's bodily frame is not an isolated structure, but that it is closely related to that of many of the other animals, and that the one structure cannot be fully understood without the other. Not the least important branch of what we may call the new anatomy was the science of embryology, which by a comparison of the human and animal embryos was able to demonstrate their close resemblance for a considerable period of their development, and thus to supply a powerful argument in favour of the conclusion, that man and what he calls the lower animals have had a common origin, and that for an incalculable time they probably pursued nearly parallel lines of evolution. In fact, embryology shows that the very process of evolution, which we postulate for the past history of our race, is summarily reproduced in the life-history of every man and woman who is born into the world.

Turning now from the physical to the mental side of man's nature, we may say that the evolution theory has in like manner opened up a new province of inquiry which has been left unoccupied by the older philosophy. Whenever in former days a philosopher set himself to inquire into the principles of the human mind, it was his own particular mind, or at most the minds of his civilised contemporaries, that he proceeded to investigate. When Descartes turned his eyes inwards and reflected on the operations of his own mind, he believed himself to be probing to the very deepest foundations accessible to human intelligence. It never occurred to him, I imagine, to apply for information to the mind of a Zulu or a Hottentot, still less of a baboon or a chimpanzee. Yet the doctrine of evolution has rendered it highly probable that the mind of the philosopher is indissolubly linked to the minds of these barbarous peoples and strange animals, and that, if we would fully understand it, we must not disdain to investigate the intelligence of these our humble relations.

It is a corollary of the development theory that, simultaneously with the evolution of man's body out of the bodies of lower animals, his mind has undergone a parallel evolution, gradually improving from perhaps bare sensation to the comparatively high level of intelligence to which the civilised races have at present attained. And as in the evolution of the bodily form we know that many species of lower orders have survived side by side with the higher to our own day, so in the evolution

of the mind we may infer that many of the existing races of mankind have lagged behind us, and that their various degrees of mental development represent various degrees of retardation in the evolutionary process, various stages in the upward march of humanity. I say the upward march, because we have good reason to believe that most, if not all, of these laggard races are steadily, though very slowly, advancing ; or at least that they were so till they came, for their misfortune, into fatal contact with European civilisation. The old theory of the progressive degeneracy of mankind in general from a primitive state of virtue and perfection is destitute of even a rag of evidence. Even the more limited and tenable view that certain races have partially degenerated, rests, I believe, on a very narrow induction. Speaking for myself, I may say that in my reading of savage records I have met with few or no facts which point clearly and indubitably to racial degeneracy. Even among the Australian aborigines, the least progressive of mankind, I have not, so far as I remember, noted the least sign that they once occupied a higher level of culture than that at which they were discovered by Europeans. On the contrary, many things in their customs and beliefs appear to me to plead very strongly in favour of the conclusion that aboriginal Australian society, so far as we can trace it backward, has made definite progress on the upward path from lower to higher forms of social life. That progress appears to have been assisted, if not initiated in certain parts of Australia, by favourable physical conditions, chiefly by a higher rainfall in the mountainous regions near the coast, with its natural consequence of a greater abundance of food, in contrast to the drought and sterility of the desert interior.

Having said thus much, I hope I shall be acquitted of the stale charge of treating any of the existing races of mankind either as degenerate or as primitive in the strict sense of the word. As to supposed degeneracy I have said enough ; but as to the allegation that any competent anthropologist regards even the lowest of living races as absolutely primitive, I will add a few words, though in doing so I shall only be repeating a protest which I have raised again and again.

Those of us who hold, as I do, that our species has been evolved in a series of gradual stages from the lowest form of animal life, believe that the line of evolution has not been everywhere the same nor the rate of evolution everywhere uniform. Whether the line was single from the outset and only divaricated later, or whether from the beginning there were several parallel or nearly parallel lines which afterwards diverged from each other ; in other words, whether mankind has sprung from a single pair of progenitors or from several pairs, is an old question which is still debated and, for aught we can see, may

continue to be debated indefinitely. The answer to the question is of little or no practical importance, and for my part I hold no brief either on the one side or on the other. But whether or no human evolution started from a single point, it has certainly run very different courses in different ages and in different parts of the world. It is not merely that the rate of progress has varied in time and place, but that the products, that is, the races, have varied in kind from each other. Hence we cannot arrange the existing races of mankind in a progressive series, and say that in the course of nature the lower would necessarily, though slowly, develop into the higher. We cannot say, for instance, that if we had spared, instead of exterminating, the Tasmanians, they would gradually have acquired all the characteristic features of the Australian aborigines; that but for our interference the Australian aborigines in their turn might have developed into negroes; and that, given a fair chance, negroes might change in time into Europeans. No, the march of humanity is not in single file. We are a very awkward squad, who are constantly breaking the ranks and are very far indeed from keeping step with each other. We have no exact standard whereby to measure the precise degree of evolution attained by any one race, because the common stock or stocks from which all have sprung are unknown to us. How then can we single out any particular race of men, whether in the present or in the past, and say that it is, or was, absolutely primitive? If we could see the whole army of our ancestors marshalled and defiling before us, from the humblest amœbæ to the noblest specimens of mankind, could we lay our finger on the exact spot in the long procession where mere animality ceased and pure humanity began? Surely the change has been too gradual, the transitions too infinitesimal, to allow us thus sharply to define the absolute beginning of our species, to draw a line across our genealogical tree and to say, All our ancestors on the hither side of the line have been men, and all our ancestors on the farther side were beasts. Thus the conception of an absolutely primitive human race, whether in the present or in the past, is so far from being maintained by the anthropologist that he even finds it difficult to attach any precise meaning to the words. Yet he is by no means thereby precluded from applying the adjective primitive in a relative sense to distinguish the less from the more advanced races of mankind. In ordinary speech the relative sense of primitive is freely admissible. Why should it be denied to the anthropologist?

The province, then, of mental or social anthropology may be defined as the study of the mental and social conditions of the various races of mankind, especially of the more primitive races compared to the more advanced, with a view to trace the

general evolution of human thought, particularly in its earlier stages. This comparative study of the mind of man is thus analogous to the comparative study of his body which is undertaken by anatomy and physiology. But whereas comparative anatomy and physiology extend the range of their comparisons far beyond the human species so as to include the whole gamut of animated being, mental anthropology is content for the present to limit its comparisons to the members of our own kind. Yet the limitation is doubtless only temporary; it is to be expected that in time a growing knowledge of the mental processes of the lower animals will permit of a comparison of them with the corresponding processes in the mind of man, a comparison which could hardly fail to throw light on many problems as yet unsolved.

But while in the interest of the science of man a greatly extended application of the comparative method is desirable and in the future inevitable, some well-meaning but injudicious friends of anthropology would limit the application of the method still more narrowly than I have assumed to be temporarily necessary or advisable. They would apparently refuse to allow us to compare the thoughts and institutions, the arts and crafts, of distant races with each other, and would only allow us to compare those of neighbouring races. A little reflection may convince us that any such restriction, even if it were practicable, would be unwise; nay, that, were it enforced, it would be disastrous. We compare things on the ground of their similarity, and similarity is not affected by distance. Radium is alike on the earth and in the sun; it would be absurd to refuse to compare them on the ground that they are separated by many millions of miles. What would be thought of any other science which imposed on itself the restriction which some of our friends would inflict on anthropology? Would geology prosper if it confined its investigation, say, of sedimentary rocks to those of England and refused to compare those of Asia and America? How would zoology fare if the zoologist were forbidden to compare the animals of his own country with the animals of distant countries? the dogs, say, of Wales with the dogs of Africa and Australia? The futility, nay, the inherent absurdity, of the proposed restriction is so manifest that simply to state the proposal explicitly should suffice to expose it. Disguised in the fallacious form of a prudent precept, the nostrum is commonly administered to the sufferer with a trite tag from Dr. Johnson about surveying mankind from China to Peru, as if the mere idea of instituting such a survey were too preposterous for serious consideration. Yet the same men who level this taunt at anthropology would not dream of directing a similar gibe at the sciences of geology,

botany, and zoology, in which the comparisons are world-wide.

To sum up: the central problem of mental anthropology is to trace that evolution of the human mind which has accompanied the evolution of the human body from the earliest times. But as the later stages of that evolution have long been studied by older sciences, it is only fair that the new science should confine itself for the most part to those earlier stages of which the older sciences had hardly taken account. That is why anthropology is commonly, and on the whole rightly, regarded as a science of origins. It is because the question of human origins was till lately a sort of no man's ground, untrodden by the foot of science but trampled by the hoofs of ignorance and superstition, that anthropology has come forward to reclaim this desert from the wild asses which roamed over it, and to turn it into a garden of knowledge. Her efforts have not been wholly in vain. Already the desert has begun to bear fruit and to blossom as the rose.

But if mental anthropology, refusing to poach on the preserves of her elder sisters, confines the scope of her inquiries mainly to the earlier phases of human thought, how is she to accomplish her object? So far as I can see, she can accomplish it only in one of three ways—by a study of the uncivilised races, by a study of children, and by a study of mental pathology. Of the three studies the first is the only one to which I have paid any attention and on which I have the least claim to speak. But, before passing to it, I may be allowed, for the sake of completeness, to say a few words about the other two. And first in regard to the study of children. That the intelligence of children in normal cases undergoes a process of development from infancy to maturity is too obvious and notorious to need proof; and it is a reasonable inference that, just as the development of their bodies in the womb reproduces to some extent the corporeal evolution of their remote ancestors out of lower forms of animal life, so the development of their minds from the first dawn of consciousness in the embryo to the full light of reason in adult life reproduces to some extent the mental evolution of their ancestors in ages far beyond the range of history. This inference is confirmed by the analogy which is often traced between the thought and conduct of children and the thought and conduct of savages; for there are strong grounds for holding that savage modes of thinking and acting closely resemble those of the rude forefathers of the civilised races. Thus a careful study of the growth of intelligence and of the moral sense in children promises to throw much light on the intellectual and moral evolution of the race.

A study of mental pathology, under which I include all

marked aberrations from the intellectual and moral standards of the community, is likely to contribute to the same end. I have been told that the wild fancies of patients in asylums sometimes resemble the superstitious notions of savages. It seems not improbable that cases of mental deficiency are often the result of arrested development or of reversion to an ancestral type; and if that is so, the observation of them should be instructive, since on that hypothesis they reproduce for us phases of the mind which normal men and women have long transcended, and of which, but for these curious reversions, we might perhaps have no inkling.

The third of the avenues by which we may approach the childhood of humanity is the study of uncivilised races in the present and in the past. The study of uncivilised races in the present is obviously feasible, though fraught with many difficulties. But how are we to study uncivilised races in the past? They are gone and have left no written records behind them. For we may perhaps best define an uncivilised race as one which is ignorant of the art of writing: the acquisition of the art of writing is the touchstone of civilisation. Our knowledge of uncivilised races in the past is derived from two sources: first, it is derived from the written, painted, or sculptured records of them bequeathed to us by civilised peoples who observed these vanished races; and, second, it is derived from the skeletons or fragments of skeletons of the races themselves, together with the relics of their handiwork, whether in the form of manufactured articles or of paintings and sculptures on rocks. In regard to the former source, the ancient Egyptians have transmitted to us many graphic representations of the barbarous peoples with whom they came into contact; and the ancient Greek, Latin, and Chinese writers have left accounts of many of the more primitive tribes lying on the outskirts of civilisation; but for the most part these accounts are very superficial and probably inaccurate.

When we come to uncivilised races which have vanished, and of which no written records survive, we depend for our knowledge of them, as I have said, on the meagre remains of their mouldering bones and on the somewhat more abundant remains of their handiwork. The task of studying these remains is the province of that branch of the science of man which is known as prehistoric archæology or prehistoric anthropology. The study was first raised to the rank of a science about the middle of the nineteenth century. In the prosecution of it France led, and still leads, the way. Of her splendid achievements in that great work the Institute of Human Palæontology, recently founded at Paris by the enlightened liberality of the Prince of Monaco, is a noble monument.

Cambridge is to be congratulated on possessing a young and vigorous school of prehistoric archæology under one who has had the good fortune to be a disciple and friend of the great French masters. Of its work it is not for me to speak. It lies outside the main scope of my studies, which have been directed chiefly to the still surviving savage or barbarous peoples, whom I cannot but regard as furnishing us with by far the amplest and most trustworthy materials for tracing the mental and social evolution of our species backward into regions which lie beyond the purview of history. I will conclude this introductory lecture with some observations on that subject. These observations I will comprise under two heads. First, I will say something as to the method of the study; and next I will mention, by way of illustration, a few of the problems which it undertakes to investigate.

First, then, as to the method. In principle it is extremely simple, however difficult it may be in the practical application. The method is neither more nor less than induction, which after all, disguise it as we may under the showy drapery of formal logic, is the only method in which men can and do acquire their knowledge. And the first condition of a sound induction is exact observation. What we want, therefore, in this branch of science is, first and foremost, full, true, and precise accounts of savage and barbarous peoples based on personal observation. Such accounts are best given by men who have lived for many years among the peoples, have won their confidence, and can converse with them familiarly in their native language; for savages are shy and secretive towards strangers, they conceal their most cherished rites and beliefs from them, nay, they are apt wilfully to mislead an inquirer, not so much for the sake of deceiving him as with the amiable intention of gratifying him with the answers which he seems to expect. It needs a peculiar combination of intelligence, tact, and good nature to draw out a savage on subjects which he regards as sacred; to very few men will he consent to unbosom himself.

Perhaps the class of men whose vocation affords them the best opportunities for observing and recording the habits of savage races are missionaries. They are men of education and character; they usually live for years among the people, acquire their language, and gain their respect and confidence. Accordingly some of the very best accounts which we possess of savage and barbarous peoples have been written by missionaries, Catholic and Protestant, English, French, Dutch, German, and Spanish. At the present time one of our most valuable anthropological journals, *Anthropos*, is edited by an Austrian priest, Father W. Schmidt, and is composed mainly of articles contributed by Catholic missionaries in many parts of the world

The articles for the most part are characterised by close observation and a scientific spirit; the theological prepossessions of the writers are not allowed to blur and distort their descriptions of native beliefs and customs. It is much to be desired that the various missionary societies of England would combine to produce a journal of the same scope and the same scientific character. Perhaps, in view of our sectarian differences, that is too much to hope for. But in any case it is highly satisfactory to know that our Protestant missionary societies are awakening more and more to the importance of anthropology in the training of missionaries and are taking active steps to remedy what till lately was a most serious defect in their mental equipment.

Next, perhaps, to missionaries the class of men who can do most for the scientific study of native races are the Government officials who reside among them. However, this class of men labours under certain disadvantages from which missionaries are usually exempt. It is not so easy for them, without a certain loss of dignity and authority, to enter into familiar converse with the natives; and being often transferred from district to district, they do not always gain an intimate acquaintance with the language, and are consequently obliged to trust to native interpretation, an uncertain and often tainted fount of knowledge.

Next to the information obtained by men long resident among savages may be ranked the information acquired by travellers and explorers, especially by the members of scientific expeditions sent out on purpose to investigate the habits and customs of certain tribes. The observations of an untrained traveller passing rapidly through a country are usually meagre, superficial, and untrustworthy; in the enormous literature of travel the percentage of scientific value is exceedingly small. It is otherwise with the information collected by trained anthropologists. Though the time they spend on an expedition is sometimes comparatively short—too short to allow them to obtain a mastery of the language—yet by the application of scientific methods of inquiry they are often able to elicit important information and to make most valuable contributions to knowledge: witness the expeditions of Spencer and Gillen to Central Australia, the Cambridge expedition to Torres Strait, and the recent Mackie expedition to Central Africa.

When a large body of accurate information, based on personal observation and inquiry, has thus been collected, the task remains of examining and comparing the accounts obtained from different parts of the field, in order to see whether they throw light on each other, and whether any general conclusions can be deduced from them. Such comparisons should never

be instituted by observers in the field. Hardly anything impairs the value of observations of a particular people so much as the interpolation of comparisons with other peoples, especially with the Jews, and next to them with the Greeks and Romans, these being the races who have suffered most at the hands of half-educated travellers. Every observer of a savage or barbarous people should describe it exactly as if no other people existed on the face of the earth. The business of comparison is not for him, at least not for him in the capacity of observer; if he desires to draw comparisons with other peoples, as he is of course at liberty to do, he should keep his comparisons strictly apart from his observations: the mixture of the two is, if not absolutely fatal, at least a great impediment to the utility of both.

But while the work of comparison is entirely different from the work of observation and should always be kept separate, it is itself of high importance and is indeed essential to anthropology; without it there could be no true science of man, and the accumulated observations, gained at the cost of great personal risks and sacrifices, would remain an undigested and disorderly heap. It is the application of the comparative method to the heap which evolves order out of chaos by eliciting the general principles or laws which underlie the mass of particulars. It is true that simple comparison is not sufficient for the discovery of the underlying law, but it is the first step towards it. If only our comparisons are just, in other words, if we have correctly sorted out the facts into their proper compartments according to their real similarities, the colligation of the similars in a general truth or law follows almost automatically. Thus everything hinges on the work of comparison. Only with its help can we rise to those generalisations which are the goal of science.

But indispensable as is the application of the comparative method to the raw materials of anthropology, it is not necessary, though it is certainly desirable, that the application should be made at once. If only the materials are collected and safely stored, the work of comparison can be done at any time hereafter; it may even be reserved for future ages. But no doubt much might be lost by thus postponing indefinitely the examination of the facts accumulated by observers in the field. For a comparison of facts observed in different, sometimes in widely sundered, parts of the world often reveals a striking similarity between them which probably escaped the observer, because his attention was rightly concentrated on one particular part of the field, and he had neither the leisure nor the opportunity to notice similar facts elsewhere. The detection of these similarities usually suggests a question which it is desirable to put to workers in the field; and the question in turn may direct the attention

of field-workers to points which they had hitherto overlooked, but which, on investigation, may turn out to be of the utmost importance, opening up a novel and fruitful line of research of which the observer might not have dreamed before. On this ground it is very desirable that the work of sifting and comparing anthropological materials should not be deferred, but should be carried on as far as possible simultaneously with the work of observation in the field. This is possible, because the work of comparison need not be done by the same men who observe the facts; indeed, it may often be done better by others, since it calls for the exercise of different faculties, which are not always possessed even by a keen and accurate observer. A good observer is not necessarily a good theorist, and conversely a good theorist may be a very bad observer. Here, as elsewhere in science, a division of labour and an intelligent co-operation of the labourers are the best guarantees of efficiency. Thus in anthropology at the present day, while the most urgent need is the exact observation of races as yet but little affected by European influence, there is still room for the student at home side by side with the observer in the field. They should work into each other's hands, the one observing and recording, and the other sifting and comparing the records, marking the similarities or contrasts which he detects between them, and questioning the observer accordingly. Thus labouring together in harmony, they will best contribute to the advancement of anthropology. The work of comparison and theory can be carried on with most ease and to most advantage at a great university, because there the inquirer has full access to all the apparatus of learning which few or no private students can command. As an alumnus of this ancient university, I should wish to see established in Cambridge a sort of central bureau or clearing-house, which would receive and examine anthropological reports from all parts of the world, and from which questions, hints, suggestions, and, if you please, theories, would radiate in return to observers stationed in the remotest regions of the earth. Thus a perpetual circulation of facts and ideas would be maintained between the central bureau and the outlying stations; observation would quicken theory, and theory would stimulate observation. You would possess in the University, as it were, a lighthouse from which the rays of science would stream out to illuminate many dark corners of the earth. *Hinc lucem et pocula sacra.*

I have said so much of the method of mental anthropology that I have left myself little time to illustrate by examples the kind of problems with which the science attempts to deal. But this part of my subject is too important to be passed over altogether in silence, though in the few minutes at my disposal I can do no more than simply enumerate a few of the problems

Mental anthropology, as I have said, is in great measure a science of human origins. It investigates, or will hereafter investigate, the origins of language, of the arts, of society, of science, of morality, of religion. To take, for example, the arts of life, it asks, How did man discover the use of fire and the modes of kindling it ? How did he become acquainted with the metals and learn to fashion them into tools and weapons ? How did he come to tame wild animals and to breed them for his comfort and convenience ? How did he first hit upon the idea of sowing seed and waiting for months till the seed should ripen and bear fruit ? In other words, how did he arrive at the conception of agriculture, a conception which has even yet not dawned on some of the rudest races of mankind ? And in regard to the origin of all the useful arts we must ask, was the origin multiple or single ? In other words, was each of the arts discovered independently in various places and at various times ? or was each of them discovered once for all at a single place, from which it gradually spread, through the contact or migration of peoples, to other parts of the world ?

Or to turn to the origin of society, we have to ask, How did men first come to herd together ? did they do so while they were still in the purely animal stage ? and are our gregarious instincts inherited from our bestial ancestors, who hunted, perhaps, in packs ? Or was man, when he first emerged from the beasts, a solitary creature, like some of the higher apes, his near kinsfolk ? Again, when the first social groups of men and women were formed, how were they organised internally ? What was the relation of the sexes to each other ? Was there a complete communism of women ? or was marriage already instituted ? and if so, was it a marriage of groups, or of individuals ? Again, in these groups, what was the relation of parents to their children ? Was the relation known or unknown ? or was it partly known and partly unknown ? Did a man, as some people think, know his mother but not his father ? his brothers and sisters and his sisters' children, but not his own children or his brothers' children ? And how did he come to refuse to marry women who stood to him in certain definite relationships and to regard, as he often did, any such marriage as a horror punishable with death ? These and similar questions have to be faced by anthropology in investigating the internal organisation of the primitive social groups. Some of them have a more than antiquarian interest ; for, if we could solve them, we might at the same time facilitate the work of the modern legislator and social reformer, who has sometimes to deal with practical problems not altogether dissimilar.

When we inquire into the government of the primitive social groups, we have to ask, Was it despotic, or oligarchic, or demo-

cratic? If it was despotic, how did the despot acquire his power? by his prowess in war or by his pretensions as a magician? If the government was oligarchic, was it committed to a troop of warriors or to a junta of old men? If it was democratic, was it in the hands of all the adult members of the community? or was there a discrimination of classes, and perhaps of sex? Or are we wrong in postulating any government at all in the primitive group? May there not have been complete anarchy, every man doing what was right in his own eyes? Here, again, the questions which the anthropologist has to ask are not altogether alien from some which still agitate the bosoms of our civilised contemporaries.

Further, the student of social anthropology has to investigate the thorny question of the rise of private property among men. Is the instinct of private property, as some think, shared by the beasts and inherited by us from our animal ancestors? or did it first develop in the human group? and was it preceded by a period of unlimited communism? and when, sooner or later, the institution of private property was first recognised in a community, did the property belong to certain social groups, say to families or clans, or to individuals? Once more, in investigating these and similar questions the anthropologist can hardly exclude from his mind the heated controversies of his own day. He may even be called in as witness by the disputants to say whether gigantic measures for the confiscation—or should I say the socialisation?—of private property may not be defended by the practice of savages.

Then, to turn for a moment to the origin of science, it is for mental anthropology to ask how men learned to form and use abstract ideas, in particular the ideas of number, which are the basis of mathematics; how they came to note the stars and the apparent motion of the heavenly bodies, the observation of which laid the foundation of astronomy; how they arrived at the idea of measuring dimensions both in space and time, thereby paving the way for geometry and physics; how by marking the annual changes of the seasons they fashioned for themselves a rudimentary calendar; how, perhaps, the false sequence of events assumed by magic may have been slowly replaced in the minds of men by a truer conception of natural law.

Lastly, the student of our science has to consider the question of the origin of religion. How did man come to believe in the existence of gods and spirits? How did he first suppose that he could propitiate them by prayer and sacrifice and so induce them to direct, or alter, the course of nature for his benefit? Whatever the origin of these beliefs, it seems certain that they are peculiar to humanity; we have no reason to assume that they are shared by the beasts. Hence we may safely conclude that they were

evolved, or revealed, at some time subsequent to the emergence of the human species from its purely animal stage.

And the conception of the human soul and its survival after death, how did man arrive at it? Was it by meditation on phenomena of dreams? Was it by observation of the fluctuations and final ebbing of the breath? by the sight of shadows on the grass or of reflections in the pool? by the apparition of the spirits of the dead, or by the sound of their voices falling mysteriously on the ears of the living from a world beyond the grave?

Such are, in the barest outline, a few of the problems with which mental anthropology is called upon to deal, and which she must attempt to solve. Hitherto many of them have been the favourite themes of sophists and ranters, of demagogues and dreamers, who by their visions of a Golden Age of universal equality and universal wealth in the future, modelled on the baseless fancy of a like Golden Age in the past, have too often lured the ignorant multitude to the edge of the precipice and pushed them over the brink. Hereafter it will be for anthropology to treat the same themes in a different spirit and by a different method. If she is true to her principles, she will not seek to solve, or to gloze over, the problems by rhetoric and declamation, by cheap appeals to popular sentiment and prejudice, by truckling to the passions and the cupidity of the mob. She will seek to solve them by the patient accumulation and the exact investigation of facts, by that and by nothing else, for only thus can she hope to arrive at the truth.

THE GEOLOGICAL HISTORY OF THE PRIMATES

By A. G. Thacker, A.R.C.S.

Human palæontology is the most fashionable of the sciences. It is notorious that during the last twenty years most branches of biological science have suffered an eclipse in public interest. The gradual cessation of the great controversies, partly scientific and partly philosophical, which raged during the later decades of the nineteenth century left the educated world somewhat sated with discussions of organic evolution. It was perhaps perceived that latterly nothing very essential or very reliable was being added to those first great revelations which Lyell and Darwin had given to the world, and public attention was diverted into other directions, towards other sciences, such as physics or psychology, or away from science altogether. But in this eclipse of biology, the study of prehistoric man has not shared. The progress of research into the geological aspects of human evolution has never lost its interest with the educated public; since Darwin's time, the importance of the subject has been readily realised by all persons of intelligence; and thus a human fossil is always in the fashion. Who could have avoided hearing of Piltdown? And even the Broken Hill Skull has been able for a few weeks to compete successfully with Freud and Einstein.

And yet, keen as this interest is, both among the onlookers and among the numerous band of investigators it ceases suddenly at a certain point. The investigation is pushed back behind the Neolithic Age and into the Pleistocene Period, in which high savage races are found, having a wonderful art. We pass Neandertal man—a name now familiar to every journalist—the enigmatic Piltdown relics, and the Heidelberg Jaw; finally in the Pliocene we reach the famous Ape-Man of Java, *Pithecanthropus*. With him the interest and enthusiasm reach their climax; but with him also the enthusiasm suddenly ceases. This is the case not only with the general reader, but even with the great majority of those who have taken a serious part in the progress of prehistoric anthropology. The Ape-Man is taken for granted. He is the beginning. He is classed as a

man, and everybody knows about him. Moreover, he lived in Asia, which is traditionally believed to be the scene of human origins. But *Dryopithecus* and *Propliopithecus* are called apes, and few have ever heard of them, though *Propliopithecus* is one of the half-dozen most significant mammalian fossils ever brought to light. It is perhaps usually known, somewhat vaguely, that behind *Pithecanthropus*, in the jungles of Miocene Europe and Asia, there existed a number of great apes, of whom our gorillas, chimpanzees, and orangs are a sparse remnant. They lived there along with the teeming population of mastodons, rhinoceroses, antelopes, three-toed horses, sabre-toothed tigers, and the rest. They were a part of that amazing abundance of mammalian life which bursts upon us suddenly in the Eocene, and which suffered such a grievous impoverishment in the Pleistocene. And the origin and history of these Miocene apes are merely regarded as a part of that uncharted ocean of mammalian evolution on to which the anthropologist refrains from venturing.

And this attitude is not altogether illogical. An anthropologist may be excused for saying that a marmoset is not a man, and is therefore not his job. The line must be drawn somewhere; and, since *Pithecanthropus* is the first of the known Hominidæ, it is appropriate enough that "prehistory" should begin with him. But the geological history of the order Primates is the immediate background of prehistoric anthropology. And it is highly important to have this background as clear as possible. The anthropologist, as such, may not be concerned with it. But it is probably due to this general disregard of the subject that a recent work on prehistoric man, otherwise fairly accurate, begins with a wholly imaginary story of the doings of Eocene apes, without any reference to the known facts of palæontology. And the Hominidæ are admittedly very closely related to the other Primates, especially closely—most of us think—to the great apes and Old-World monkeys. And hence the geological study of the Primates is the necessary and logical link between mammalian palæontology as a whole and the special and narrow subject of prehistoric anthropology.

And the geological history of the Primates is perhaps the more interesting in that it is far from being a simple and straightforward story, such as that of some other mammalian orders, for instance the Carnivora or the Perissodactyla. I do not mean that, when studied in detail, the history of any order is simple, or is anything but highly complex and puzzling. On the contrary, in the case of the Carnivora, there exist some very pretty problems, particularly in regard to the Ursidæ and Viverridæ. But the appearance and subsequent history of the Carnivora

(Fissipedia), considered as a whole, constitute a fairly straightforward story, and are in obvious congruity with other geological facts. With the monkeys and apes it is quite otherwise. The attempt to unravel the history of the monkeys immediately involves the investigator in some of the most vexed problems of geological and geographical distribution, and in all manner of cognate questions—problems of parallelism in evolution, the rising and sinking of continents, the indirect implications of other fossils, and the soundness or otherwise of our usual criteria of mammalian classification.

We have, therefore, to consider the position occupied by the Primates in the history of the higher or placental mammals. The subject is intimately bound up with certain problems of geographical distribution. At the present day, the most striking feature of the mammalian faunæ of all the continents, other than Australia, is their uniformity. There are great cats all over Africa, Asia, and both Americas. The dog-tribe are notoriously cosmopolitan. There are elephants in Africa and in Asia; and within the human period there have been elephants of one kind or another all over the Western Hemisphere as well. There are swine in Eurasia, swine in Africa, and swine in Brazil. There is a tapir in Indonesia, and another tapir in South America. There are monkeys in the jungles of Africa, in the jungles of India, and in the jungles of tropical America. There are hosts of allied groups of rodents all over the world. The list of such cases could be extended indefinitely. It is true that there are exceptions. There are no deer and no bears in Africa south of the Sahara. There are no civets anywhere in the Western Hemisphere. One great group of edentates is purely American. But these exceptions are few. They are so few that they serve only to emphasise that general uniformity which we encounter all round the world, save only in Australia. The great orders of the Placentalia are intermingled nearly everywhere. Even granted that they have been evolved in isolated areas in the past, it might appear that they are now inextricably mixed. Yet geology does enable us largely to extricate them from the apparently hopeless confusion. Geology tells a beautifully simple, albeit a highly dramatic, story of the presence of the great Felidæ, the Canidæ, the deer, the tapirs, and the swine that we find in South America. But it leaves us utterly puzzled about the teeming hordes of American monkeys. At first sight, nothing appears more natural than the presence of monkeys in forests which harbour also cats, dogs, deer, swine, and other mammals familiar to us in the Eastern Hemisphere. But, as will be seen, the American monkeys have quite a different history.

At the present day the continents of what we may call the

placental world are not, of course, perfectly continuous with each other, North America being severed by narrow seas, both west and east, from Eurasia. But within very recent geological times, in the Pliocene, and probably during parts of the Pleistocene, the continents were so continuous ; there was a bridge across what is now the Bering Strait, and probably another through Greenland, Iceland, and the Faroes. The connection between Africa and Eurasia was also more extensive. Moreover, during the epochs mentioned the climate in the latitudes of the northern bridges was less inhospitable than it now is. It is this recent union which accounts for the wide uniformity of fauna to which I have referred. But, apart from this existing unity, which though very real is superficial and in a sense misleading, there is another much more profound unity between the five continents. If, fifty thousand years ago, a land-bridge had arisen between Indonesia and Australia, the mammals of the Oriental Region would speedily have annexed Australia to the placental world. But, as between the other five continents, nothing comparable to such an annexation has ever occurred. The Cainozoic Era, usually called the Age of Mammals, would be more accurately styled the Age of Placental Mammals. And, throughout the Cainozoic Era, all the five continents have been populated by placentals. All of them are true placental territory ; none is an annexation. For, although the existing unity is a thing of yesterday, in the beginning of Cainozoic time there were routes by which quadrupeds could wander everywhere, except to Australasia, and each of the five continents received a population of lowly archaic placental mammals. Great geographical separations occurred later ; but, owing to this original union, the five continents have possessed a fundamental unity throughout the present geological era, and have stood out in sharp contrast to Australia.

We cannot indicate with any exactitude the geographical positions of these original continental connections. Indeed, in some cases it is far from easy to guess where they were situated. In particular, Africa presents a puzzle ; because in the Eocene period the Mediterranean was evidently much more extensive than it is now. It is, moreover, desirable to beware of creating enormous imaginary trans-oceanic land-bridges. The *a priori* objections to great geographical changes which have been advanced by Matthew and others are not indeed very convincing. Large areas of land have come up from beneath the sea at different epochs, and hence there is nothing improbable in the idea that considerable masses have sunk. The question is rather one that should be judged on the evidence in each particular case. But we must remember that, when viewed in true geological perspective, the entire Cainozoic Era is very

short, and we must therefore be cautious in accepting stories of stupendous geographical changes within that era. In particular, the theory of a great trans-Atlantic connection between Africa and South America, a connection persisting till Middle Cainozoic times,[1] creates many more difficulties than it solves. This theory has been supported by many naturalists, including Berryman Scott. But whilst the hypothesis helps us to account for the distribution of a few small groups of mammals, it leaves the utter dissimilarity of the respective mammifaunæ of Africa and South America during Eocene, Oligocene, and Miocene times completely unexplained. It is, therefore, impossible to indicate all the routes followed by the quadrupeds in the original dispersal. Nor is this surprising, for the events in question took place in that mysterious time represented in stratigraphy by the top of the Cretaceous and the base of the Paleocene, and by the gap which so often exists between them. We are much in the dark about this epoch. What we do know is that the northern continents, Africa, and South America all possessed stocks of placental mammals in early Cainozoic times.

It is necessary, in order to appreciate the character of the problems surrounding the fossil Primates, to enter into certain further details of historical zoögeography. As the reader is aware, we divide the Cainozoic Era into the Paleocene, Eocene, Oligocene, Miocene, Pliocene, Pleistocene, and Holocene (Recent) Periods. It is useful to recall what these periods are. They are, of course, pieces of time corresponding to certain stratified rocks which happen to have been preserved for our edification. These pieces of time are incomplete fragments of Cainozoic time. They are, moreover, a European and North American series. If the pioneer work in geology had been done in South America, we should not have had the same series. Further, if we knew the complete story of the Cainozoic we should sub-divide it on a different principle : we should divide it, as we divide human history, by the outstanding events. Now, we are not altogether in the dark about the outstanding events of the Cainozoic ; through and between the stratigraphical periods we can perceive other periods, which are marked by these outstanding events. We can see a partial but important discrepancy between the results of these two methods of dividing the present geological era. It is not a complete discrepancy, because the stratigraphical divisions are, of course, founded on palæontological data ; but the partial discrepancy is important for our present purpose.

[1] The distribution of various groups of terrestrial invertebrates is said to afford evidence of such a trans-Atlantic connection in Mesozoic times. But that is another question, which it is not the object of the present article to discuss.

We have seen that at some epoch which we should describe as Late Cretaceous or Early Paleocene there was a dispersal of Placentalia throughout the five continents. The land-bridges may not all have existed actually contemporaneously, but they existed at about the same time. We infer, then, the existence of a period of general union. In the Paleocene rocks of Europe and North America we find five orders of archaic placental mammals. Four of those orders reached South America. The fifth order is the Creodonta, the group of primitive beasts of prey. These animals teemed in North America, but apparently never reached South America. We infer, therefore, that South America was separated at a date prior to the formation of the oldest known Paleocene rocks, and at a time when four of the Paleocene orders, but not the Creodonta, had been evolved. This is rather a pretty result. The Creodonta would naturally be evolved later than the herbivores on whom they preyed. From this point onwards till the Pliocene, South America has a history totally distinct from the rest of the world.

We know much less about the fossil mammals of Africa than about those of South America ; indeed, until the present century, we knew practically nothing about them. We do not meet the fauna until the Middle Eocene of Egypt. Here we find a fauna distinct from that of the northern continents, but less distinct than that of South America. There were Creodonta in Africa. Also certain rodents, chiefly of the Hystricomorph (Porcupine) group. Also certain Artiodactyla (even-toed ungulates), including swine and a primitive extinct group, the Anthracotheriidæ. All these groups flourished in northern lands, where they had apparently been evolved since the severance of South America. But there were no Perissodactyla (odd-toed ungulates), nor true Carnivora in Africa, although these abounded in Eocene Europe ; and there was a similar absence from Egypt of certain groups now extinct which were characteristic of the Eocene of the north. On the other hand, there were primitive Proboscideans and primitive Hyracoidea in Africa, both these groups being absent from the Eocene of Europe. In addition, there were in Africa representatives of two other quite peculiar orders of ungulates, the Embrithopoda (the genus *Arsinoitherium*) and the Barytheria. Hence it is usually inferred that these latter four orders were evolved in Africa. In regard to the Creodonta and Artiodactyla possessed in common by Europe and Africa, it is difficult to know whether these are all ancient animals dating back to the original connection, or whether some of them may have made their way southwards across the seas, perhaps by chains of islands. The correct interpretation of the evidence

is none too clear; but one is inclined to think that the connection of Africa with the main land-mass was closer and later than the similar connection of South America.

During the Cainozoic Era there have also been separations between Europe and North America. But such separations were of relatively brief duration, and for the purposes of this general survey Europe, Asia,[1] and North America may be regarded as a single land-mass, constituting the Mainland.

It remains to be stated that Africa was reunited to the Mainland at the end of the Oligocene, and that the reunion of South America occurred in the Pliocene. In the Miocene we find the Proboscidea wandering all over Eurasia and North America. At the same time, also, there were doubtless great southward migrations of Perissodactyla, Artiodactyla, true Carnivora, rodents, and other animals into Africa. During the long period of South American isolation there occurred a remarkable and of course independent evolution of Placentalia in that continent. Three orders of herbivores were evolved, the Toxodontia, Pyrotheria, and Litopterna, certain of the last-named group being famous for the extraordinary parallelism which they display to the horses of the Mainland. Apart from these three extinct orders, the American edentates, the Xenarthra, flourished and produced a number of families—sloths, armadillos, and others. Although not known from Paleocene rocks, the Xenarthra appear to have been evolved in that period—probably from the Paleocene Tæniodonta—because an aberrant armadillo is known from the Eocene of North America, long after the separation of the two continents. In the absence of carnivorous placentals, a peculiar group of rapacious marsupials, the so-called Borhyenas, survived and prospered in South America during the period of isolation. The presence of these great groups in pre-Pliocene South America is much what we should expect; the groups are either very ancient and archaic or peculiar to the continent; the only surprise is the existence of Xenarthra in North America as well, and the inference which we have to draw regarding the antiquity of that group. But there are two other elements in the ancient fauna whose presence there is very startling. One of these is the Hystricomorph (Porcupinish) rodents. The other group is the monkeys of the existing family Cebidæ These latter problems are perhaps the most difficult and puzzling questions which arise out of mammalian distribution.

As already stated, South America was reunited to the Mainland in the Pliocene, and the greatest of all migrations then took place. The united faunæ of the Mainland and Africa had

[1] This does not apply to the southern peninsulas of Asia, which were largely insular during the earlier Cainozoic.

then reached the very climax of their evolution; Artiodactyla, Perissodactyla, Proboscideans, and Carnivora swarmed into South America, and it went ill with the feebler indigenous quadrupeds of the southern continent. In this way the existing uniformity of the five continents was brought about.

At this point it is convenient to summarise the foregoing argument. In the Cainozoic we can discern, I think, five periods. The first period is one of general unity, though, as already pointed out, there may never have been a point at which all the land-bridges existed absolutely contemporaneously. Then South America was separated. The second period was therefore characterised by the isolation of South America and the union of the other four continents. Then Africa was separated. In the third period therefore three enormous but distinct areas of placental evolution existed. This period was long; it corresponds to the greater part of the Eocene and most of the Oligocene of the stratigraphists. Then Africa was reunited. The fourth period therefore resembled the second in the independence of South America and the union of the other four continents. Finally, South America was at last reunited, and this event ushered in the fifth and last period, an epoch of general unity; this general unity seems to have been surprisingly perfect in the Pliocene, though seriously interrupted for brief periods during the cold phases of the Pleistocene and at the present day.

These separations and reunions are the outstanding events of the Cainozoic Era. They profoundly influenced mammalian evolution. They are, it seems to me, to be regarded as literally "epoch-making."

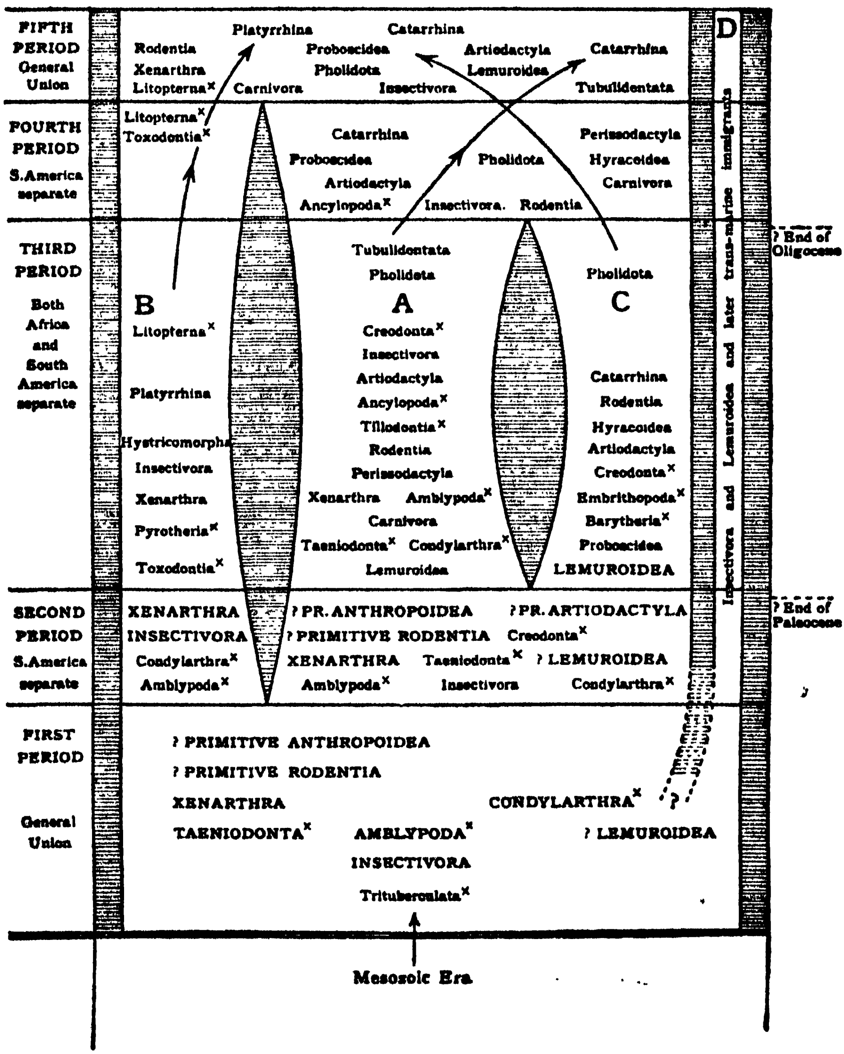
I have endeavoured to show these periods in the diagram. By abandoning all attempt at depicting the actual form of land, it is possible (in this simple case) to show land and time on one diagram.

Within the limits of the present article it is impossible to enter into the thorny problem of Madagascar, and, since there are no Anthropoidea there, it is fortunately unnecessary to do so. The old theory put forward by Wallace and Sclater was that the Madagascan fauna (other than the civets) was a "sample" of the pre-Miocene mammalian life of Africa. This theory was invented, however, before the recent discoveries in Egypt, and it is obviously incorrect. Matthew now thinks that the fauna is entirely derived from chance voyagers from Africa. The assemblage has not an African appearance; but, from wherever the lemurs came, and whenever they arrived, they have not had to compete with their higher relatives, and Madagascar has been the chief centre of lemurine evolution.

We pass now to a brief examination of the actual fossils

of Primates. In view of the considerations that I have advanced in the foregoing pages, the reader will readily appreciate that the horizontal distribution of these fossils is quite as interesting and significant as their vertical distribution. The

TIME AND SPACE DISTRIBUTION OF THE PLACENTALIA



place of the Primates in mammalian evolution cannot be perceived unless both aspects of the matter are borne in mind.

As the reader is aware, the Primates are divisible into two sharply distinguished sub-orders, the Lemuroidea and the An-

thropoidea. The former have the orbit and the temporal fossa incompletely separated, the lachrymal foramen is outside the orbit, the humerus has an epicondylar foramen, and the femur has a third trochanter. The Anthropoidea have the orbit completely shut off from the temporal fossa, the lachrymal and its foramen are inside the orbit, the humerus (ordinarily) has no epicondylar foramen, and the femur has no third trochanter. There are also differences in the soft anatomy, particularly in the reproductive organs. The Lemuroidea are much the more primitive. Some writers separate them from the Primates altogether, and place them as a distinct order, the "Prosimiæ."

As we should expect, the fossil Lemuroidea take us back much further than the fossil Anthropoidea. None are certainly known from the Paleocene of any part of the world, though a number of fragments (probably of Insectivora) have been claimed as lemurine. But a genus *Plesiadapis* occurs in the Landenian of Belgium, this being a transition bed between the Paleocene and Eocene. Lemurs are numerous in the Eocene, both of Europe and North America. All the remains are of course fragments, but many of them are well preserved, and are quite typical. There are three families. The Adapidæ of Europe include *Plesiadapis* and *Adapis*. The *Anaptomorphidæ*, found in both Europe and North America, are believed to be related to the existing Tarsier. The *Notharctidæ* of North America are nearer to the typical lemurs now living. It is interesting to note that *Notharctus* had retained the first premolar, which is lost in all other Primates. Lemurs are not known either in Europe or North America after the end of the Eocene. None have been certainly recorded from South America at any period. The chief subsequent evolution of the sub-order has been in Madagascar, as already indicated. In that island they have flourished exceedingly, and some of the species attained large dimensions in the Pleistocene.

Passing on to the Anthropoidea, we find the fossils less ancient, but having the most astonishing horizontal distribution. The Anthropoidea other than the Hominidæ have undergone scarcely any evolution since the Miocene, and even the Simiidæ (apes) were in existence in the Oligocene. I endeavoured to point out the bearing of these particular facts on prehistoric anthropology in a former number of this journal.[1] Great apes are known from the Miocene of Europe and India—*Dryopithecus*, *Hylobates*, *Paidopithex*, *Sivapithecus*, and others. And with these also were associated Cercopithecidæ (Old-World monkeys), which were apparently typical and modern in character. All these animals, be it noted, belong to the fourth

[1] See "The Extinct Apes," October 1914.

period shown on the diagram. They are later than the reunion of Africa to the Mainland. But the oldest fossils of the Old-World Anthropoidea occur not on the Mainland, but in Africa, namely in the Egyptian Oligocene. These specimens are two small mandibles to which the names *Propliopithecus* and *Parapithecus* have been applied, and a third fragment resembling the latter. *Propliopithecus* is referred to the Simiidæ. Schlosser made *Parapithecus* the type of a new family, as he supposed its dental formula to be different from that of the Cercopithecidæ, but I concur with Gregory's view that Schlosser misidentified the teeth, and that the formula is really the same as that of all other Old-World Anthropoidea. *Parapithecus* is therefore probably a primitive member of the Cercopithecidæ. The two points especially worthy of notice are the surprising antiquity of the true apes, and that these animals dwelt in Africa before the reunion, that is, in the third period.

At the present day there is, of course, a very sharp line, not only geographically but anatomically, between the New-World Anthropoidea, the Platyrrhina, and the Old-World Anthropoidea, the Catarrhina. The Platyrrhina have the nostrils far apart and have no bony external auditory meatus ; the Catarrhina have a narrow nasal septum and possess a bony external auditory meatus ; and there are minor differences. As contrasted with the Lemuroidea, however, the resemblances of the Platyrrhina and Catarrhina outweigh the differences between them. The Platyrrhina include the two families Hapalidæ and Cebidæ. The Catarrhina include the two families Cercopithecidæ and Simiidæ, as well as the Hominidæ. The Hapalidæ (Marmosets) and the Cebidæ are quite distinct from each other ; the relationship of the Cercopithecidæ and Simiidæ is closer. Moreover, there are certain cross resemblances between the Cebidæ as such and the Catarrhina. One point has perhaps special interest. The Cebidæ and the Catarrhina have nails on all their digits. The Hapalidæ have claws on all the digits, except on the hallux (first toe), which has a nail. If these claws of the marmosets be primitive, as there seems no reason to doubt, and if the association of the Hapalidæ and Cebidæ be valid, we must suppose that nails were independently evolved by the Cebidæ and the Catarrhina.

Fossil Cebidæ are found in South America as far back as the Miocene, the best-known genera being *Homunculus* and *Pitheculus*. The relics preserved seem to indicate that these monkeys did not differ greatly from the modern representatives of the family.

We find, therefore, that the Catarrhina and Platyrrhina were already established as distinct groups anatomically in the Oligocene. This in itself implies a high antiquity for

the Anthropoidea as a whole, but such an inference is much strengthened by the astonishing character of the geographical separation. These Anthropoidea who were living among primitive and peculiar mammals in Miocene South America—whence did they come, and how long had they been there? The only really parallel case is that of the Hystricomorph rodents, to which I have already referred. The early Hystricomorph remains are somewhat older and much more abundant than those of the Platyrrhina. Berryman Scott makes use of his trans-Atlantic bridge to get these animals across from Africa. Matthew evolves the Platyrrhina from hypothetical Paleocene lemuroids, quite independently of the Catarrhina; as for the Hystricomorpha, he suggests, though with comprehensible hesitation, that they may have floated across the Atlantic on a raft. One feels that even "Atlantis" is less improbable than such a raft. Abel brings the monkeys across the east-west channel from North America in the Eocene, and apparently Osborn also accepts this theory. The objection to this view which has been felt by many naturalists, is that there is a total absence of both Anthropoidea and Hystricomorpha from the geological record of North America. But negative evidence of this kind is of doubtful value; and in the case of the Hystricomorpha it certainly has little weight, for they occur in the Eocene of Europe; and, since they were therefore on the Mainland, a few of them may well have reached North America. This seems much more plausible than either rafts or trans-oceanic bridges.

Finally, there is the possibility that certain groups of the higher, as distinct from the archaic, placentals have a greater antiquity than has been supposed, and existed before the barriers to dispersal arose, or when those barriers were less formidable than they subsequently became. This may be true of the Xenarthra, of the Hystricomorpha, of primitive Anthropoidea, and perhaps of certain Artiodactyla. It can hardly be without significance that the same two groups of higher mammals, the Hystricomorpha and Anthropoidea, whose presence in ancient South America puzzles us, are also found associated in ancient Africa. And the two branches of the Anthropoidea resemble one another in too many points for the convergence theory to be probable. Some of the higher groups of mammals may have existed as scarce and inconspicuous elements in the fauna of Paleocene times.

Apart from these theoretical considerations, I have endeavoured in the foregoing pages to show how much is clear and how much is at present uncertain in the general story of the Placentalia, and to indicate the highly mysterious part in that story which is played by the higher Primates. The American

monkeys are still one of the most remarkable problems in mammalian zoology.

Another point which is of special interest to anthropologists is that, according to the indications which we possess at present, the evolution of the Catarrhina as a distinct group took place in Africa.

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EXPLANATION OF DIAGRAM

The Diagram shows the distribution in time and space of the terrestrial orders of Placental Mammals, as described in the text.

A = the Mainland.
B = South America.
C = Africa.
D = Madagascar.

The names in large type are those of orders whose presence at the particular time and place is only a matter of *inference* from geographical distribution.

The extinct orders are starred thus : X.

As stated in the text, the chief element of uncertainty is the date of the separation of A and C. I have assumed that it occurred at about the transition from Paleocene to Eocene conditions.

For the information of the general reader it may be added that the Amblypoda and Condylarthra are archaic ungulates, the Trituberculata are a group of Mesozoic mammals, believed to be placentals, the Tæniodonta are archaic edentates, the Ancylopoda are ungulates, and Tillodontia are a group of aberrant insectivores, the Pholidota are the pangolins, and the Tubulidentata are the aardvarks. These groups are included for completeness, although they are not all mentioned in the text.

SPITSBERGEN

ITS NATURAL HISTORY AND RESOURCES

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To many Spitsbergen is but a name, or perhaps a shadowy phantom in some way connected with the Peace Conference; to others who study their atlases and note the geographical position of Spitsbergen its character becomes more definite. By these people, however, it is probably dismissed as an ice-bound and snow-covered desolate tract, when they discover its latitude—between 76° and 81° N. This estimate is, in some ways, a just one as far as the greater part of the year is concerned.

Very few people, except those who have visited Spitsbergen, realise what a wonderful country it is at its best. In addition to natural beauties of a very high standard, which would appeal to all, Spitsbergen presents to those who have a scientific bent some problems of absorbing interest.

Spitsbergen is an archipelago composed of a large number of islands, of which two—West Spitsbergen and North-East Land—compose the majority of the area, while of other islands, Edge Island, Barents Island, and Prince Charles Foreland are important. Bear Island, which, though some way off, belongs naturally to the group, is also interesting.

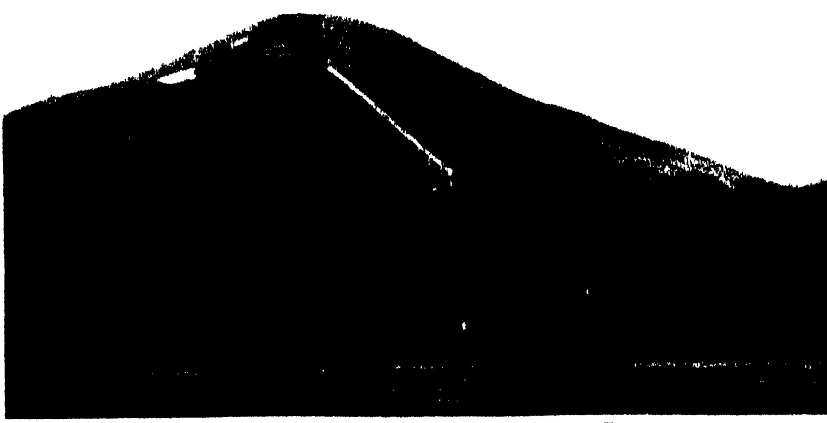
A careful study of the map and a mental comparison with surrounding districts reveal the first remarkable feature of the country. This is referred to above—namely, its extremely high latitude. A comparison with Greenland, generally looked upon as a typical arctic country, shows that the greater part of that country lies further south than Spitsbergen. The same applies to the North-American archipelago and to the Arctic regions of Siberia. Yet during the summer months—June, July, and August—Spitsbergen enjoys a climate very similar, and in some respects superior, to that of England in January. This wonderful summer climate is, of course, common to many Arctic and Antarctic countries, but nowhere else are such con-

Mount Chisholm, Prince Charles Foreland

[illegible] showing the type of scenery [illegible] North-West Spitsbergen

The Colorado Range, Temple Bay

[illegible] extensive [illegible] horizontal [illegible] very [illegible] of the region



The Coal Mine at Longyear City, Advent Bay

In the foreground will be seen the wire ropeway for conveyance of coal to the quay

ditions found in so high a latitude. What, then, is the explanation? On further examination of the map the mind leaps at once to that universal panacea for climatic anomalies—the Gulf Stream. This explanation is partially correct. There is no doubt that the Gulf Stream Drift or North Atlantic Drift is responsible to a certain extent. This is borne out by the fact that the west of Spitsbergen, on the whole, has a less severe climate than the eastern part of the archipelago. However, the oceanic nature of Spitsbergen, surrounded as it is in all directions by extensive seas, is also responsible for the climate. In addition to this, there are certain factors as yet unexplained.

Spitsbergen, therefore, as the result of the combined effect of the factors mentioned above, enjoys a similar climate to that of Southern Greenland, Novaya Zemlya, and the northern Asiatic coast, all of which are much further south.

The climate may be divided into four seasons, although these are not well defined. At the beginning of the year all the country is covered in snow, and the coasts are icebound—the temperature being on an average from $-20°$ to $-40°$ F. This continues until May, when the temperature rises considerably. In June, the spring month, the thaw sets in, and by July most of the snow, at any rate in the lowlands, has disappeared. During June, July, and August the weather is usually fine, but may be overcast. The sun is up during the whole twenty-four hours, and this makes the summer, although short, very favourable to life and growth. In September gales commence, accompanied by snow, and these continue throughout September and October, this period constituting the autumn. The winter then follows with its continuous darkness and extremely low temperature.

The severe climate, with its frosts and gales in autumn and winter, and the erosive action of the snow-water in spring and summer, have their usual effect. Erosion is very rapid and proceeds along the usual Arctic and Alpine lines. Aiguilles, screes, and other frost effects are common. In addition to this, however, there is ice action, and this is extremely important. Glaciers abound, especially in the northern part of the archipelago, where they generally form sea fronts as in Greenland. There is, on the other hand, very little real inland ice, except in New Friesland and North-East Land.

The result of these erosive agents is the production of a very complicated physiography. As a general rule the interior of Spitsbergen consists of a complex of mountain ranges, the valleys between which are occupied by glaciers. This is enhanced by the many fjords and sounds which penetrate into the heart of the archipelago, and which almost divide West Spitsbergen (the main island) into several parts.

Another factor which tends to complicate matters, and helps, incidentally, in the production of magnificent scenery, is the very varied geology of the region. Almost every geological horizon from Pre-Cambrian up to Quaternary times is represented somewhere or other. Yet another point of importance is the fact that the arrangement of the various strata is exceedingly complicated. Generally speaking, the older rocks are in the north-west, the newer towards the south-east; but, apart from that, nothing very definite can be said with respect to the sequence of the strata. This confusion is probably due to the extremely varied history of the archipelago.

Then, again, in the actual lie of the rocks there is great variety. In north-west Spitsbergen and Prince Charles Foreland the dip of the rocks is tilted at various angles, with the result that sharp peaks are easily produced on weathering. In the region around Icefjord the strata are horizontal, and weathering results in the production of flat-topped, table-like mountains. Following on, however, the effect of frost is seen in such mountains as Temple Mountain, where a very close resemblance to some kind of Eastern temple is apparent.

When the investigator probes deeper into the tale which the rocks have to tell, even more valuable information can be obtained. The fossil remains of Spitsbergen are very varied and numerous; they have been known for a long time, while many articles have been written both on the fossil animals and the fossil plants. The animal fossils show us, as elsewhere, that the country has in the past enjoyed climates very different from that which it now possesses.

It is, however, on the evidence of the plants that the most interesting information regarding the past history of Spitsbergen is based. At the present day there are no trees, as we know them, on Spitsbergen. The Polar willow is only a few inches high, and hardly merits the name of tree; for shrubs one has to be content with the cross-leaved andromeda, a dwarf shrub allied to the heather; even the herbs are nowhere more than eighteen inches in height, and that only during the short flowering period.

The records in the rock, on the other hand, have a very different story to tell. Plant fossils occur in many places, and are of various geological ages. Many of them have been identified as belonging to plants closely allied to the oak, beech, and other species of our common woodland trees. From these remains there seems to be no doubt that there were formerly extensive forests in Spitsbergen. Other fossils belong to groups and genera of plants now found only in sub-tropical or tropical regions. For instance, leaves of magnolias and of cycads have been found; the latter are common, especially in certain areas.

The Nordenskjöld glacier from the west
[illegible]

An area of typical dry tundra at Klaas Billen Bay
In the drainage channels is the Arctic willow accompanied by mosses

The cycads are at present a purely tropical group. An interesting point is that the same cycads used to live in this country at the same time as in Spitsbergen—namely, the Jurassic period—in fact, they had a world-wide distribution then. The cycads also were most of them tree-like, as are their modern representatives.

All this evidence of conditions at such a comparatively short distance from the pole so much more favourable than at present has been the origin of many interesting theories. Some authorities contend that there has been a shifting of the poles since the Tertiary period ; others oppose this view. There is, however, at present no satisfactory explanation, but speculation is rife, as it always is in such vital and fundamental questions.

Many of the fossils are not so much important from the theoretical questions to which they give origin as from their own intrinsic interest. An example of this is the Labyrinthodont, which has been known for some time, but which has only just been brought away from the country by the recent Oxford University Expedition. This fossil is interesting in that it is a very early example of a vertebrate, having existed during the Triassic period. When investigated more fully, it will probably throw light on the question of the evolution of the Vertebrata.

When the present-day conditions are examined, other questions equally absorbing are encountered. These are mainly connected with the life, both animal and plant, of the archipelago.

Owing to the climate the active period for life in the country is necessarily limited, and consists of the summer months—June, July, and August. During the winter, however, active manifestations of life are not entirely lacking, although, owing to the deep snow-covering, they are limited to animals. The animals which are found in the winter are the reindeer, Arctic fox, and Polar bear, while the ptarmigan is also a winter resident. Of these the reindeer and ptarmigan subsist on the vegetation beneath the snow. The reindeer, with its snow-scraper antlers, is able to clear a way to the succulent mosses and lichens, and thus can eke out a precarious livelihood. The Arctic fox and Polar bear, being carnivorous, prey on the other animals. The fox usually prefers ptarmigan ; the bear hunts chiefly in the sea, seals and other marine creatures forming its main diet.

As mentioned above, the first sign of the approaching summer is the rise in temperature commencing in May or even in the latter part of April. The rise continues throughout May, so that by June the melting-point of the snow is reached. A thaw thereupon commences, and this is continuous during the summer. As soon as, or before, the snow melts, the birds begin to arrive from the south. Nesting is started on any

area free from snow, especially on maritime cliffs which many species, e.g. guillemot, kittiwake, fulmar petrel, usually make their home. These cliffs, being steeply sloped, are naturally devoid of snow at a comparatively early date. The birds which nest on the lower and flatter country usually arrive somewhat later, as their breeding-grounds are still snow-covered. Soon after the snow has left any area the plants thus uncovered wake up from their long winter sleep. Shoots are pushed rapidly, leaves appear, and finally flowers may be seen, making gay spots which only a week previously were snow-covered wastes. This does not apply to all the plants, many of which require a longer time to attain the flowering stage. The bird life, as evinced by breeding, reaches its maximum development in June and July. On the other hand, vegetable life, although well developed, is at its best, at any rate as regards flowering and fruiting, in the latter half of July and during August. In September the gales commence; a fall in temperature sets in; the birds begin to seek more congenial southern climes; the plants with their ripened fruits dispersed, or ready for dispersal, sink again into the dormant state in which they pass the winter.

The dominant type of animal life in Spitsbergen is without doubt the birds. Although there are actually not very many species—approximately twenty-five—yet they make up for it in numbers, in activity, and in size. The snow-bunting, a close ally of our common finches, is the only small bird. Of the others, the majority—such as gulls, skuas, ducks, geese, divers, etc.—are relatively large species. In addition to this, the birds, with the exception of the snow-bunting and ptarmigan, are either maritime or freshwater birds; several are waders. Many of the species are extremely interesting to ornithologists for various reasons. The barnacle goose, for instance, which only nests in Arctic regions and very locally, chooses such inaccessible nesting-sites that its nest is only found with difficulty. Indeed, the nests and eggs discovered last year by the Oxford University Expedition were the first ever taken by an Englishman. In other cases very little is known of the nests. The ivory gull is a good example of this, as its breeding-places have not yet been found.

Another point is the so-called "Sexual Inversion." This is found in the Phalaropes and probably in other waders. The interest lies in the fact that the cock-bird carries out what should be the work of the hen-bird. He incubates the eggs and looks after the young, while the hen, which is usually more gorgeously coloured, hunts for food and does the ordinary work of the cock.

The only three higher land animals in Spitsbergen are those

mentioned above—namely, the reindeer, Arctic fox, and Polar bear. None of these is as common as in past years ; they are not frequent in the west, but become more abundant as one goes northwards or eastwards. There are, however, several marine mammals, as well as fish. First in interest are the whales, of which there are several species. The Greenland whale is now rarely seen, owing to the depredations of whalers. The white whale, or beluga, is frequently met with, although less common than formerly. Both these species have at various times supported large and flourishing whaling industries. There are also finner whales of various kinds, as well as dolphins and porpoises. The walrus, once so common, is scarcely ever met with on the west coast, but occurs more frequently in the east. Hunting for profit and sport has played havoc with its numbers. Of seals there are several species, and these, although hunted a great deal, are still quite common. There is one freshwater fish, the Arctic char, closely allied to the salmon ; it is found in a few of the lakes. Of the salt-water fish the cod is the most important.

Passing now to the lower animals, the deficiency is seen to be even more striking. The insects are very poorly represented : not only this, but to find them careful search is necessary. There are no butterflies, and but two species of moths have yet been discovered. Bees and wasps are absent, although there are many suitable flowers. On a fine sunny day, flies—not the ordinary house variety—come out in numbers ; they may be seen on the flowers of the purple saxifrage. Otherwise, flying insects are practically non-existent. Ground animals are rather more common. On turning stones, especially on a sunny day, spiders, mites, springtails, and perhaps a beetle, can be found. Ants are conspicuous by their absence. Small round worms—nematodes—inhabit the soil ; of earthworms there are none, neither are there slugs nor snails. This scarcity is probably to be correlated with the frozen state of the ground during the greater part of the year. Even in summer there is ground ice at no great depth.

In the case of freshwater animals conditions are more favourable. A comparatively abundant fauna—as regards numbers—is to be found in the pools, especially in those of a permanent character. Very interesting results are expected from the study of the freshwater fauna carried out by the Oxford University Expedition. In the sea Crustacea and Mollusca are fairly well represented. Here conditions are not so extreme as on the land, the variation in the temperature of the water being comparatively small.

When one turns to the plants, however, a rather different aspect is revealed. Among the animals, birds constitute the

only prominent group—with the plants there are several groups well distributed and very prominent. On the other hand, several large groups are nearly or completely absent.

It is hard to say at times which is the dominant type of plant in Spitsbergen. To begin with, tree-like forms are entirely absent, shrubs practically so. The tallest herb is nowhere more than eighteen inches in height! There is, therefore, a great uniformity in the vegetation. The same dreary monotony of low herbage greets the eye everywhere; it is only beautified by the wonderful display of flowers. In spite of their small size, the plants seem covered with blossoms—and what marvellous blossoms! The beautiful reds and purples of the purple saxifrage; the delicate yellow of the mountain avens and the poppies; the pink sheets of the moss campion; the glorious yellow of the marsh saxifrage. From this point of view it may well be argued that the flowering plants, as elsewhere, are the dominant type. Yet there are extensive areas, covered with moss and lichen, in which the higher plants hold a very subordinate position. In these areas the lack of flowers is compensated for by the varied shades of green, yellow, orange, and brown, alternating with one another in a manner resembling patchwork. It is a wonderful sight on a sunny day, many of the lichens especially being quite showy. I well remember a large, erratic boulder—10 feet in height, at least—one blaze of gorgeous orange or flame-colour due to a crustaceous lichen.

The vegetation may be classed as ice-desert or tundra. Much of Spitsbergen, owing to the configuration, presents the features of Alpine vegetation rather than that of tundra. The two are, however, very closely allied. There are one or two areas which are characteristic tundra—flat and swampy, pools dotted about at intervals, and everywhere under foot a quaking bog.

The flora, as might be supposed, is not very rich in species. Of flowering plants about 150 species have been described, while additions are made at intervals. There are also several hundred species of mosses and lichens, one or two ferns, and a horsetail or so. Freshwater Algæ are fairly frequent in the streams and pools; marine Algæ less so, although on suitable rocky coasts Laminarias and allied species make a good show. Of the extensive group of the Gymnosperms (pines, cycads, etc.) there is not a single representative.

Among the flowering plants the relative proportions of the various orders are very different from those in this country. The Compositæ (daisy family) numerically greatest in Great Britain, as in the world generally, is represented only by three or four species, none of which is common. The Leguminosæ

(pea family), the second family of the world, is not even represented, nor is the Orchidaceæ. The former is, of course, well represented in this country; the latter is more characteristic of the Tropics. Another interesting absentee is the Labiatæ (which includes the deadnettle, etc.). The order in Spitsbergen with the greatest number of species is the Graminaceæ (grass family), while the Caryophyllaceæ (pink family) and Cruciferæ (wallflower family) follow close behind. The most characteristic family of Spitsbergen, however, is the Saxifragaceæ, of which there are twelve species, several of which are extremely common. In Great Britain these saxifrages are nearly all restricted to the summits of Scotch mountains.

The flora, taken generally, is usually considered with the Arctic European flora. Almost all the Spitsbergen species occur in Arctic Europe, together with a great many other Arctic species. On Bear Island, which is smaller in area than Spitsbergen and more isolated, the flora, as would be expected, is much poorer in species, only about twenty-five to thirty species having been found hitherto. These figures for Spitsbergen and Bear Island are quite in agreement with the laws of chance and the general laws of migration of plants. It also suggests that the plants which are now in Spitsbergen did not come from Europe via Bear Island, but rather by way of Franz Josef Land and Novaya Zemlya. A peculiarity about the flora is the presence in eastern Spitsbergen of several Arctic American plants, which do not occur in the western part of the archipelago. No satisfactory explanation of this has yet been advanced.

The vegetation of Spitsbergen presents many interesting features and provides several important problems. In connection with both of these points, it is the way in which the plants are adapted to the peculiar conditions which is most instructive. The essential points of the climate for the plants are the long, severe winter, with its continuous darkness, allowing of no growth or development, and the very short, warm summer, continually light, when all the processes of the plants' activities are possible. Also the conditions during summer may not be altogether favourable to plant life, especially in rather exposed places. It is, therefore, necessary for the plant to sandwich into the three summer months all the work for which other plants in Great Britain have eight to nine months available. Special developments for this purpose are common and merit careful consideration. They will be dealt with rather more fully than are the other subjects already mentioned.

Perhaps the first set of adaptations to consider are those against the severity of the climate. Naturally, owing to the

thick snow-covering, the plants are protected during the winter from frost, wind, etc. In the summer, on the other hand, the plants are fully exposed to any gales, snowstorms, frost, and other violent atmospheric effects. There are various means of protection against these. One of the commonest, and one which is characteristic of many Spitsbergen plants, is the adoption of the "cushion" habit. The stems branch profusely and keep very short, while the leaves are borne closely together. A compact cushion is thus formed in which the branches mutually protect one another. Debris of various sorts collects in the interstices, adding thereby to the protective effect. The moss campion (*Silene acaulis*), several of the saxifrages, and the species of whitlow grass (*Draba*) are excellent examples of this. In some species, e.g. the Arctic willow, a further protection is afforded by the burying of the branches below the surface of the ground or in clumps of moss or lichen. It is noticeable that plants of a given species may adopt a cushion habit in exposed places and a creeping open type of growth in more sheltered spots. A further protection, especially against high winds or sand-blasts and moving substrata generally, is the production of very long, strong tap roots. This development is also found in situations in temperate countries where the substratum is mobile, e.g. sand dunes, shingle beaches, etc. In Spitsbergen, owing to the open nature of the plant covering, and the extensive denudation, such mobile substrata occur almost everywhere.

As a result of the strong winds and the continuous daylight, together with the absence of any shelter, transpiration (the passage of water-vapour out of the plant) is very high. Various modifications of the plant structure are developed to cope with this, and to reduce loss of water as much as possible. The "cushion" habit already mentioned is valuable in this connection. There are also rolled leaves, coverings of hairs to the leaves, and succulent leaves. Rolled leaves in which the stomata are enclosed within the roll are not well represented. The cross-leaved andromeda is almost the only representative at all common in Spitsbergen. Hairy leaves in which the transpiring surface is covered by a thick felt of hairs are rather more frequently met with. Species of Draba and chickweeds are good examples of this type of protection. Succulent plants, which, as is well known, grow in places where the transpiration is high, are represented by the rose root (*Sedum*) in Bear Island and the purple saxifrage. However, the succulence in Arctic plants is never so marked as in desert and steppe species, possibly owing to the freezing effect of the low temperature and the consequent destruction of watery tissue. The modifications mentioned above can all be observed in quite different

habitats in temperate and tropical regions. A second set of adaptations, however, those developed as a result of the very short summer, are not paralleled elsewhere, except, perhaps, in Alpine regions.

The first point in this connection is the well-known fact that Arctic plants, such as occur in Spitsbergen, can grow much more rapidly than can plants in temperate countries. The development of the vegetation in Spitsbergen immediately the snow disappears, and provided favourable conditions follow, is extraordinarily rapid. This is partly due to actual capability for quick growth, partly to the continuous daylight, which allows uninterrupted assimilation of food, and partly to the fact that everything is got ready beforehand, so to speak. On examining individual specimens of many perennial plants in Spitsbergen in October or May, it will be found that the flowers for the coming season are already in existence. They are in embryo, but are nevertheless perfect. All that is necessary in spring is an enlargement of the various parts. As a result of this the plant is able to produce flowers very early, without first having to store up a large amount of constructive material. This is so marked a feature in some species that one may find flowers of, for example, the purple saxifrage fully out at a distance of only a foot or so from the edge of the melting snow, the snow having melted from over the plants just a few days previously. Not less rapid is the production of seeds which may be found at the end of July or even earlier in favoured positions. This rapid development is without doubt much helped by the continual daylight, but can also be traced to some intrinsic quality of the plant. Tentative experiments seem to indicate that the same species growing in lower latitudes are not endowed with equal capabilities.

In Great Britain and similar countries it is a commonplace that the development of insects and the development of flowers are closely bound up with one another. Many flowers in this country depend on insects for their fertilisation. In Spitsbergen, however, there are practically no insects; as far as can be ascertained, none which is necessary for the fertilisation of the flowering plants. Flies may sometimes be seen on the purple saxifrage, otherwise insect visitors are extremely uncommon. Yet fruit is produced in abundance, as has been noted on many occasions. The only satisfactory solution to this problem is self-pollination; this is one of the means adopted by the plants in Spitsbergen in order to make up for the lack of insect life. Much research has shown that the majority of the plants are self-pollinated. Many have modified the usual order of development of the organs of the flower for this purpose. Another interesting point is that in some cases pol-

lination takes place before the flowers open; this is also a method of speeding up the life-cycle as well as affording protection to the delicate organs of fertilisation.

A second way in which the plant overcomes both the lack of insects and the unfavourable conditions is by not relying on flowers as a means of reproduction. The alternative is reproduction by vegetable means, in which a new plant is developed from some part of the stem, independently of flowers. A curious method is found in the flagellate saxifrage. At the base of the plant a number of runners, as in the strawberry, about two to four inches long are developed, at the tip of each of which is a small green fleshy bud. These buds take root in the ground, fresh plants being formed. The whole plant bears a quaint resemblance to a spider when viewed from above. In other plants many of the flowers are replaced by thick fleshy buds or bulbils. These fall off when ripe and generate young plants.

From the foregoing it can be seen that the plants in Spitsbergen, by means of various modifications, have been able to some extent to protect themselves from, and adapt themselves to, the severe and peculiar climatic conditions existing there.

Although Spitsbergen is essentially a country where nature is little modified by man's activities, yet it will be fitting to conclude this account with a reference to the work accomplished and being accomplished in the country by human beings. The first pursuits followed by man in the archipelago were all of the nature of hunting, usually from a commercial standpoint. Whaling was at one time a very prosperous and important industry, but has disappeared with the whales. Trapping, mostly carried out by Russians, was for a long time a profitable pursuit; it, too, has now sunk into insignificance. Another animal of economic importance in bygone years was the walrus, and a flourishing trade in walrus ivory was established. Indiscriminate killing has put an end to this also.

Spitsbergen has at different times been the scene of attempts to reach the Pole. The two most celebrated, perhaps, are those of Parry and Andrée. The former tried to sledge across the ice from the north of the archipelago. The latter, equally unsuccessful, but more unfortunate, attempted to reach the Pole in a balloon. He was never seen again. Since then the idea of reaching the Pole from the direction of Spitsbergen, at any rate by sea, has been abandoned as not being feasible.

The present interest in Spitsbergen is centred in the mineral wealth of the country. This was in part discovered at a very early date. It was not until about thirty years ago, however, that any of the minerals were worked on a commercial scale. The mineral occurring in largest quantity is undoubtedly coal.

It occurs in many places in the country, the largest deposits being around Icefjord. The coal, which is very variable in quality, is of different geological ages. The best coal, unlike that of this country, is the Tertiary coal. It is considered to be equal to Welsh steam coal for marine purposes. At present the total output is probably about 80,000 tons a year, but some of this is not of the best quality. With further development this output should be greatly augmented. There are many other minerals in Spitsbergen besides coal. Iron ore, of good quality, has been discovered ; gypsum occurs in large quantities near Temple Bay, while petroleum has been found, but work on it has not yet been commenced. Probably with further prospecting more minerals will be discovered. The value of the various deposits is enhanced by their proximity to good harbours and the ease with which the minerals can be shipped. A great future for Spitsbergen is assured should full advantage be taken of the mineral wealth known to exist.

With the taking over of the archipelago by Norway, it is to be hoped that the development of Spitsbergen will proceed apace, and that it will ultimately occupy in the world the position to which its natural resources entitle it.

POPULAR SCIENCE

SOME AFRICAN BUTTERFLIES

BY HERBERT MACE

I

LITTLE by little the secrets of the once " Dark Continent " are being given up, as European civilisation presses slowly down from the North or up from the South. Comparatively speaking, however, but little is yet known, for both nature and the human inhabitants seem determined to hold back their secrets till the last moment.

From the biological point of view, the Continent seems remarkably uniform, and has been consolidated for purposes of classification into one region—the Ethiopian. Not quite all of Africa comes under this denomination, however, for the barrier between this and the Palæarctic region is not, as one might at first sight suppose, the Mediterranean. Comparatively narrow even in its widest part and extremely so in its narrowest, it would scarcely prevent the frequent intermingling of forms from both its shores at the present time. Moreover, geologically speaking, it is quite recent and the real barrier between the two regions is the Great Desert, the one-time ocean, which lies to the south of the Mediterranean littoral countries and is passed only with difficulty by man, while the humbler forms of life find it a more effectual barrier than mountains or seas.

The northern coast, including Lower Egypt, has a butterfly fauna not greatly different from that of the Southern European countries, and it is only in Upper Egypt and the Sudan, where the Nile Valley forms a slender link between the two regions, that we come across characteristically Ethiopian forms.

South of this point there is, considering the enormous number of butterfly species which have already been found on the continent, wonderful uniformity of species, and very few forms indeed are identical with those familiar to the European butterfly collector. In such a vast area there are naturally species present in one country which are absent from another; but, just as the now extinct Large Copper, once confined to the

English fens, has a very closely allied relative, equally restricted in its range, in Central Europe, so the local forms of West Africa have intimate allies in South and East.

A very striking feature of the Ethiopian butterflies is the large number of species which take part in that mimicry which has such great bearing on the form and colour of species. This is accounted for by the abundance of the two families of *Danaidæ* and *Acræidæ*, almost totally absent from the Palæarctic region, which are notorious for the possession of powerful scent-glands which render them distasteful to birds. These protected species have been the cause of the most astounding modifications of colouring and even of form among butterflies of totally different families, this modification being rendered even more remarkable by the fact that often it is confined to the female. This is, of course, plausibly accounted for by the fact that the survival of the female is more important than that of the male. After pairing has taken place—often within a few hours of emergence from the pupa—nature has no further use for the males, but, in order to deposit her eggs to advantage, the female is obliged to stay on the wing much longer and to take many risks which are not necessary previous to pairing. The extreme of this necessity can be well seen in the case of the familiar *Vanessa* butterflies, such as the Common Tortoiseshell, which leaves the pupa in August or earlier. Pairing takes place almost immediately, but the eggs are not deposited until the following spring, so that for the remainder of the autumn and the long winter the female must manage to survive, if her progeny are to get a fair start in life. Nothing could more effectively explain why this modification of form is so often confined to the female, extraordinary though it appears at first sight.

II

In attempting to give some idea of the general character of African butterflies, I can scarcely do better than commence with these protected families, or, as many lepidopterists prefer to call them, subfamilies, *Danainæ* and *Acræinæ*. The *Acræinæ* are entirely unknown in the Palæarctic, only a few species are found in the Indo-Malayan, and one genus only is native to Tropical America. The *Danainæ* are not so exclusive. One species, indeed, the *Limnas chrysippus*, has a very wide range. It is one of the commonest of butterflies throughout Africa, is found plentifully in Southern Asia, and less commonly in Greece. Several others are found in the South of Asia and are, indeed, only less widely distributed there than in Africa.

The nearest relatives to these groups which are familiar to

the European collector are the *Nymphalinæ*, with which they agree in the primary characters. There are only four perfect legs in the imago, the front pair being aborted. The pupæ are also suspended by the tail. In *Danainæ*, however, the larvæ are smooth, with fleshy appendages, and in the perfect insect the cells of the wings are closed. In the males there are conspicuous patches of raised scales, sometimes in the form of lines on the forewings, but more generally as round patches on the disc of the hindwings. These scales cover the scent-producing organs, which, while primarily sexual in their office, have also some protective function.

Structurally, *Acræinæ* are chiefly noteworthy for the thick and diverging palpi, while the wings are remarkably clean, rounded, and rather long in outline. Many species have transparent wings, and a very unusual feature in butterflies is the possession by the females of many species, of a horny pouch at the end of the abdomen. The larvæ have branching spines, and, like those of the *Nymphalinæ*, feed gregariously.

Limnas chrysippus is, as I said before, one of the commonest of African butterflies, abounding in such distant places as the Gold Coast, Natal, and the Sudan. It expands from 80–85 mm., specimens from different parts of the Continent varying but little in size. In the typical form, the colour is brick-red, with black borders spotted with white. The tip of the forewings is black, crossed by a row of connected white spots, with some smaller ones near. The hindwings have, in the female, three, and in the male four black spots round the cell of the hindwings, the fourth being the largest and at the bottom.

This butterfly varies a great deal in colour, the most constant and striking form being that known as var. *Alcippus*, in which the hindwings are almost entirely white. My specimens of this variety are from the Gold Coast, where it appears to be the predominant form, while those from Natal have the dark hindwings. In the Sudan, however, all forms seem equally prevalent, and specimens I have recently received from my friend, Mr. B. W. Whitfeild, show an almost complete series, ranging from red to white, while some dark forms of the male have the large black spot surrounded by a white ring, the rest of the wing being reddish. The larva is bluish grey, with yellow longitudinal stripes and black transverse lines, while the filaments are black and red.

Quite a large company of butterflies and moths mimic this remarkable insect. The most striking is the female of *Hypolimnas misippus*, which, apart from structural characters, only differs by being generally slightly larger, and, instead of having four black spots on the hindwings, has one only, on the costa. Other very close mimics, some African, others Indian, are

Argynnis niphe, Euphœdra eleus, Elymnias undularis, Papilio merope, Caryatis phileta.

Amauris is a genus which, like *Limnas*, has numerous striking mimics, none more so than *A. niavius*. This is a large butterfly, very boldly marked in broad patches of black and white, which is abundant in West and South Africa. It is chiefly remarkable for the fact that, wherever it occurs, the female of *Papilio merope* assumes a special form closely resembling it. *P. merope* and its allies are widely distributed throughout Africa, including the island of Madagascar. The male is creamy yellow, the wings having black borders with one pale spot near the tip, while the hindwings have very prominent spatulate tails. In Malagasy and one or two East African forms the female is almost exactly like the male, the only noticeable difference in colour being the addition of a black bar on the costa of the forewings. On the rest of the mainland, however, no females resembling the male have ever been found, although the male never varies much in any district. Not only one, but several species of *Danainœ* are mimicked by this extraordinary lady. One, as I have indicated, conforms closely to the pattern and colouring of *A. niavius*, while another assumes the totally different hues of *L. chrysippus*. In no case do these females possess tails, and the only noticeable difference from the species mimicked is the dentated form of the hindwings.

III

Acrœinœ are perhaps the most abundant butterflies in Africa, taking the place of our familiar white butterflies in gardens and cultivated places. They bear a strong likeness to one another, so that, although several genera have been proposed in order to divide the enormous number of species into workable form, yet no very definite characters have been assigned to them, and the two genera *Planema* and *Acrœa*, the latter containing more than a hundred distinct species, may generally be regarded as the only permanent division of the African species of the subfamily.

Nymphalinœ, the great group of butterflies containing so many richly coloured species, even amongst Palæarctic forms, are extremely abundant in Africa, and, although the species do not generally surpass in beauty the richly coloured Admirals, Tortoiseshells, and Peacocks familiar to every English schoolboy, they display an almost limitless variety, which, while having certain points of resemblance to Palæarctic species, includes forms distinctly peculiar to the African Continent. A large and typical genus is *Precis*, which comprises butterflies having some general resemblance to the Tortoiseshells, but

the hindwings are produced at the anal angle into a peculiar point, which alone is usually sufficient to identify a species belonging to this genus. Another closely allied to *Precis* is *Junonia,* which also has the hindwings somewhat extended at the angle, but not to the same degree as *Precis,* while the markings in this genus generally include one or more conspicuous eye-spots reminiscent of the familiar Peacock, though scarcely equal to it in exquisite blend of colour. *Junonia* is less distinctly Ethiopian, numerous allied species being found in the Indo-Malayan region, while *Kallima,* most abundant in the latter region, and so striking on account of the amazing resemblance of the resting insects to leaves, has only one or two African representatives.

I have already mentioned *Hypolimnas misippus,* which is such a remarkable mimic of *L. chrysippus.* It has many allied species in Africa, all of which are handsome insects, the largest being *H. salmacis,* a common and widely distributed butterfly, expanding fully four inches, and having an intricate pattern of rich blue and white on a black ground. *Pseudacræa* is a closely allied genus containing species which, as the name implies, very closely mimic various species of the protected *Acræinæ.*

A very fine section of the subfamily is that which contains the genus *Charaxes,* large bi-tailed insects of peculiarly striking colour, one species of which, *C. jasius,* is found in South Europe and the Mediterranean region generally. Curiously enough, though it would appear probable that the European form has reached Europe via the Nile region, there is, at the present time, so far as has been discovered, a wide belt extending from Lower Egypt through the Sudan, in which no species of this genus is found, and the *C. jasius* has evidently been cut off at some remote period and modified out of close resemblance to the African species.

To deal at all adequately with this great and interesting subfamily is impossible within present limits, so, after mentioning the very typical African genus *Euphædra,* containing large species of singularly rich and varied colouring, in which a semi-metallic green is often prominent, I must pass on to the next subfamily, the *Satyrinæ,* familiar to the British collector as the somewhat dull-coloured "Browns" and "Heaths." They may be dismissed as having no very striking features. Though the typical European species are absent, there is no great disparity between them and the usual African forms, which are largely included in one genus, *Mycalesis,* practically all of which may be readily distinguished by having a large black eye with white pupil and surrounded by a pale ring, near the hinder angle of the forewings, while the hindwings have

two or more smaller eyes. Brown of varying shades is the prevailing colour, though there are one or two yellow and pale species.

IV

The *Lycænidæ*, commonly known as "blues," though many of them are not blue at all, are extremely numerous in Africa, and many genera are met which have striking features as compared with those of the Palæarctic region. At the same time, the generality of species in this family shows close affinity with those of other areas, the whole group being, in point of fact, very cosmopolitan. Thus, the familiar Tailed Blue, *Lampides bæticus*, is found throughout Africa, passing down the Nile Valley into Europe, where it is fairly widely distributed.

Of the genus *Thecla* there are no true representatives in Africa, but many closely allied genera. Very interesting indeed are the species in which the tails, more or less incipient in *Thecla* and other Palæarctic genera, are very strongly developed.

Passing to that family containing so many typically English butterflies, commonly called "Whites," the *Pieridæ*, we find that, although certain groups are common to both regions, the species which comprise them are totally different in Africa from those familiar in Europe. The common Cabbage butterflies of our gardens are replaced by multi-spotted forms often with a good deal of bright orange upon them. The group of typically Alpine butterflies known as *Colias* is also represented, but in rather curious fashion. There are several species distributed through the Continent, mostly somewhat smaller than typical *Colias*, and of the Pale type allied to the Pale Clouded Yellow. On the other hand, the Clouded Yellow, abundant throughout the greater part of Europe, has an almost identical African relative, *Colias electra*, differing only in its markings being slightly obscure and the whole surface suffused with a peculiar purplish lustre. It seems quite likely that the two species are identical, but have varied in opposite directions after being cut off from intercommunication.

The Orange tips, familiar to us by one indigenous and charming species, are very characteristic of Africa, though the European species is not found in the Ethiopian region. In the varied collection of African species grouped under the large genus *Teracolus*, the tip is not only orange, but in different species may be red, violet, or blue, while generally this patch is bounded inside as well as outwardly with a black bar. Interesting features of this group are some striking instances of seasonal dimorphism, well illustrated by specimens of *T. omphale* in the Natural History Museum at South Kensington.

A striking group somewhat allied to the familiar *Gonopteryx rhamni* of British butterflies is that of *Catopsilia*, remarkable for the possession by the males of a brush of silky hair at the base of the forewings and for the strongly arched costal margin. *C. florella* is an extremely common species, ranging almost all over the Continent right into Upper Egypt.

Other characteristic genera are *Leptosia*, in which the wings are peculiarly broad, rounded, and delicate, and *Terias*, mostly small species closely alike and all more or less intensely yellow or orange, with deep black margins.

V

That most generally admired family of butterflies, the *Papilionidæ*, or Swallow-tails, very scarce in the Palæarctic region, and in Britain represented by only one species, is extremely rich in African species, many of which are excelled in beauty and size only by the enormous *Ornithoptera* of the Malay region.

Although all the species are readily recognised as members of the family, the African forms may be divided into three groups. The first contains the boldly tailed species, of which the *P. merope*, already mentioned for its remarkable mimicry and *P. policenes* may be considered typical. The latter is a very richly coloured West African species banded in metallic green and black.

The next group, while retaining the characteristic shape and marking of the tailed species, has lost those appendages, which are replaced by dentated or crenated hindwings. In this group the commonest African *Papilio* may be placed. It is *Papilio* (*Orpheides*) *demoleus*, abundant throughout the Continent, and very handsomely decorated in a pattern of black and yellow with rich purplish eye-spots above and below. Specimens from all parts of the Continent show remarkable uniformity, except in size, those from the south being markedly larger than those taken on the West Coast.

In the third group the wings are not tailed or dentated and are much longer and narrower than in the typical species. In this group are some, such as *P. cynorta* and *P. similis*, which mimic certain species of *Danainæ* and *Acræinæ*.

In the *Hesperidæ* some very striking forms are met with fairly commonly, especially on the West Coast. Skippers of the normal European genera are found throughout Africa, one or two Mediterranean species passing up the Nile and being found unaltered and fairly freely in the Sudan. In the genus *Rhopalocampta*, however, some very fine skippers are included. *R. iphis*, for example, is almost three inches across and is

covered with exceedingly rich velvety plumage of deep metallic blue. *R. bixol* is rather smaller, but even brighter in colour, and *R. forestan* is a very distinctive species in brown, orange, and white.

Reviewing the whole series, it becomes strikingly manifest that the circumstances of Africa and its almost complete detachment from the Palæarctic and Indo-Malayan region have tended to isolate the butterfly fauna. I have before me a list of over seven hundred African butterflies, in looking through which I find only six species which have ever been taken in Europe. They are *Limnas chrysippus*, *Lampides bæticus*, *Tarucus telicanus*, *Zizera lysimon*, *Gegenes nostradamus*, and *Pyrameis cardui*, the remarkable butterfly which is, I believe, found all over the world except in South America, while, with the exception of the Australasian forms, which are somewhat distinct, it appears to be unaltered by any environment.

NOTES

Scientific Politics.—II. Labour and the Labour Party.

THE Labour Party is a new thing in politics, but labour itself is far older than politics, for without labour there can be neither politics nor civilisation. To discuss labour politics, therefore, without some reference to historical perspective would be confusing and misleading

It is often said, for example, by writers of one school that, without capital, labour could not live ; and by their opponents, that labour is itself the origin of capital. Obviously both doctrines cannot be true ; examination will show that neither is wholly true.

Capital in its potential form has always existed, and will always exist ; it is only another name for the resources of nature. But those resources can only be utilised by labour, which is common to all life. The difference, at least the fundamental difference, between animal and human labour is that the animal merely obtains what it needs for its sustenance, and no more ; human labour produces much more than the actual needs of the individual.

The whole of human civilisation is built upon that surplus. If the primitive agriculturist had only secured a living for himself and his dependents, and no more, there would have been no civilisation, because there would have been no surplus, and therefore no possible division of labour. Kings and popes would have followed the plough, and the Shakespeares and Newtons of the world have spent their lives in tilling the soil ; and most certainly there would have been no Labour Party, because urban labour could not have existed, since there would have been no cities and no urban industries. It is sometimes forgotten that the present industrial and urban age, in which eight men out of ten in England live in the great towns, was only rendered possible by the improved agriculture of the eighteenth century, and the increased agricultural resources of the nineteenth. Civilisation, like Napoleon's army, marches on its stomach.

There is a sense in which it may be said that only the peasant earns his own living ; for only the peasant grows his own food.

The rest of us live on the peasant, and render services in exchange. The justification of civilisation is that the services rendered have been, on the whole, greater than the value of the sustenance given; universally the world ranks its prophets, priests, and kings higher than its peasantry. The basic work of the world is done by rural labour, but this has always been taken for granted—by the Labour Party no less than by others. The labour leader may be "a son of toil," but very rarely is he a son of the soil. Almost invariably he is a townsman, who knows little of the country, and who unconsciously adopts the townsman's attitude that the business of the countryman is to feed the town as cheaply as possible.

If civilisation has been built on the surplus product of labour, many of the troubles of civilisation have sprung from disputes over the division of the surplus. In many instances the problem was settled in summary fashion. In the typical slave-state the whole of the surplus was taken by the master, after he had fed his slaves. On that basis many ancient civilisations were founded, notably that of Greece; and on the ground of pure economics there is perhaps little to be said against a civilisation based on slavery. It produces the goods; it secures the producers against unemployment and privation, and, therefore, relieves them of most of the anxieties of life; and, under any reasonable authority, it provides for manumission and does not forbid the exceptional slave the "career open to the talents." Epictetus is only one instance of many a slave who rose to greatness and honour.

It is our ethical, not our economic sense, that has revolted against slavery; in the long run the world has preferred freedom to security, even if freedom meant unemployment and the chance of starvation. We have indeed paid a long price for freedom, but few will argue that the boon has not been worth the cost.

The Labour Party is the creation of the new industry. It was because the capitalist secured most of the gains, and labour suffered most of the losses, at the outset of the new industrial age that the trade unions came into being, and in time gave birth to the Labour Party. Sixty years ago the classical economists spoke of the "freedom of contract" between master and man; but the freedom was a fiction, and labour knew it. In the rebound from Mill it adopted Marx, to whom capital was the enemy, and the workman, "who had nothing to lose but his chains," was the wage-slave of capital. Marx has been the old, and John Ruskin the kindlier new testament of political Labour; perhaps Mr. Webb is entitled to rank as its Athanasius. But this appears to be only a passing phase; some doubts of the plenary inspiration of all three writers are

now betrayed by the younger school of Labour—which, however, still waits for an original thinker who shall construct an economic system to its satisfaction.

But from the first Labour in Britain (and in the United States also) has been markedly different from the Continental movement. The Syndicalists in France, the Revolutionary Socialists in Germany and Italy, and the Bolsheviks in Russia have indeed produced reactions in the Labour Party in this country, and some of their doctrines—the general strike, direct action, the dictatorship of the proletariat—have been praised and imitated by recognised British Labour leaders. But none of these doctrines has prevailed.

British Labour does not concern itself much with doctrines until they involve action; it prefers practice to theory. And in practice the general strike has broken down in this and other countries. Direct action has been rejected as unconstitutional after several sporadic attempts. The dictatorship of the proletariat remained a mere catch-phrase until Lenin proceeded to translate it into fact. The moment that fact was seen to involve the conscription of labour by the Government, British Labour would have no more to do with Bolshevism.

Theologians declare that there is one unforgivable sin. As its nature is nowhere defined by authority, interpretations differ as to its nature; but, to Englishmen, the one unforgivable sin is to oppose liberty. Labour has often charged that sin against Capital, but, when it discovered that the very system in Russia which had abolished Capital stood for industrial slavery, it parted company with Communism. Silently, but without hesitation or doubt, it rejected Communism for ever.

The fact is profoundly significant. There are many revolutionaries in British Labour, but Labour is not revolutionary. The revolutionaries are at the bottom of the movement, the constitutionalists at the top. It is a source both of weakness and strength in Labour politics. The extremist attracts the young with the promise of a new heaven and a new earth; the constitutionalist, with his belief in ordered development, reform, and progress, attracts the older and more thoughtful. The weakness of Labour is that the extremist frightens as many as he attracts, and is naturally taken as typical by political and other enemies of the movement, while the constitutionalist has to fight for his life against the extremist. Every politician is prepared to be attacked to his face by his enemies. But to be kicked in the back by his friends is an addition that makes neither for comfort nor sound constructive thought. It is not even conducive to progress.

Labour has externally a stricter discipline and a greater

unity than other political parties. But internally it is less united than its rivals, and many of its difficulties spring from acute differences which are not publicly debated. It is often easier to pass a meaningless resolution for which every delegate to a congress can vote than to fight out a controversial issue on a platform; but this method of compromise leads to woolliness of thought and a tendency to shirk realities. Labour has not yet the same debating ability as the older parties, and its deficiencies in this respect have done it harm in the present Parliament. It has doubled its numbers; it has certainly not doubled its effective weight. It has attracted the respectable; it has not yet produced a leader of genius.

These troubles may disappear in time, for Labour is still only in its initial stage in politics, and its organisation is not complete. Its eventual position, however, must remain uncertain for another and a deeper reason.

In the psychology of man, and consequently in his politics, there are two fundamental and contrasted elements—liberty and order, which correspond roughly to the dynamic and static elements in nature. Of the old historic parties in this country, Liberalism has usually been more identified with liberty, and Conservatism with order. Labour has attempted to escape the antilogy by organising itself on a class-basis; but this has admittedly been found too narrow and is breaking down, since Labour now appeals to the middle-class intellectual, the "brain-worker," and the "black-coat," to vote for it. The chances of politics—or, rather, the convenience of a party feud—may revive the class-basis for a time, but in a country like England class-consciousness has never been a decisive, and it now seems a diminishing, factor. It is not that there are no classes, but that there are too many; the sharpness of the divisions is blurred by the number of sub-divisions. Labour itself has its own class-divisions no less than the rest of the nation.

The issue of a class-fight may, therefore, be dismissed. It is a piece of occasional tactics; no more. The conflict with Capital may last longer, either as a real issue or a platform phrase; but Capital is as permanent as Labour, and the thoughtful Labour leaders smile at the crude idea of "abolishing" Capital. What they want is a working agreement, a co-operation between the two, in which Labour meets Capital on equal terms.

But these things are economics, and therefore secondary and impermanent; neither Capital nor Labour in the current sense of the words is two hundred years old, and both are obviously changing before our eyes. On the other hand, the question of liberty or order is primary and permanent. And on this Labour has manifestly not yet made up its mind. Its natural instinct, like that of most Englishmen, is towards

liberty; its policy, which envisages regulation, control, official sanctions and State action, is towards order. It demands political liberty and economic security; but history shows that the latter has been purchased at the price of the former. The slave-State gives security; in the free State the individual takes the risk. Labour has not decided whether, in the last resort, it is for the individual or the State.

The Chesterton-Belloc school have coined the phrase, "the servile State," to describe a certain type of modern legislation. It is precisely that legislation which the Labour Party approves. Yet it would repudiate the label with indignation. It advocates nationalisation, but denounces bureaucracy; unluckily, it has not yet made it clear how it proposes to secure the one without the other.

The fact is, that political Labour contains both Liberal and Conservative elements, and the balance of power sways now in one direction, now in another. It believes in State railways, but not in a State Church; it denounces privilege, but supports the Trade Disputes Act; it suspects combinations of Capital as instruments of tyranny and a danger to the State, but sees no danger in precisely similar combinations of Labour.

Every political party, it is true, commits inconsistencies. Liberals have resorted to coercion, Unionists have passed the Home Rule Act, Conservatives are arguing in favour of reforming the House of Lords. But these inconsistencies are accidents of office or opposition, not defects in the underlying philosophy of the group. In the case of Labour the inconsistencies seem to be an indication of incomplete development. The party has yet to test its fundamental principles in their actual working.

Rewards.

In our fair land, where'er the eyes
Can range the open scape and skies,
A hundred beauteous mansions rise;
Whose turrets to the seeing Sun
Flash back his beams when day is done;
Whose oaken floors can scarce endure
Their lordly load of furniture;
Whose swarded lawns and gardens trim
Are laid to flatter every whim.
"Here dwell," the Stranger cries, elate,
"The men who made this Britain great
In Science, Wisdom, Art and State.
This house is doubtless that of one
Who hath some superservice done.
And that, and that—say, theirs who find
Great benefits for all mankind,

With toil infinite, endless pain,
The racking labour of the brain;
The knowledge that our age endows;
The wisdom vested in our brows;
Who teach us how to weigh the star
Or harness nature to the car,
To handle lightning, hold the fire,
To tame the tempest's feeble ire,
To drive the skulking sickness hence
And curb the murd'rous pestilence.
There dwell the men who mould our tho't
In beauteous phrases justly wro't;
Who give experience without pain
In tales to teach or entertain;
Who make the music that's divine
To strengthen, gladden and refine;
Who feast the eyes and lift the heart
With labour'd gems of perfect art;
Who give us all that makes us great
Above the prime barbaric state.
No wonder Britain rules us when
She honours thus her greatest men."

"Oh no," the local swain replies;
"There lives a man who makes pork-pies;
In that there house, so folk aver,
A noble Jewish usurer;
In yonder one a mighty peer
Who brews from chemicals our beer;
A politician, lawyer, quack
Lives there, and there; and further back
A smart municipal contractor,
A welcher, and a comic actor."

So goes the tale; and such the gods
Who dwell in Britain's blest abodes.
And where are they who bless? Unknown
Each toils in silence and alone;
His highest glory, to have none;
His widest fame, to be unknown;
His greatest riches, to be poor;
His keenest pleasure, to endure.
For mark the law that underlies—
By work alone one cannot rise;
He wins no wealth who merely toils;
The idle schemer takes the spoils.
Who stands upright in Britain falls.
He wins the prize of life who crawls.

How to Spend Money on Research.

In February last the newspapers announced that Lord Atholstan had offered a prize of one hundred thousand dollars for the discovery of the cure of cancer, and, later, that Sir William Veno had supplemented this by an additional ten thousand pounds. Shortly afterwards the Earl of Athlone appealed in the Press in favour of this money being given to a special cancer research which is being conducted in one of the large London Hospitals. The results were that Sir William Veno changed his mind and gave his money to this institution instead of offering it as an open prize; and that Lord Atholstan promised to give another hundred thousand dollars for institutional cancer research, but, very wisely, maintained his original offer of the same amount as a prize.

Of course all kinds of research should be encouraged as much as possible by monetary grants. All kinds are almost sure to give some results. The cancer problem is an exceedingly difficult one, which has consistently foiled the best workers for years. But the disease is so terrible and often so hopeless that mankind should now make every possible effort to conquer it; and whether we give money in one way or another does not matter so much, as long as we give it at all. Nevertheless, we think that research may be divided into two kinds, namely Individualistic Research and Institutional Research, and that the former is the more likely to lead to success. An immense amount of institutional research on cancer is now being done everywhere—in universities, in laboratories, in several cancer institutions, and in hospitals. Almost any promising student can set up his microscope in such places, and can even obtain grants of scholarships from a number of bodies which are specially endowed for this purpose. We do not know how many people are now working in this way, and what is the total aggregate of the money which they receive as salaries or use for expenses—it is probably a much greater sum than Lord Atholstan has now offered for a prize—and yet progress is not so fast as we could wish. We do not blame the workers—they are probably doing their best. But many of them are young men who cannot possibly afford to continue in research work all their lives, and many do the work in order to obtain diplomas and professorships, or merely as an occupation until they are old enough to begin medical practice. On reading the history of medicine we fear that this kind of work seldom leads to any final victory. Nearly all the great medical discoveries have been made by men who have devoted themselves to their task in spite of professional loss, and with little chance of reward even if they be successful. The world does not understand that the greatest medical discoveries may ruin, and not remunerate, the discoverer. Jenner's long research on vaccination destroyed his practice, and Robert Koch was for a time impoverished by his labours on tuberculosis, though both were rewarded later by their respective countries. We maintain that the offer of large prizes for results achieved, and not the giving of small grants for results expected, is the proper way to stimulate men of this kind.

The popular idea appears to be that the man of genius is necessarily a fool who must inevitably sacrifice himself in pursuit of his dreams. We do not think so: many men of genius are wise enough to be turned away by the prospect that their work will lead to the garret. But the meanest argument we have ever heard in this connection is that, as the man of genius is compelled to work by his genius, so, therefore, the world may safely utilise his labours without attempting to pay him! The most despicable form of of ingratitude, both in States and in individuals, is to profit by the altruism of poor men.

Lord Atholstan is wisely trying to remove this reproach in the case of cancer research. Money offered in the form of *large* prizes stimulates many more real workers, and rewards more really deserving men, than that which is frittered away in the form of research-subsidies to a number of people who will never achieve anything—*verbum sapientis*.

Civis Britannicus Sum.

On the 7th December 1921 Sir Rider Haggard, in giving away the prizes at the Hastings Grammar School, made the following remarks with which all boys nowadays ought to become acquainted. He said :

"When they had grown up they would have to exercise a duty towards their country. To his mind there had been a considerable loss of sense of that duty during late years. In the old days citizenship was highly prized. 'Civis Romanus Sum' was the proudest boast a Roman could make, and they would remember also that St. Paul spoke of himself as a citizen of no mean city; but recently there had been a tendency to deprecate the functions and status of citizenship. He was sorry to say that there was springing up in their midst a school of internationalists which decried all loyalty, and said that patriotism was a curse. No true citizen would ever take that view. He asked them to think what it meant to be a citizen of our Empire; he had been round the Empire several times, and knew what it meant to be a citizen of the British Empire—that great community of nations which acknowledged one head, the King. As such a citizen they had many rights and glorious privileges, but they also had duties, and they must always bear in mind that each of them was one of a body co-operating together. They did not live to themselves alone. What was the true object of all education? It was not merely book learning, but the drawing-out of character. That was the object of education, and it was character above everything else which would make them good citizens of the future. He maintained that the principle of 'propaganda' was evil in many ways, for it treated a nation as a flock of silly sheep, to be led by pictures, startling headlines, etc. He urged them not to be driven in life, but to drive themselves towards some definite aim. If they were driven, no matter by whom, in the end it would mean trouble. The result of the driving of a community was to be seen in Russia to-day. They must form their own opinions, cultivate their convictions, and change them only through the light of experience, not through prejudice. He urged them to remember that sixty million 'yous' made up the Empire. They must not be slaves of newspapers or politicians, but think for themselves. Thousands of voices would call them in all ways, but he urged them not to trust any of them until they had found their worth. They wanted sound, reflecting men and women in the dangerous days they had to face, and they must always bear in mind that the future of England depended upon them collectively, and so they must act as if it depended upon them individually. The opportunities open to them were vast, and everything was to be won by work and effort. They must remember that the greatest privilege which destiny could give them was to be born a citizen of the British Empire, and to be able to say proudly, 'Civis Britannicus Sum.' The greatest prize any of them could have, when at last their work was done, would be to be able to say and know that their lives had justified that great description."

From the *Hastings Observer*.

Life-Saving.

Mr. Murray has recently produced the second edition of Dr. Malcolm Watson's book *The Prevention of Malaria in the Federated Malay States, A Record of Twenty Years' Progress*. It is indeed a magnificent record. The author estimates that something like a hundred thousand lives have been saved in the Malay Peninsula by sanitary improvements, especially those connected with anti-malaria work. Not only have the lives been saved, but the land has been often greatly improved for agricultural purposes and the prosperity of the numerous rubber estates has been greatly enhanced. Seldom has a finer sanitary achievement been accomplished and, when we remember the great difficulties attending the work in the Federated Malay States, we must agree that the achievement is not second even to the achieve-

ment of the Americans in Havana and Panama. If all British colonies and possessions had shown equal interest in such work since 1899, similar results would doubtless have been attained almost throughout the malarious parts of the British Empire. Unfortunately the same scepticism, the same ignorance, the same jealousy which has impeded all great advances have impeded anti-malaria work everywhere; and some of the opposition shown probably gives examples of the most utter stupidity ever seen. It is amusing, as an example of our administration, that many of the men who have done the best work in this line have not received any recognition for it whatever, while those who have thwarted every step now sit in the seats of the mighty.

Northern Numbers.

Probably men of science, like most other men, always feel inclined to take up their hat and stick when poetry comes on the stage or the film. One reason for this is the very wide prevalence of professional poetry. We all know beforehand what it is going to be like—the usual torturing of words into strange meanings, the absence of experience either in life or in thought, evidence of what some may call Wordsworthian mediocrity trying to get its flat feet into the winged shoes of genius, the under-flavour of incorrect academical theories of art, the wild desire to be original in order to attract attention, and lastly the usual absence of anything to write about. Perhaps, therefore, the man in the street may pause for a moment to look at special anthologies in which the professional element is not so visible, and the excellent title of "Northern Numbers" (T. M. Fouliss, London, Edinburgh, etc.) may possibly tempt even readers of SCIENCE PROGRESS. We cannot see, however, that there is much of the Doric style in these verses—it may comfort some to know this: the language is mostly simple English-Scotch or Scotch-English, so that most of the sense is intelligible even to the poor Southerner. Attention will be specially attracted by such an author as General Sir Ian Hamilton, who is experienced indeed, and we want to know what he says in verse, especially when we find his verse to be admirable. Both of his two pieces, the sonorous and moving lines on Gordon and his *Night at Hafton*, are very good; and the latter, which describes his experiences when he was a little boy and was sent to bed alone through the corridors of a great dark mansion into his lonely and distant bedroom, tell exactly what many children less brave than he once endured. The man of intelligence might prefer the poetry of a General even to better poetry from Grub Street; but, as a matter of fact, he will in this case have to travel far before he can get better poetry. None better than that of John Buchan is often seen, and the soliloquy of the monk Lapidarius is very fine stuff, with a touch of real psychology. The Editor, Mr. C. M. Grieve, tends to be more modern, but one of his sonnets on the highland hills, namely that on Schehallion, is really prime stuff. There are some beautiful bits of music, especially the sonnet on the Pewitt, by Donald A. Mackenzie, and the Raiders, by W. H. Olgivie; and Lewis Spence's Haschish and sonnet on Holofernes are fine bits of invention.

Notes and News.

The New Year Honours List included the following names: Professor C. S. Sherrington, Pres. R.S.; Professor Herdman, Professor G. E. Cory (of the Rhodes University College, Grahamstown), and Dr. J. H. Parsons, F.R.S. The first-named was appointed a Knight Grand Cross of the Order of the British Empire (G.B.E.) and Knighthoods were conferred on the others.

The Nobel Peace Prize for 1921 was awarded to Dr. Elis Stroemgren, professor of astronomy at the University of Copenhagen, for his efforts to effect reconciliation among the scholars of European countries.

The list of candidates selected by the Council of the Royal Society for election into the Society this year is as follows: T. H. Bryce (Professor of Anatomy, University of Glasgow); C. G. Darwin (Mathematician, Lecturer and Fellow, Christ's College, Cambridge); C. G. Douglas (Physiologist, Fellow, St. John's College, Oxford); S. R. Douglas (Director, Bacteriological Department, Medical Research Council); A. J. Ewart (Professor of Botany, Melbourne University); A. Hutchinson (Demonstrator in Mineralogy, University, Cambridge); F. W. Lanchester (Advisory Committee for Aeronautics); J. Mercer (Mathematician, Fellow and Lecturer, Christ's College, Cambridge); S. R. Milner (Professor of Physics, University, Sheffield); M. S. Pembrey (Professor of Physiology, Guy's Hospital, London); F. L. Pyman (Professor of Technological Chemistry, University, Manchester); G. A. Schott (Professor of Applied Mathematics, Aberystwyth); N. V. Sidgwick (Demonstrator in Chemistry, University, Oxford); D. M. S. Watson (Lecturer in Vertebrate Palæontology, University College, London); Sir A. F. Yarrow (Marine Engineer).

Professor E. G. Coker has received the Telford medal of the Institution of Civil Engineers for the session 1918–1919, and Professor F. C. Lea a Crampton prize for the session 1919–1920. These awards have both been delayed owing to the war.

The council of the Geological Society has made the following awards for the year 1921: Wollaston medal, Dr. A Harker; Murchison medal, Dr. J. W. Evans; Lyell medal, Dr. C. Davison; Wollaston fund, Dr. L. T. Wills; Murchison fund, Mr. H. Bolton; Lyell fund, Mr. A. Macconochie and Mr. D. Tait.

Dr. Charles Singer, Lecturer in the History of Medicine in the University of London, University College, will preside over the Third International Congress of the History of Medicine to be held in London in July next.

We have noted with regret the announcement of the death of the following well-known scientific men during the past quarter: H. Bourget, Director of the Marseilles Observatory; Dr. G. S. Brady, F.R.S., Professor of Natural History, Armstrong College; Lord Bryce, O.M.; Dr. T. A. Chapman, entomologist; Sir William Christie, F.R.S., Astronomer Royal 1881–1910; Professor J. H. Cotterill, F.R.S., of the Royal Naval College, Greenwich; Professor W. Foord-Kelcey, Professor of Mathematics at the Royal Naval Academy, Woolwich; Dr. Edward Hopkinson, M.P., electrical engineer; Sir John Kirk, G.C.M., K.C.B., F.R.S., explorer and botanist; H. R. A. Oertling, the balance maker; Professor P. Thompson, Professor of Anatomy in the University of Birmingham; Sir G. Sims Woodhead, Professor of Pathology in the University of Cambridge.

Sir David Prain has retired from the directorship of the Royal Botanic Gardens, Kew, under the age-limit regulation. His successor is Dr. A. W. Hill, the Assistant Director.

Several expeditions have been arranged for observing the total eclipse of the sun which will occur on September 21st. The most important is that commissioned by the Joint Permanent Eclipse Committee of the Royal Society and the Royal Astronomical Society, which consists of our contributor Mr. H. Spencer-Jones, Chief Assistant at the Royal Observatory, Greenwich, and Mr. P. J. Melotte, the discoverer of the eighth satellite of Jupiter. This party has already left England for Christmas Island—an isolated island nine miles broad and twelve long, which lies to the south of Java. The chief work of the expedition will be, of course, to obtain further data bearing on the Einstein deflection of light rays passing near the sun; but, in addition, an extensive photographic survey has been planned, and it is hoped to start this work in May. A joint Dutch and German expedition, including Dr. Voute, of Batavia, and Professor Freundlich, of Potsdam, is also to station itself on Christmas Island, while Mr. J. Evershed, Director of the Kodaikanal Observa-

tory, India, is going to the Maldive Islands, where totality lasts 4 min. 10 sec. as against 3 min. 42 sec. at Christmas Island, but with the sun only 34° above the horizon. Finally, an Australian expedition may observe from Cunnamulla, South Queensland, the terminus of a railway from Brisbane.

We have received the second edition of Sir J. J. Thomson's *Rays of Positive Electricity and their Application to Chemical Analysis* from Messrs. Longmans, Green & Co. (price 16s. net.), and the third edition of J. A. Crowther's *Ions, Electrons and Ionising Radiations* from Arnold & Co. (price 12s. 6d. net). The first has been considerably extended by the inclusion of an account of the advances made since 1913—notably, of course, by the work of Dr. Aston; while Dr. Crowther has made additions in the sections devoted to the X-ray spectra of the elements, and the theory of isotopes. Further, the work of Rutherford during the last two years has set the nuclear theory of the atom on a firm basis, and the whole book is now arranged from this point of view. The fact that three editions of his book have been printed since August 1919 is a sufficient indication of the usefulness of Dr. Crowther's work, and with the two books lying side by side on the table one is forced to comment on the excellent way in which Messrs. Arnold have done their part in its production.

We should like to draw attention to the *Survey of the Present Status of the Atomic Structure Problem*, by David L. Webster and Leigh Page, published by the *National Research Council*, Washington, D.C. (*Bulletin No.* 14). Written largely from the view-point of the Lewis-Langmuir theory of the arrangement of the electrons, it contains an admirable account of the latest speculations concerning the structure and dynamics of the atom and, being issued at 75 cents, will probably be welcomed by those who have resolved not to purchase books of this ephemeral character at the prices demanded by English publishers.

The second memorandum on the production of artificial fuel for motor transport, issued by the Fuel Research Board, will not make satisfactory reading for the multitude who hoped that the costs of motor transport might be lessened by a vegetable fuel. The Board concludes that the prospects of adding to our supplies of liquid fuel by the manufacture of alcohol from materials grown in this country are very remote. Further, that the production within the Empire of alcohol from materials containing sugar or starch is only likely to be possible commercially in the near future in some of the Dominions and Colonies, and then only on a sufficient scale to meet local requirements. Such production could be accomplished owing to—

(*a*) The availability of molasses, now a waste product in many places. (*b*) The possibility of growing vegetable substances giving a high yield per acre combined with a relatively high starch content, such as cassava, sweet potatoes and yams. (*c*) The comparatively cheap labour and production costs. (*d*) The high cost and scarcity of other liquid fuels. (*e*) The relatively small liquid fuel requirements. It is unlikely that alcohol could be produced in the manner referred to above in excess of local needs and at a price, when freight to seaboard and to this country is included, at which it would find a market here.

Synthetic production on a commercial scale in this country is also unlikely; it might be possible, however, in Canada and Australia. Finally, research work for a process, either chemical or bacteriological, to produce alcohol commercially from tropical vegetation or waste vegetable materials has not yet reached the stage where its possible industrial application can be practically considered. The discovery of such a process, where locally applicable, probably offers the best chance of a large-scale production of power alcohol for export.

Bulletin No. 5 of the Department of Scientific and Industrial Research contains an account of the work carried out by Dr. Alexander Scott, F.R.S.,

on the cleaning and restoration of museum exhibits. Dr. Scott spent some eighteen months working in a temporary laboratory at the British Museum, testing methods for dealing with a great variety of exhibits, including prints, enamels, articles of silver, lead, iron, and copper, and lichen-covered prehistoric rock paintings. Among some of the methods he describes may be mentioned the application of pure hydrogen peroxide to lead paints blackened by the action of the sulphuretted hydrogen in the atmosphere, by suspending the blackened surface about an eighth of an inch above a block of plaster of Paris which has been sprinkled as evenly as possible with a concentrated solution of the peroxide. The action of the vapour is, of course, slow and the exposure should last for some hours. Another neat device is that for the preservation of cracked enamels by keeping them in an oil-pump vacuum for about half an hour, covering them with a 10 per cent. solution of dried Canada balsam in benzol, and finally letting in the air so as to force the balsam into the cracks. The enamel is then left in a vertical position to drain and dry.

We have already referred in these notes to the work of Dr. Margaret Fishenden on the radiant efficiency of various kinds of domestic grates. *Technical Paper No.* 3 of the Fuel Research Board describes the further experiments intended to test the usefulness of the low temperature carbonisation coke cakes from the Greenwich Fuel Research Station for sitting-room fires and various types of kitchen ranges. The experimental methods were: (1) to determine with a Richmond radiometer the radiant heat passing through a surface 12 in. square opposite the centre front of the fire and 34·4 in. away; (2) to compare, with a thermopile, the radiation over a hemisphere of 34·4 in. radius round the centre of the fire. These observations permit the total amount of radiant heat to be measured. The ratio of this total to the calorific value of the coal burnt gives the "Radiation Efficiency" of the fire. As before, the old-fashioned register grate *with the space beneath the bars boxed in by a fender with adjustable doors and a plate flue damper* proved itself more efficient than any of the modern barless grates which were tested, and this both for coal (efficiency 24·2 per cent.) and coke cakes (30·8 per cent.). Further, the coke cakes in every case gave better results than coal. The experiments on kitchen ranges lead to the conclusion that it is impossible to design a range which will perform the three entirely separate functions of water heating, oven heating, and room heating simultaneously without a serious diminution in the efficacy of all three. We recommend this report to all those who are interested in the domestic heating problem, especially if they are thinking of alterations in their own homes. It may be obtained from H.M. Stationery Office, Imperial House, Kingsway, W.C.2, price 9*d.*

We have now received the full report on Heat Insulators by the Engineering Committee of the Food Investigation Board. An abstract of the results has already been given from the pages of the annual report of the Board. A study of the full data now published shows that cork, slagwool, charcoal and wood fibres, when of good quality and dry, have practically the same thermal conductivity, *viz.*: 0·00011 cal. per sec. per cm. per 1° C. This suggests that the resistance these substances offer to thermal conduction is not very far from the limiting value that is practically obtainable by subdividing the air space. (The thermal conductivity of still air is 0·00005 c.g.s. units.) The only substance to give better insulation than these is rubber expanded by gas into a highly cellular form (density 0·06 to 0·12 gm. per c.c.) by vulcanisation under a high pressure (*e.g.* 100 atmospheres), which is gradually released in the cooling down stage so that the gas distributed through the rubber expands and gives it an unbroken cellular structure. The conductivity of this material is about 0·000085 c.g.s. units. An appendix to the Report contains a detailed account of an ingenious calorimeter for finding the specific heat of insulating materials which cannot be heated to 100° C. on account of the resulting alteration of

their moisture content. They are crumbled and heated electrically in a rotating calorimeter. The motion of the calorimeter (which is only about one-third filled with the solid) brings the finely divided solid into contact both with the heating coils and the thermocouples which measure its change of temperature.

Those of our readers who were attracted by the Draysonian ideas referred to in an article in this journal three years ago (April 1919) will be interested in an article by Dr. E. O. Fountain in the *Journal of the British Astronomical Association* (vol. xxxii, No. 3, 1921–1922) pointing out the fallacies upon which the theory is based.

It has long been felt that an important aspect of the great problem of the conservation of the national coal resources involves the study and classification of the coal seams which are at present being worked or developed, and also of seams or portions of seams which are being left unworked or are thrown aside above or below ground. This study and classification on its directly practical side must deal primarily with the suitability of each particular coal for those purposes for which its individual qualities render it most adequate, *e.g.* for gas making, coke making, steam raising, or for domestic use.

This question of survey has for some years been receiving the anxious consideration of the Fuel Research Board, but the unstable conditions which prevailed in the coal industry during and since the war have necessarily led to the postponement of the work of organisation. It is now, however, considered that the time has arrived when a beginning can wisely be made.

The Fuel Research Board believe that this work can be most effectively carried out with the help of local Committees in which colliery owners, managers, and consumers are associated with the representatives of the Fuel Research Board and the Geological Survey. By this combination not only will local knowledge and experience be made available, but the initiative of those most deeply interested in the practical aspects of this survey will be secured.

The survey work will thus from the outset assume a practical character, for the selection of seams for examination will be in the hands of those who are in the best position to estimate the relative importance of the problems awaiting solution. The selected seams will be submitted to physical and chemical examination by the local experts; and, as a result of this examination, a further selection will be made of those which appear to justify experiments on a practical scale, to test their suitability for particular uses or methods of treatment. This experimental work will be carried out either at H.M. Fuel Research Station, or at other works, as may be found most convenient.

The first Committee is already actively at work in the Lancashire and Cheshire District, where the local Research Association has been recognised by the Fuel Research Board as its representative body for the purpose. It is felt by the Fuel Research Board that the experience gained in the work and organisation of this Committee will be of great value in the establishment of Committees in other districts when the time is ripe for further developments, but they are satisfied that it will be wise to build up this national organisation on the sure foundations of actual experience.

ESSAYS

THE EINSTEIN THEORY OF RELATIVITY (D. Laugharne-Thornton, M.A.).

THE Newtonian conceptions of physical phenomena have been considered as of universal validity for so long a period that the introduction of the Einstein theory of Relativity is regarded by some students of science as an instrument of destruction towards the classical mechanics. This view, in itself, is a natural reaction and of but passing moment; there is a more serious form of this reaction, and this is the tendency to regard this new interpretation as an idea which is to be resisted. This last-mentioned view-point is a thing to be deplored. For if the doctrine of evolution itself forbids one to admit any transcendental source of knowledge, then such a thing as a *universal* truth is merely a fictitious picture of the human brain. Bearing this in mind, we see the futility of the assertion that Einstein's work has taken away any of the brilliance which is shown in Newton's monumental labours in the world of physics. One may say that what Vesalius was to Galen, what Lobatchewsky was to Euclid, that is Einstein to Newton. In short, the new Relativity comprises the old, but, in addition, it includes considerations which are not part of Newtonian Relativity.

It may be well to remember that the classical mechanics brought together many of the natural forces and their implications, but failed to include in its scheme of centralisation the forces attributed to gravitation. These latter forces were treated as constant, and, as such, it appeared that they could be added to the net result of all the other forces. As a contrast, we see that the new conception of natural phenomena has accounted for all the attributes of gravitation as a function of the other forces, and thus we may say that, what projective geometry has done for geometry, the same "focusing" effect has been done by the new relativity for the science of mechanics.

Great difficulties are encountered by many students in studying this new interpretation of facts, this in a large measure being due to the philosophical views held in the past regarding the material structure of the universe. From Mill's work, one may define matter as a permanent possibility of sensation, but in so doing we are apt to forget that the spectator of nature observes discrete quantities while the brain makes out a continuous phenomenon of such a group of facts. Further, any system of inference in science depends on the uniformity of the natural facts concerned, and thus we see that between the observed fact as comprehended and the natural state of nature a complex path of reasoning and interpretation intervenes. It is in this mental passage that much difficulty manifests itself to the reader of Einstein's work.

From the above line of thought it follows that the knowledge of an exact law in the theoretical sense would be equivalent to an infinite observation. By this we do not mean that such a knowledge is impossible to man; but we do say that it would be absolutely different in form from any knowledge that we possess at the present time. As a compromise to an infinite investigation, which is impossible, we shall later see that a process of taking average values is carried out, and that within certain limits this process is justifiable. The study of the "quantum theory," which involves, *ipso facto*, the science of

discrete number, while the science of quantity is founded on the totally different hypothesis of continuity, is an example of the question we are now considering. This involves an important point, for physical science is concerned with quantity, and this condition necessitates the use of infinitesimals in the geometry of the world.

Then, again, if all the points of space are to play the same rôle, that space must be homogeneous; if all the directions from any one point are to play the same rôle, that space must be isotropic. If the space is to be homogeneous it must be without limits, for if it were finite in extent it could not be homogeneous, since its boundary could not play the same part as the centre in the scheme of things. By this we do not mean that space is necessarily infinite; this apparent paradox may be overcome by regarding the sphere as having a surface without boundary, yet finite in extent.

In the past it has been customary to look upon force as the great fact that lies at the bottom of all things; but this is far from the final act. Later, the conception of force disappears, and whatever happens is regarded as the fact to be observed. It will be evident from this examination of the point-events of nature that an infinitesimal geometry is required to explore the properties of space; this attitude now brings us to a new and all-important view-point of phenomena. The hypothesis of continuity involves such an interdependence of the facts of the universe as forbids us to speak of one fact, or a group of facts, as the *cause* of another fact, or group of facts. From this hypothesis of space-time a knowledge of the whole history of a single particle is shown to be involved in a complete knowledge of its state at any moment. While dealing with this point, the reader's attention might be drawn to a statement in the admirable book of Viscount Haldane, *The Reign of Relativity*, p. 114 (3rd edition), where the author states that the "laity have been taught that infinitesimals are now banished out of mathematics, except as symbols for limiting relations of order in quantity." The *raison d'être* of the infinitesimal geometry lies deeper than would at first appear from this quotation, in so far as the element of length, ds, implies that a process involving the *average* value underlies the consequent mathematical treatment of the event under consideration. Mathematically stated, Taylor's Theorem gives the entire past and future history of the movement of a point-event, provided that there is no infinite change in the derivatives involved.

As it is easy to show that no velocity can exceed that of light, so long as the facts of nature are recognised in their true sequence, it may be reasonably asked, What would happen in the case of two particles passing each other with a relative velocity greater than that of light? The answer to this query, in the light of the new theory of time and space, would be that the presence of the two particles would alter the neighbouring space in such a way as to make the relative velocity in four-dimensional space less than that of light. This explanation is very useful in interpreting the variation of the mass of high-velocity particles, and it also suggests that gravitational energy has mass.

Thus it is seen that Einstein's Theory makes for a great unification in mechanics, for it brings together inertia and gravitation—further, it does more, it identifies these two forms of forces as belonging to one common force, and not, as heretofore, calling them opposing forces. Henceforth, a body travelling through a frictionless medium has a non-uniform motion, since its path in space is parabolic in form; to describe a uniform motion a suitable mesh system of the space-time continuum would have to be used.

The tendency of a moving body is to follow a natural track, or geodesic, and in the presence of matter a force will be called into play which acts on the body in question; this force is composed of gravitation and inertia elements. In other words, a gravitational field necessitates a curved, or grooved, continuum, and this consequent groove in space can be shown to be of the first order of curvature. From the earlier remarks, it is now clear that this dis-

turbance in the space of the world *is* matter, and the more matter there exists the more space there is to accommodate it. Thus, because space depends on matter, this matter is a necessity of the world. Bearing in mind the fourfold nature of our present world, it can be seen that there is greater continuity in this geodesic structure of space than in the older material structure of the world.

Point-events are used in the theory because they represent the most fundamental concepts of nature, and the aggregate of *all* such point-events represents the universe. Therefore, certain coefficients in the equations of space must be identified with natural forces in order to correlate the new dynamics with the old. To illustrate this process, let us take the simple case as given on p. 189 in SCIENCE PROGRESS, October 1921.

Consider a unit gauge of interval length as being l at point P and let it be moved from P in space, whose co-ordinates may be defined as the origin, to a point Q whose co-ordinates are defined by the infinitesimals δx_1, δx_2, δx_3, δx_4. Let the change in its length due to this movement, in terms of the displacements, be $\lambda . l$.

λ is thus made a function of the displacements δx_1, δx_2, δx_3, δx_4, since the change in length must be proportional to the initial length, and independent of the direction of motion because the interval length is independent of direction.

$$\text{Then } \lambda = \phi_1 \delta x_1 + \phi_2 \delta x_2 + \phi_3 \delta x_3 + \phi_4 \delta x_4 \quad . \quad . \quad . \quad . \quad \text{(i)}$$

Now consider a frame of co-ordinates x, y, z, t in uniform motion; let $F, G, H, -\Phi$ stand for ϕ_1, ϕ_2, ϕ_3, ϕ_4, respectively, in (i)

$$\text{then } \lambda = \frac{dl}{l}$$
$$= Fdx + Gdy + Hdz - \Phi dt \quad . \quad . \quad . \quad . \quad \text{(ii)}$$

where dl represent the change in length of the rod due to its movement from P to Q.

On integrating (ii), we get

$$\log l + \text{const} = \int (Fdx + Gdy + Hdz - \Phi dt) \quad . \quad . \quad . \quad . \quad . \quad \text{(iii)}$$

From an examination of (iii), it will be seen that if l is to be independent of the path taken from P to Q, the expression

$$Fdx + Gdy + Hdz - \Phi dt$$

must be a perfect differential.

If this is true we have

$$\left.\begin{array}{l|l} \dfrac{\delta H}{\delta y} - \dfrac{\delta G}{\delta z} = 0 & -\dfrac{\delta \Phi}{\delta x} - \dfrac{\delta F}{\delta t} = 0 \\ \dfrac{\delta F}{\delta z} - \dfrac{\delta H}{\delta x} = 0 & -\dfrac{\delta \Phi}{\delta y} - \dfrac{\delta G}{\delta t} = 0 \\ \dfrac{\delta G}{\delta x} - \dfrac{\delta F}{\delta y} = 0 & -\dfrac{\delta \Phi}{\delta z} - \dfrac{\delta H}{\delta t} = 0 \end{array}\right. \quad . \quad . \quad . \quad . \quad \text{(iv)}$$

On examining these forms of (iv) it will be seen that these are the expressions for the three components of magnetic force and the three components of electric force, if we let F, G, H, $-\Phi$ represent the usual potentials of the electromagnetic theory. Thus we may state that, in the given case, the condition for distant intervals to be directly compared, independently of the paths between the points, is that the magnetic and electric forces have a null effect in the neighbouring four-fold space.

THE GOLGI APPARATUS (Reginald James Ludford, Ph.D., B.Sc., F.R.M.S., University College, London).

TWENTY-THREE years ago, the Italian histologist, Golgi, first described in cells of the central nervous system a net-like structure which he called the "interno reticulare apparato" (internal reticular apparatus). Since that time a similar structure has been found to occur in all types of cells prepared by appropriate methods, and has been called, after its discoverer, the Golgi apparatus.

There is considerable variation in the form of the apparatus in different animals and also in the different tissues of the same animal. In nerve cells of the higher animals it has the appearance of an irregular broken network, but in most of the other cells of their bodies it consists of a small compact network usually external to the nucleus. In Molluscs (*e.g.* Snail and Limpet) it is usually represented by a number of small semilunar rods either scattered throughout the protoplasm of the cells or else grouped together to form a similar body as in the higher animals. Very commonly in insect cells, the apparatus occurs in the forms of short rod-like granules without definite arrangement (6). (These typical forms of the Golgi apparatus are shown in Fig. 1.)

Generally the Golgi apparatus is only visible when cells have been prepared by special techniques (see 4). It can, however, be seen in the living male germ cells of the Garden Snail (*Helix aspera*) as small refractive rods, while in some other cases it can be observed by special dark-ground illumination methods. Some cells, especially those of Molluscs, show the apparatus when stained in the living condition by dyes such as Janus green and Janus black, and sometimes neutral red is effective. Usually, however, it is necessary to kill the cell and impregnate the Golgi apparatus in a special manner (4). A method commonly employed is to transfer the cell which has been killed in a fixing and mordanting solution to silver nitrate solution, and afterwards to a reducing solution. As the substance of the Golgi apparatus has a marked affinity for silver stains, this procedure leaves the apparatus impregnated with the silver. Another very effective method is to keep cells in dilute osmic acid for two or three weeks. The Golgi apparatus then becomes black, owing to the reduction of osmium tetroxide to the dioxide by its lipoid constituents. It is interesting to find that if developing male germ cells of the snail be prepared by both of these methods, the form of the Golgi apparatus is the same in each case, and identical with its appearance in the living cell stained with neutral red.

Various researches have shown that the apparatus plays an important part during periods of special cellular activity, and at certain stages of the life-history of animals. One of the most comprehensive studies of the subject is that which was carried out by Gatenby with the common pond snail, *Limnæa stagnalis* (5). Gatenby followed the behaviour of the Golgi apparatus from the earliest stage of development of the germ cells to the formation of the young embryo. The primitive germ cells contain a Golgi apparatus consisting of a few semilunar rods grouped together at the side of the nucleus. The female germ cell, as soon as it commences to mature, grows considerably in size and undergoes a series of changes, the purpose of which is to prepare it in such a way that on fusion with the mature male germ cell, it will develop into a new organism. Very important for this purpose is the elaboration of reserve food substances to provide the necessary energy and materials for development. The Golgi apparatus is specially concerned with this process. Each individual rodlet divides repeatedly, and, breaking away from the original compact group, the rodlets spread throughout the cell, continually dividing. From the very beginning of this activity certain of the rodlets become concerned with the formation of yolk (9). A little globule of fatty substance appears on the concave side of the rodlet, and this grows consider-

ably, until a comparatively large yolk sphere is formed. By the time the egg is ready for fertilisation it is full of such yolk spheres and contains numerous free rodlets as well. The male germ cell which enters the egg brings no Golgi elements with it, so that all those of the embryo are derived from the mother. When the fertilised egg-cell begins to divide, the Golgi elements which are scattered throughout the cytoplasm become shared out among the daughter cells; they undergo no division as do the chromosomes of the nucleus. Gatenby found that this sharing out of the elements of the apparatus continued throughout development, and that during intervals between cell division the Golgi rodlets increased in number by fission (Fig. 2, L, M, N; Fig. 1, C).

The mode of behaviour of the Golgi apparatus is not the same in all animals. In some cases it does not appear to play any part in yolk-formation, but as far as we know it is always present during development, and behaves as in *Limnæa*, during early developmental stages. In Mammals, the male germ cell carries with it a few elements of the apparatus which enter the egg at fertilisation, but their subsequent fate is unknown (7).

The indefinite behaviour of the Golgi elements during cell division, first pointed out by J. A. Murray (10), militates against the view that they play any definite part in hereditary transmission. Gatenby has pointed out that the same indefinite behaviour of the elements of the apparatus is characteristic of the reduction divisions of the male germ cells, further emphasising the improbability of the Golgi apparatus being concerned in heredity (8) (Fig. 2).

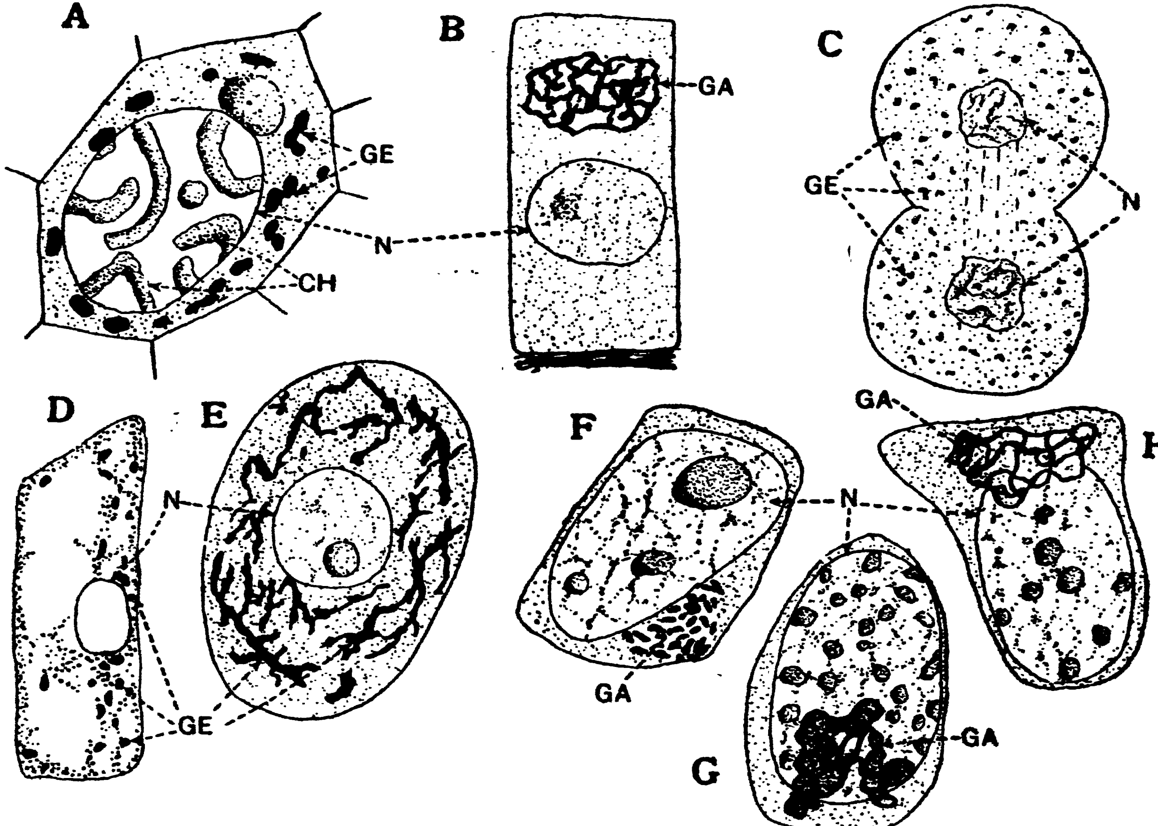
It is at periods of special activity of cells that the apparatus plays an important part. In gland cells, where it is reticulate in form, several observers, amongst them Golgi himself, have described the displacement and change in form of the apparatus during functional activity. This has been observed in glandular cells of the stomach, and in sebaceous and mammary glands. In goblet cells of the intestine, Cajal found that when secretory activity commenced, the apparatus increased in size, then broke up into fragments, and on discharge of the secretion from the cells, some of the fragments passed out with the secretion (1).

Several researches have been carried out recently on the apparatus in nerve cells. Penfield has found that decerebration and high section of the spinal cord of Mammals had no effect upon the anterior horn cells of the cord. Similarly tetanus produced no change, nor did strychnine poisoning (11). Da Fano kept rats on a reduced diet, then examined the cells of the spinal cord, but again there was no difference apparent in the Golgi apparatus: however, when rats were exposed to the cold, then the apparatus of the nerve cells of the cord seemed to shrink up and contracted around the nucleus (2).

Penfield cut the axone branch of nerve cells of the cord, with the result that in a period of from four to seven days, this was followed by the spreading out of the Golgi apparatus towards the periphery of the cell, and was succeeded in some cases by a partial disappearance of a part of the apparatus. The same investigator showed that on displacement of the apparatus towards the cell wall, a system of canals could be observed around the nucleus. These comprise the so-called "trophospongium" of Holmgren, which has been erroneously regarded by some cytologists as the negative representation of the Golgi apparatus (12) (Fig. 2, I, J, K).

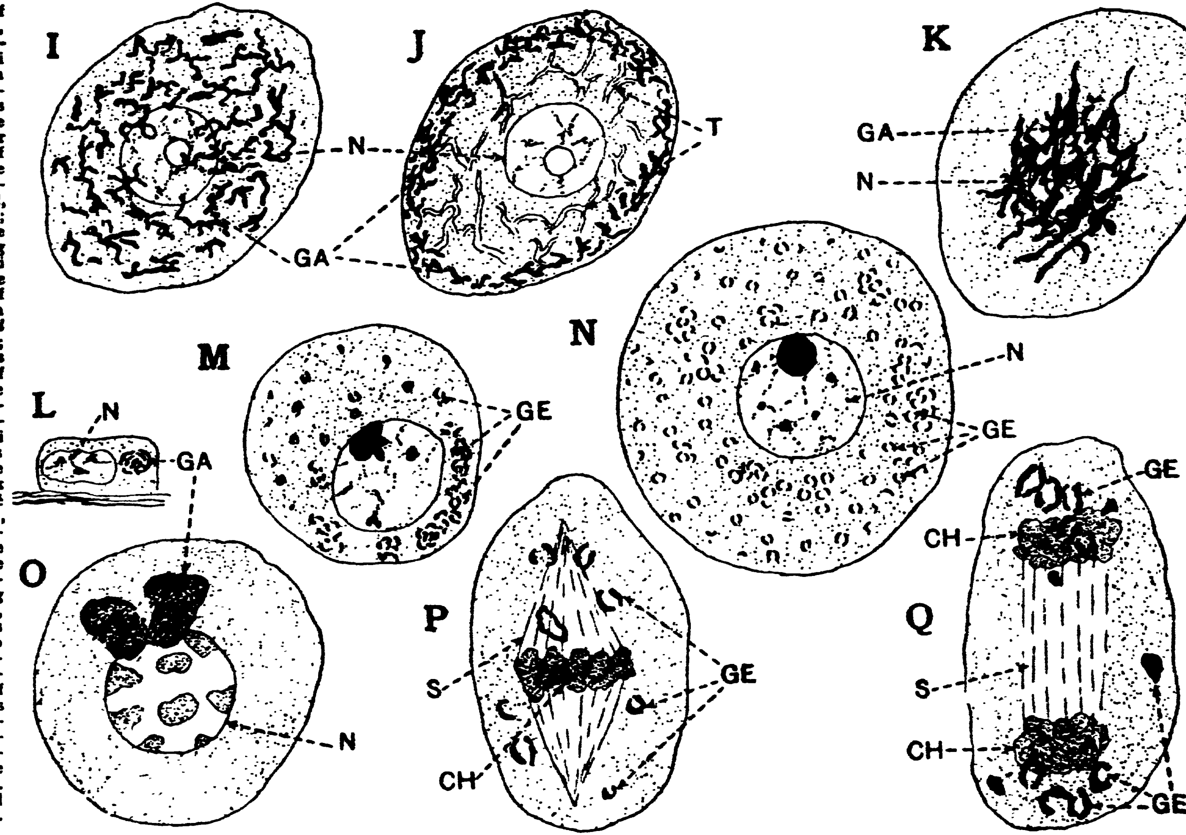
Da Fano has recently investigated the nature of the apparatus in cancer cells (3). He has found that it presents a characteristic appearance in different growths. In some tumours it has the form of a thick network, in others it is a very fine network, while in some cases it consists of irregularly scattered rodlets (Fig. 1, F, G, H).

From this survey of some of the researches that have been carried out on the Golgi apparatus, it will be seen that this cell organ apparently plays an important part in the mechanism of life. As Harper says, "the cell appears



A—Male germ cell (spermatocyte) of cricket [J. Bronté Gatenby and R. J. Ludford, *Proc. R.S.*, 1921]; *B*—Epithelial cell of rat [original from a Da Fano preparation]; *C*—Dividing ovum of the Mollusc, *Limnæa* [J. Bronté Gatenby, *Q.J.M.S.*, vol. lxiv]; *D*—Cell of onion root [A. H. Drew, *Jour. R.M.S.*, 1920]; *E*—Developing oöcyte of the bat [D. Cattaneo, *Est. dall' Arch. Ital. di Anat. e di Embr.*]; *F, G, H*—Three Forms of the Golgi apparatus in cancer cells of the mouse—*F*, Adenocarcinoma; *G*, Jensen's carcinoma; *H*, Alveolar carcinoma [C. Da Fano, *Rep. Imp. Cancer Res. Fund*, 1921].

FIG. 1.—*Forms of the Golgi Apparatus in Various Cells.*



I—Nerve cell of spinal cord of the rat, showing normal arrangement of the Golgi apparatus [from a Da Fano preparation]; *J*—The Golgi apparatus displaced towards the periphery of a similar cell as the result of section of its axone branch, seven days previously [W. G. Penfield, *Anat. Record*, 1921]; *K*—Nerve cell of the spinal cord, showing contraction of the Golgi apparatus around the nucleus, as the result of exposure to cold [C. Da Fano, *Journ. Nerv. and Ment. Dis.*, 1921]; *L, M, N*,—Three stages in the development of the egg cell of the Mollusc, *Limnaea*, showing the growth and dispersion of the Golgi apparatus, which is concerned with the formation of yolk spherules [J. Bronté Gatenby, *Q.J.M.S.*, vol. lxiv]; *O, P, Q*—Three stages in the division of the male germ cell (spermatocyte) of the guinea-pig —*O*, the early phase, the Golgi apparatus nearly divided into two parts; *P*, the commencement of the anaphase, the elements of the Golgi apparatus scattered; *Q*, the telophase, the Golgi elements are coming together to re-form the compact apparatus [J. Bronté Gatenby and R. J. Ludford, *Proc. R.S.*, 1921].

FIG. 2.—*Modes of Behaviour of the Golgi Apparatus in Different Animal Cells.*

to be a colloidal system in which special processes and functions have become localised and fixed in certain regions"; this has led to the evolution of cell organs such as the nucleus and Golgi apparatus, which are more or less permanent structures. Exactly what part in such colloidal systems the Golgi apparatus plays, it is impossible to say, owing to the incompleteness of our knowledge of the physico-chemical activities of the cell.

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Text Figures.—The greater number of the figures have been drawn diagrammatic, involving alteration from the originals. In these cases the journals in which the original figures appear is given in brackets together with the names of the authors.

Explanation of Lettering.—*CH* = Chromosomes; *GA* = Golgi Apparatus; *GE* = Element of Golgi Apparatus; *N* = Nucleus; *S* = Division Spindle; *T* = Canaliculi of Holmgren.

THE PLACE OF INSTINCTS IN OUR SOCIAL LIFE (J. S. Dunbar).

It is a truism to remark that, when we compare the life of animals with the life of human beings, we find certain broad similarities alike on the physiological and the psychological sides. The dominant facts of nutrition and reproduction are obviously analogous, and in the same category are the more patent similarities in anatomy. But in the general field of behaviour, the comparison is not so easily traced; man does not seem to be in the same line of development. It was this distinction that led Descartes to formulate a purely mechanical scheme to explain the behaviour of all the animals except man. It is evident that such a scheme sprung from the observation that the behaviour of animals could be more or less easily accounted for in terms of well-defined laws. Nowadays, however, we no longer cling to the mechanical explanation, but rather define the difference between man and the other animals as one depending on the proportion of

instinct and intelligence displayed in behaviour. It is a mistake, however, to assume that the great use man makes of intelligence puts him on a wholly separate plane. For in his fundamental psychological make-up, he is in the same category as the other animals. That it is possible to overlook that fact is due mainly to the development which has by now relegated the basic motive powers of his life to the background of his consciousness. The cave-man is nothing more than an exceptional element in modern society; the mass of men have emotions and feelings which are compounded in various ways from the elemental psychological structure which man has in common with the other animals. The strength of some of the elements in this structure has ensured the continual progress of man in the direction of more complicated social relationships; and this progress has at the same time been accompanied by the gradual covering-up of the more egoistic tendencies.

When we see animals behave as if they were subject to some apparently mechanical laws, and when we realise that the behaviour of men in most respects springs from similar sources, we are inclined to ask what is the nature of these motive powers which impel men to think and act and feel, and in what ways do they operate in our complex modern society. It is clear that the conduct of man is regulated by many considerations affecting his relations with his fellow-men, or in other words, that he is by no means a free agent, giving full play to every impulse that prompts to action. The important fact, however, is that the impulses are there; and it is when we come to ask what they are, what is their number and kind, that we are faced with the problem of defining an instinct.

We shall proceed best by taking an example. Suppose a man to be suddenly struck by another so that pain is inflicted. If we examine the reactions of the man who is struck, we shall find that the blood rushes to his face, that the circulation generally is quickened, and that—except, of course, in cases where he is rendered unconscious—the whole body immediately assumes a defensive or an antagonistic attitude. Now, in such a case as this, there can be no possibility of arguing that the reaction, or the state of mind caused by the blow, is the result of deliberation. The effects indicated follow without conscious interference on the part of the man who exhibits them. Here, then, we seem to have arrived at something that man has the capacity to do without first learning how to do it, without experience. Such a reaction as that described above is thus apparently part of the original endowment of human beings, part of the inheritance of the race; and the capacity, or the thing inherited, does not vitally change from one generation to another.

On further examination, we can separate three distinct experiences in the total experience we have described above. First of all, there is the sensation of the blow, and the realisation that another man is offensively disposed. Secondly, there is the emotional state, which is the bodily disturbance (the quickened circulation and so on), and which we call the emotion of anger. Lastly, there is the impulse to act, to retaliate on the aggressor. On further analysis, we should find some important distinctions in the operation of these three parts. We should find, for example, that the emotion of anger may be actually roused without blows. It may be roused, for instance, on a man learning that some hurt has been done to a near relative or friend. The mere relation of the fact that an assault has been made on a man's wife will be sufficient for him to experience all the bodily disturbances we have described above, and even to seek to express his impulse by an attack on the offender. We thus see that the emotion of anger may be roused by direct or by indirect offence; and the ways in which the indirect method may take effect are extremely varied. Men become angry at an implied insult to their own characters, at hurt or at the threat

of hurt to persons whom they cherish, even at the *idea* of injury to, or depreciation of, these persons. On the other side, too, we find that the impulse to act may take outlet in equally various directions. To some extent the nature of the action will depend on the intensity of the emotion; so that a man who on one occasion would immediately strike his antagonist, would on another occasion defer his retaliation until he could make it more effective. Or, in another way, a man might make it a practice to check his impulse to strike, because he held certain ideas about the effect of physical force. We can now see that two parts of the experience are quite definitely capable of variation, and also that the impulse given by the emotion may be greatly controlled. It only remains to add that the central part of the experience, the actual emotional disturbance, appears to be unchangeable for each individual. This capacity is one part of his inheritance which is not subject to modification as a result of experience.

We can now say that an instinct is an inherited or innate tendency to experience a definite emotion when we become aware of certain things, and also to experience an impulse to act in a similarly definite manner.

We have merely to consider the principal instincts for a moment in order to realise the determining effect they have on life as a whole. The parental instinct, with its corresponding emotion, known as the tender emotion, has effects which reach far along the line of social life. And if we take with it the reproductive instinct, we get a combination that plays perhaps a greater part in life than any other instinct. In some animals, these two instincts do not always act together; it is found that in the males of some species the parental instinct is not shown. But in the case of females, it is almost invariably strong; and it is needless to point out the value of this in protecting and rearing the young during their period of dependence. In human beings, however, it is only in depraved natures that these two instincts do not operate together. As regards the vast majority, we can easily see the far-reaching influence in the fact that our social life has as its base the family. There is no need here to record instances of the extent to which men and women sacrifice themselves for their children at the prompting of the parental instinct. While in the case of the father the instinct may not appear to be very strong, that is largely because of the degree to which woman effaces her own desires and needs for the sake of her children. The working of the instinct, also, is not confined to immediate needs; it prompts the parents to look far into the future and make plans for the well-being of the offspring years ahead. It is the cause of an amount of suffering and self-sacrifice which is not paralleled in any other relation in life.

While we are dealing with the parental instinct, it is worth while to remark that it is very probably from its emotion that altruistic conduct in any form arose. As society emerged from its simple form, where the tribe was the largest unit, the tender emotion man experienced towards his family or tribe made it possible for him to develop a similar—though no doubt less intense—feeling for his fellows; and as a result rendered the extension of the range of his society more easy. It is very likely, also, that from this same source there sprung the idea of the whole human race as one family.

In connection with the position of man in family, tribe, and nation, there is another instinct that works in a binding way, and that is the gregarious or herd instinct. The animals other than man, of course, present very many instances of this instinct in its simplest form. When an animal has become detached from the herd, it is not always fear that makes it run at once to its fellows. The same tendency occurs when the possibility of fear is ruled out. And we see animals in some cases perform actions which can only be interpreted as expressive of satisfaction at being again in the herd: as, for example, when a cow rejoins the herd and rubs itself against

others. Though the working of this instinct is not so obvious in the life of human beings, it can nevertheless be easily traced. Cities like London and New York have not grown to what they are, merely because of economic and geographical factors. Men do not by intelligent choice live amid squalor and dense packing, and unhealthy conditions generally. One factor which goes to explain the conditions we find in those cities is, that men get a peculiar satisfaction in being together in crowds. Even in aimless street-walking there is a certain enjoyment from the feeling that one is not alone, but surrounded by one's fellow-men. In the case of such sports as horse-racing and football, the working of this instinct is even more obvious. How much enjoyment would a man derive from a race or a football match if he had to watch it alone? Here, indeed, the fact of being in a crowd evidently accounts for the greater part of the enjoyment. It is no doubt in virtue of the possession of this instinct that man has been called a social animal.

The instinct which corresponds to the emotion of fear has been called the instinct of flight. That is perhaps the most convenient way of naming it. For when the emotion is experienced in an acute degree, the predominant impulse is to flee. Fear is undoubtedly the most terrible of all the emotions that man may have. The range of its intensity is very great: from what is no more than a shadow of fear to the other extreme when it actually paralyses the individual. Then he becomes incapable of thought or action, and is aware only of the terrifying object. Literature abounds with descriptions of persons seized with this emotion in all shades of circumstances. Later on, we shall have to examine the great influence fear has had on man's general moral conduct. In that connection, too, we shall mention the instinct of subjection. It is somewhat analogous to fear because it comes into play when we find ourselves in the presence of superior powers, but of course in the case of subjection there is nothing of the element of terror. It is merely an impulse to hide our own personality in the face of something greater. It is often found in a very marked degree in children and in adults of mild, timid disposition.

From subjection we may pass to its opposite—the instinct of self-display, or self-assertion. That it is an instinct in the real sense of the word is undeniable. Children give evidence of it very early; they are anxious, sometimes to the point of tears, that we should see what they have done. It is also recognisable among animals, if we recollect the strutting behaviour of a big dog in the presence of smaller ones. Right through our own life, however, we can trace its workings: in the boasts of boys and the vanity of girls, and in adults, where it plays an important part in one's idea of oneself. Unlike subjection, which is accompanied by slow movements, and drooping of the head, self-assertion brings along with it an increase in strength, and a greater confidence born of the feeling of physical vitality.

Curiosity must be given a high place among the principal instincts. Its corresponding emotion of wonder is expressed in the attitude of keen and questioning approach to the object which inspires it. Considering this impulse to approach and examine, we can understand how those who possess the instinct of curiosity to a highly developed extent have been the discoverers of history. In fact, the instinct takes us further; for when we remember that in primitive society the bonds of custom were extremely rigid, we must thank the individuals who pushed their curiosity far enough to break these bonds and make progress possible. Or, again, we may say that it is because men possess this instinct in a marked degree that science has been able to reach the point it is at to-day. It is also true to say that the conflict of science and religion springs from the difference in the strength in which men possess this instinct.

Disgust is an emotion which is very commonly experienced. It occurs

at the sight of an ugly object; an offensive smell or taste provokes it; and it may even be caused by the bearing or general look of people. The impulse is always to shrink from the object which inspires the emotion; for example, to cast out of the mouth what is offensive to the taste. In all directions the biological value of this instinct of repulsion is very evident, in protecting man from things that may injure him unawares.

One important instinct remains: viz. the pugnacious, with its corresponding emotion of anger. We have already seen how powerful this emotion is, and how it may be aroused in a great variety of ways. Its operation is evidenced in the life of society in the conflicts which arise between nations or between different sections in the same nation. Its strength carries men on to untold sacrifice and exertion. In one respect it holds a unique position: the reason for its coming into play is very often that one or other of the remaining instincts is thwarted. The best example is perhaps that of animals, when in the mating season the reproductive instinct is denied expression. Then the animal's anger will turn on anything without discrimination.

Two subsidiary instincts of importance should be mentioned. One of them, the instinct of construction, is apparent from the desire men have to make things, whether these things are useful or not; and even more apparent from the activities of children. The other is also very easily noticed, the instinct of acquisition. We have instances of it at all stages in the life of the individual; from the time when, as a boy, he collects foreign stamps, till the period of manhood when his collection may be of pictures or books. Men, too, go on acquiring wealth when it will not be possible either for themselves or for their children to use it all.

Anyone who would understand the complex factors which go to make up the civilisation we know, must begin by realising that the instincts are the prime sources of human activity. From the impulses that follow from the instincts, we derive the motive power for every train of thought, for every feeling or action. The instincts are the steam without which the engine would be useless, the key without which the door of life would never be opened. It might be objected that man as he is to-day is not a creature that shows himself to be to any noticeable extent subject to the free working of these prime movers. Superficially that is true; for it cannot be said that in our everyday life we continually show the simple, undiluted workings of the instincts. Our life is not a series of clear examples of actions which can immediately be traced to the operation of a single instinct at a time. The play of intelligent adaptation and control has rendered it possible for man to achieve the habit of living in societies, as he does to-day, where his relations with other men may be complex to a very high degree, and where, as a consequence, his actions are no longer simple and easily explained, but are in many ways compounded of varying mixtures of his instincts. It is obvious, for example, that our attitude to other men is not always one of fear, or anger, or curiosity, or tender emotion, or of any one of the instinctive attitudes. Our relations show the existence of a large group of complex emotions such as admiration, reverence, contempt, loathing, reproach, anxiety, shame, revenge; and also of experiences like love, hate, and respect, which we call sentiments.

In giving the name of "sentiment" to love, hate, and respect, we are recognising that they are different in kind from both the primary and the complex emotions. We can see the force of that distinction if we recollect that we may experience a large number of these emotions towards the persons we love or hate or respect, according to the circumstances they are in. For an object of our love we may have anxiety, jealousy, or resentment. From that we may deduce that these sentiments are made up of tendencies to experience certain of the emotions towards the persons whom we love, hate,

or respect. In other words, the fact that we hate an individual means that in certain circumstances we shall be liable to experience a certain emotion towards him; if he succeeds where we have failed, we are jealous of him. Now it is evident that we can have an almost indefinite variety of shades in which the primary emotions may be compounded. A simple example is the emotion of admiration. It is not merely an expression of wonder; because we are conscious of a temptation to shrink from approaching the admired object, and in a case of simple wonder, we do not have that feeling. This other element must surely be the emotion of subjection, which we experience in the presence of what we feel are superior powers. It should be remarked that our admiration may be excited by a person for whom we have not yet developed an organised sentiment; and the same occurs in the case of the complex emotions of reverence, contempt, and loathing. For the remainder of the complex emotions we mentioned—reproach, anxiety, shame, or revenge—there is required a developed sentiment before we can experience them. Reproach, for example, appears to be compounded of anger and tender emotion. The object of our reproach is a person whom we love; and it is that fact that explains why we feel reproach towards an action of the loved person, when towards a person to whom we were merely indifferent we should experience simple anger. These are but two examples of the complex emotions that we may have, but they are sufficient to suggest the lines along which the evolution of our emotional life has proceeded.

We have already referred to the working of intelligence on the free play of instincts. It is clear that society would not have reached the complexity and diversity it shows at present if the primary impulses of man had been allowed free play. Man, indeed, has had to learn how to live in society, and the process of learning has been long and slow, and has very often ended, as a matter of fact, in the decay of successive societies. In the primitive tribe or clan, the social discipline which the individual had to submit to was of a comparatively simple and rigid type. The prime necessity was safety from attack by other tribes, for the sufficient reason of obtaining an adequate food-supply. It was to be expected, therefore, that in such a society the discipline was based on fear—fear of the leader who acted for the whole tribe in enforcing the restraints imposed on individual freedom. The great force of the emotion of fear was bound to make that discipline react with great efficacy on the conduct of the whole tribe; and it is found to-day among primitive peoples that the strength of the tribe as a defensive or aggressive organisation is an indication of the rigidity and also of the enlightenment of the social restraints upon individual conduct.[1] In modern society there is undoubtedly in practice a large measure of reliance upon fear for the enforcing of moral rules; but also there is a growing tendency to substitute some reason or motive of a less harsh and primitive nature. It is realised that, in many cases, threat of punishment, either human or Divine, is at once unreliable and too severe. There can be little doubt that this change is largely due to the extension of the sympathy that is engendered by the tender emotion. In addition, it represents a further stage in the subjection of instinct to intelligence.

At this stage we are confronted with what we may call the dilemma of civilisation. We have noticed that the more aggressive tribes among primitive peoples do actually show higher morality than the more pacific ones. We can illustrate this truth from another point of view—the history of a single civilisation that has gone through all the stages from birth to decay. Babylon, Greece, Rome, Spain—all give evidence pointing to the same conclusion. In each case the rise to eminence took place along with

[1] See Wm. M'Dougall's *Introduction to Social Psychology*, chapter xi.

a rigid social discipline, a growing sense of the necessity of enforcing on individuals conformity to a high plane of moral conduct. So long as the citizens of these empires retained their moral and physical vigour, so long were their civilisations intact and worth preserving. But as soon as that vigour showed signs of relaxing or decaying, the process of degeneration began. The restraints that once held the social structure together were no longer enforced with the same rigour. It would appear that the intelligence had proceeded too far in the regulation of conduct and the control of the instincts. It is said, for example, that the fall of Greek civilisation began when control of the parental instinct became so great as to interfere disastrously with the continuance of the race. What seems to emerge from these facts about civilisations that have decayed is that, after a certain point, the control of the primary impulses produces a suicidal result, that in limiting the play of instincts, there has in every case resulted the eventual atrophy of them, with finally the decadence or disappearance of the race.

We can approach this problem in a more cogent way if we consider the case of the individual. The progress from childhood to manhood is in one respect very much the control and redirection of the strongest impulses. A man of thirty will certainly not show the same impulsive affection for his mother as he did when he was a boy of ten; and a similar process will have taken place in the expression of his other impulses. In another way, it may happen that, through lack of encouragement, the impulsive boy will find less and less opportunity, as he grows older, to express his emotions. Examples of what is commonly called hardness are not difficult to find in everyday life; but it is a mistake to assume that the tender impulses are not to be found in the hard man. In the early part of this article, we insisted on the fact that the instincts are a vital part of man's inheritance, and that in their expression the only factor that is constant or unchangeable for each individual is the capacity for experiencing the emotion. In other words, if adequate play is not given to the impulses that arise from the instincts, these unexpressed impulses recoil on the individual and seriously affect his whole life. It is as if the steam in an engine were not allowed to escape after a certain point, and in consequence deranged the working of the machinery.

There emerges, then, quite clearly from these considerations the vital need for allowing the motive power that the instincts give to man to be used to the fullest degree. We saw in the beginning of this article how the impulse that arises from the emotion of anger may be expressed in many different ways. From that fact we can proceed to the conclusion that for the other impulses the attitude of society ought to be the same: viz. that the impulse *in itself* is not the thing that is to be eradicated or allowed to decay; but that it should be directed into channels which will permit of full expression, and which at the same time will be injurious neither to the individual nor to the community to which he belongs.

THE MENTAL ABILITY OF THE QUAKERS (By E. H. Hankin, M.A., Sc.D., Agra, India).

If a community adopts a system of education in which efforts to develop intelligence are quite subordinated to the teaching of religious dogma and "formal discipline," and if its members hold a creed in which sensible reasoning about many matters of daily conduct is regarded as a temptation of the Evil One, what effect will such a mental régime have on their ability? An answer to this question is given by the history of the Quakers.

This religious sect, properly known as the "Society of Friends," was founded by George Fox in the year 1648.[1] Besides ordinary theological

[1] Sewel, *History of the Quakers*, English edition of 1811, vol. i, p. 26.

beliefs that they held in common with other Christian sects, they held certain accessory beliefs, which profoundly influenced their daily life and which were based on reasoning so imperfect and short-sighted as almost to deserve the name of irrational. Macaulay says of George Fox: "One of the precious truths which were divinely revealed to this new apostle was, that it was falsehood and adulation to use the second person plural instead of the second person singular. Another was, that to talk of the month of March was to worship the blood-thirsty god Mars, and that to talk of Monday was to pay idolatrous homage to the moon. To say Good morning or Good evening was highly reprehensible; for such phrases evidently imported that God had made bad days and bad nights. A Christian was bound to face death itself rather than touch his hat to the greatest of mankind. When Fox was challenged to produce any scriptural authority for this dogma, he cited the passage in which it is written that Shadrach, Meshach, and Abed-nego were thrown into the fiery furnace with their hats on; and if his own narrative may be trusted, the Chief Justice of England was altogether unable to answer this argument except by crying out, 'Take him away, Gaoler.' Fox insisted much on the not less weighty argument that the Turks never show their bare heads to their superiors; and he asked, with great animation, whether those who bore the noble name of Christians ought not to surpass Turks in virtue. Bowing he strictly prohibited, and, indeed, seemed to consider it as the effect of Satanical influence; for he observed, the woman in the Gospel, while she had a spirit of infirmity, was bowed together, and ceased to bow as soon as the Divine power had liberated her from the tyranny of the evil one . . . from these rhetorical expressions [in the Bible] in which the duty of patience under injuries is enjoined he deduced the doctrine that self-defence against pirates and assassins is unlawful."[1]

In their use of the words "thee" and "thou"—the so-called "plain language"—in their habit of designating the days of the week and the months by number, in their refusal to use the ordinary salutations demanded by social custom, in their objection to self-defence, and in their peculiar dress, the Quakers held a set of tenets whose incompatibility with profane reason must have been a matter of daily experience. Those who joined the sect must have been persons whose minds were so constituted that they could easily disregard the results of sensible reasoning.

They also habitually showed an unwillingness to rely on reasoning, which sometimes led to singular consequences. A ship belonging to a Quaker once started on a voyage to America carrying a number of Quaker missionaries. Observations of latitude and longitude would have involved an unwelcome reliance on profane reason. So, instead of making such observations, they "daily waited on the Lord," and with guidance thus obtained, reached their destination after a rather lengthy voyage of two months. A Quaker assembly once issued orders that fish were not to be caught in their breeding season. Instead of giving a rational sanction for this ordinance, they preferred to give a religious one; they said that catching fish in the breeding season was in some measure "a violation of the command of God in the beginning, when He blessed them and commanded them to increase and multiply."

Even in their religious services Quakers showed reluctance to rely on conscious reasoning. Instead of a fixed form of service led by an appointed minister, the Quakers, at their meetings, would sit in solemn silence until one of them, impelled by an impulse from the subconscious mind, would begin a prayer or an extempore sermon. Music was banned both from their religious services and from their schools. They considered that in education it led to "self-gratification and little improvement of the mind."

In meetings for secular affairs they objected to anything so rational as

[1] *History of England*, chapter xvii.

counting votes or a show of hands. Controversial speeches referring to previous speakers were strongly discouraged. One or two members would act as assessors and come to a decision from the tenor of the speeches.

As to education in the Society of Friends, " a fear that it might interfere with the higher light led, at certain periods, to much opposition towards the higher branches." In 1781, a Friend wrote deprecating any large share of learning for school-children " lest it should be injurious to them, touch their vanity and infect them with the disease of taste and refinement that too much prevails amongst us." In 1705 various " heathenish authors," as Vergil, Horace, Juvenal, etc., were laid aside in favour of the Latin Bible, the *Academia celestis*, and Robert Barclay's *Catechism and Apology.* The frame of mind of the Quaker school-teachers may be judged from the following anecdote: The Right Hon. W. E. Forster, as a boy at a school at Tottenham, wrote an essay in which he referred to mathematics as the noblest of the sciences. Such praise of a product of pure reason somewhat alarmed the worthy schoolmaster, who accordingly wrote on the essay that " all worldly knowledge, even that of ' the grandest and noblest structure ever raised by mental art,' was but dross in comparison with the excellency of the knowledge of Christ Jesus and Him crucified."

Despite such education and such habits of thought, the Quakers from the first displayed a singular shrewdness, ability and success in business affairs. This is the more remarkable in that certain of George Fox's tenets provided very serious handicaps to them in commerce. In 1693 Quaker overseers inspected shops to see if " needless things were sold," such as " lace and ribbons." " Gilbert Latey, the London Court tailor, lost the greater part of his trade because he could no longer trick out his work with the trimmings that the vain world demanded." [1] Owing to their regarding it as wrong to take an oath, even in a law court, it was impossible for them to prosecute anyone by whom they had been defrauded. Their refusal to take off their hats when brought into a court of law involved them in heavy fines and imprisonment. They were mostly recruited from the yeomen and trading classes, and none of them could boast of ancestors distinguished either intellectually or in commerce.

It frequently happens with religious sects that, when the first fervour has worn off, converts are made who join owing to hope of some worldly advantage. It might be suggested that such success as the Quakers had in business was due to hypocritical or self-seeking adherents of this kind. But such an explanation will not meet the case, owing to the fact that the successors of George Fox introduced a measure that effectually excluded insincere converts. This was the " birthright membership rule " introduced in 1737. In virtue of this rule, the wives and children of Quakers had all the privileges of membership, whether or not they belonged to the sect, including assistance in distress and free education for poor children. Henceforward conversion implied pecuniary liability, while expulsion had the opposite tendency. As a result conversion almost ceased, and there was " wholesale expulsion for trivial causes," especially between the years 1753 and 1820. In 1707, at Aberdeen, a member was expelled for " playing at gowf and other suchlike games." Between 1760 and 1825, " disownment " for " marrying out " was specially frequent. Consequently during this period the number of adherents of the sect rapidly diminished.[2]

Thus during the period of religious depression that followed the first enthusiasm, the careless and indifferent were being expelled. There was a

[1] *The Beginnings of Quakerism*, by W. C. Braithwaite. Macmillan & Co., 1912.

[2] *Religious Societies of the Commonwealth*, by R. Barclay. London: Hodder & Stoughton, 1876.

natural selection in virtue of which those who remained in the community were those who could best tolerate the formalism and peculiar habits inculcated by their creed. A further reason for believing that the success of Quakers in business was not due to insincere adherents is the fact that their business ability was shown from the first and at a time when they were being subjected to severe persecution.

After this brief review of the history of the Quakers we may proceed to consider what success they had in business.

In the first place it must be recognised that their honesty did give them an advantage in trading. Until the time of the Quakers, anyone going into a shop in England had to haggle and bargain about the price or stand the risk of being cheated. The Quakers held it dishonest to ask one price and to accept another. Hence they introduced fixed prices, a custom that gave them a substantial advantage, early in the history of the movement. George Fox, referring to the confidence in Quaker tradesmen thus produced, says in his journal, " and yt last they might send any childe and bee as well used as ymselves att any of there shoppes."

But honest men have failed in business before now. Other qualities besides honesty are wanted in commercial affairs. Quakers soon showed their ability in other forms of business than retail trading. Wherever the history of Quaker families is sufficiently known it appears that the power of making money often did appear, and that it only appeared at the time of or shortly after their conversion. It will be of interest to quote some examples.

A well-known Quaker family is that of the Gurneys. They are descended from Hugh de Gournay, who came over to England with William the Conqueror. Manors and lands were given to the family in Norfolk and Suffolk. They owned a vast territory in Normandy, which was lost to them in 1204. A descendant, Anthony Gurnay, married an heiress in the reign of Henry VIII. His estate was much diminished in his life-time by the sale of several manors. Henry Gurney, of West Narsham, who died in 1623, in his will bequeathed the reversion of £200 to his younger sons "so that none hould any fantastical or erronious opinions as adjudged by our Bishop or civill lawes." The beginning of interest in religious matters was not accompanied by any success in business, as it is recorded that the later generations of the family at West Barsham were in straitened circumstances. One of the sons of Henry Gurnay was a clergyman. Another, Francis, was a merchant who became bankrupt. The grandson of Francis, John Gurnay (later spelt Gurney), born in 1655, became a Quaker and was the founder of the wealth of the Gurney family. He was a silk merchant in Norwich. A descendant of his, Hudson Gurney, F.R.S., wrote in his diary in 1850 :

" John Gurney, 1670, was a thriving merchant of Norwich, worth £20,000,
" John Gurney, his grandson, died 1770, worth £100,000,
" and I, the grandson of the last, wind up 1850, worth £800,000."

The banking firm of John and Henry Gurney & Co. was founded in 1775. In 1838 this bank was described as of a power inferior to no banking establishment in Great Britain, that of the Bank of England alone excepted. It is of interest to notice that Hudson Gurney was disowned by the Friends for sending a subscription to a fund for volunteers in 1804.

Lest it should be thought that a Quaker becoming a banker was an exceptional case, it may be stated here that the number of English country banks founded by Quakers was far out of proportion to the numbers of this small community. English banking originated from some Italian merchants whose business it was to transmit to Italy the revenues drawn from England by the Pope. Lombard Street in London was named after them in 1318, but banking properly so-called only began to develop there about the year 1685. But it is mainly to the Quakers that belongs the credit of having founded a system

of banks in country towns in England that for long was "the wonder and admiration of Europe."

To return to our consideration of Quaker families, we may next consider the Barclays, of Ury in Scotland. They are descended from Theobald de Berkeley, who was living about 1140. One of his descendants, David Barclay, born in 1580, is recorded as having sold estates that had been in the possession of the family for 550 years. A later descendant was Robert Barclay the Apologist (1648–1690), one of the earliest of the Quakers. His controversial writings prove that his capacity for conscious reasoning was well developed. He showed no capacity in business, but his son was a successful London merchant. He had four grandsons, of whom three became bankers. Their descendants founded the important London bank till lately known as Barclay, Bevan, Tritton & Co.

Richard Hanbury (1647–1714) was one of the earliest members of the Society of Friends. His grandson John was known throughout Europe as "the greatest tobacco merchant of his day, perhaps in the world." The grandson of this last was the first Hanbury of the Quaker firm of Truman, Hanbury & Co., brewers.

As regards the Hoare family of bankers, it is known that a Major Hoare went from Devonshire to Ireland as an officer in Cromwell's army. He had five sons. Four of these did not become Quakers and no records are known as to their having shown business capacity. The remaining son, Francis, joined the Society of Friends, and afterwards was a merchant and a banker in Cork. Francis had a son, Samuel, who was a London merchant in the Irish provision trade. The son of Samuel, also named Samuel (1751–1825), was a banker in London and founded the present firm.

Tangye's pumps and engines are known all over the world. The founder of the firm, Sir Richard Tangye, a Quaker, was a grandson of an agricultural labourer of the Methodist persuasion. Richard's father, a miner, joined the Wesleyans and afterwards became a Quaker. It is recorded of Sir Richard Tangye that "he had a remarkable power of coming to a quick decision on any matter of business, however complicated, which was put before him. . . . He would himself have been at a loss, not infrequently, to explain the process by which he arrived at the decision." Thus instead of relying on conscious reasoning, he had a power of deciding intuitively similar, perhaps, to the power often possessed by doctors of making a rapid diagnosis without being able to give reasons for their decision.

More often the evidence available as to the origin of the business ability of Quakers is less complete. The records show that many large business firms were founded by them, and there is no evidence that their non-Quaker ancestors had any conspicuous success in commerce.

Quakers early obtained a reputation for shrewdness in business affairs. This perhaps was shown especially in their preference for businesses that supplied articles or conveniences for which there was a great public demand. The predominance of Quaker bankers in country towns in England has already been mentioned.

At one time almost every town in England had a Quaker wine merchant or a Quaker brewer or a Quaker maltster. Among Quaker breweries are, or rather were, Walkers, Parkinson, Allens, Hanbury, Barclay and Perkins. Owing to the temperance movement, in which Friends played an important part, Quakers have now given up the trade in alcoholic beverages.

Horniman, a Quaker, a self-made man, born in 1803, was retailer of tea by which he made a fortune and died worth more than £300,000.

Zephaniah Fry, born in 1658, was a follower of George Fox. His grandson, Dr. Joseph Fry, purchased a patent for making cocoa and founded the firm of S. F. Fry & Sons, cocoa manufacturers, of Bristol. Francis, the grandson of Dr. Joseph Fry (1803–1886), greatly enlarged the firm. He was a

pioneer of railway enterprise and played an active part in the agitation for abolition of the slave trade.

Rowntree & Co., of York, is another well-known cocoa manufacturing firm of Quaker origin.

The firm of Cadbury, cocoa manufacturers, of Birmingham, was founded by a Quaker of whom it is recorded that a piano was never allowed in his house, and who never sat in an easy chair till he reached the age of 60. He had twelve employees when he gave over the business to his two sons. Under their hands the business grew till it employed 3,400 workmen.

Quakers early recognised the wide demands for medicines. Names of well-known firms of chemists and druggists of Quaker origin are shown in the following list:

	Date of founding or (in brackets) dates of life of founder.
Allen & Hanbury	(Allen, 1770–1843.)
Corbyn	(T. Corbyn, 1711–1791.)
T. Bell & Sons	(J. Bell, 1774–1849.)
Howards	1793.
Reynolds & Bransome	1816.

The son of J. Bell was the founder of the Pharmaceutical Society.

Perhaps the most remarkable proof of the foresight and shrewdness of the Quakers is furnished by the part they played in the introduction of mechanical transport. "The railway system may almost be said to owe its existence to their enterprise." Friend Edward Pease (1767–1859) aided George Stephenson in his experiments. It is recorded that he watched Stephenson running alongside his first locomotive stoking its furnace. The sight of this imperfect machine encouraged him to furnish capital for the first railway, namely the Stockton and Darlington (1821), and also to provide capital for the first factory for steam locomotives. Friends also were the backbone of the Liverpool and Manchester line. Friend Francis Fry, the cocoa manufacturer, has already been mentioned as a pioneer of railway enterprise. Friend John Ellis (1789–1862) promoted the Leicester and Swannington Railway and was the originator of the Midland Railway, of which his son, E. S. Ellis (1817–1879), was afterwards chairman. William Hutchinson was another Quaker manager of the same railway. "In the matter of rails Friend Ransome devised the best form of chair for holding them, and Charles May the compressed oak trenails that pin them to the ties. When the lines began working under a cumbrous system of passenger booking, continued from the coaching days, it was Friend Edmundson who devised the present effective system of railway tickets, and likewise invented the machine in general use for stamping them, and it is Friend Bradshaw who still enlightens the public as to train movements by his Time Tables." [1]

Quakers were also prominently connected with early steam navigation. Sir Samuel Cunard (1787–1865) was of Quaker descent. Friend James Beale sent the first steamer across the Atlantic. This was the *Sirius*, which started on its voyage on March 31, 1838. Friend Joseph Robinson Pim (1787–1858) founded the St. George Steam Packet Co., running between England and Ireland, in 1824. He was described by James Clerk in 1835 as "an Irish Friend well known as principal manager of, I suppose, nearly half the steam packets in the kingdom." [2]

[1] *The Friends—Who they are and what they have done*, by William Beck. Edward Hicks, London, 1893.

[2] *Irish Friends and Steam Navigation*, article in *Journal of the Friends' Historical Society*, vol. xvii, No. 4, 1920, p. 105. This is based on an article in the *Journal of the Cork Historical and Archæological Society* for 1917, vol. xxiii.

The following is a list of other well-known Quaker firms :

Name.	Business.	Date of founding or (in brackets) dates of life of founder.
R. & J. Beck	Opticians	(R. Beck, b. 1827)
Thomas Hoyle & Sons	Dyers	Before 1788
Hoyle	Calicoes	
Christy	Hats	(1777–1846)
Ashworth's	Cotton manufacturers	(H. Ashworth, 1794–1880)
Bright & Co.	Cotton spinning	(Jacob Bright, b. 1775. John Bright, 1827–1841). The firm in 1809
Were & Fox	Woollens	(Peter Were was a serge maker in 1686. T. &. R. Were & Co. in 1752. Thomas Fox joined in 1768)
T. Southall	Chemists and tea	1766
David Barclay	Linendraper	(1728–1809)
Joseph Fry	Banker and tea shipper	(1777–1861)
Fry & Sons	Bankers	In 1806
Tangye Brothers	Engineers	1852
George Thomas	Merchant of Bristol	(1791–1869)
John Hanbury	Tobacco merchant	(1700–1758)
Jeremiah Head	Ipswich banker	(d. 1734–5)
Bryant & May	Matches	c. 1840
Reckitt & Sons	Blue and starch	1840–1850
Huntley & Palmers	Biscuits	1841
Ransome	Agricultural implements (Ipswich)	(Robert Ransome, 1753–1830. James Ransome, 1728–1849)
Darby	Iron casting	1735
Sir C. Cunard	Steamships, Cunard S.N. Co.	(1787–1865) 1838–1840
Boulton & Watt	Steam engines	
Rt. Hon. W. B. Forster	Woollens	(1818–1886)
John Overend	Bill broker	(1769–1832)
William Cookworthy	Porcelain manufacture	First discoverer of English China clay, about 1745

That such a number of important firms should have been founded by so small a community is a remarkable fact. Quakers have never formed more than a small fraction of the population. The only statistics known to me on this point are the following :

In 1661 the number of Quakers living in Great Britain was 30,000 to 40,000
,, 1669 ,, ,, ,, ,, ,, ,, ,, ,, 60,000
,, 1727 ,, ,, ,, ,, ,, ,, ,, ,, 60,000

(Between the years 1727 and 1820 there was a rapid decrease in numbers, as previously explained.)

In 1862 the number of Quakers living in Great Britain was 17,034
,, 1876 ,, ,, ,, ,, ,, ,, ,, ,, ,, 17,000
,, 1920 ,, ,, ,, ,, ,, ,, ,, ,, ,, 19,130

Besides their ability in business, the Quakers showed commendable initiative in works of public utility. They originated most of the philanthropic movements of the nineteenth century. Facts in support of this statement are as follows:

In 1758 Quakers refused to own slaves. In 1787 the movement for the abolition of slavery was begun by Quakers, and was carried to success after many years of strenuous agitation.

A Quaker, Dr. Thomas Hodgkin (1798–1866), founded the Aborigines Protection Society.

A Quaker, Samuel Bowly (1802–1884), was the first advocate of temperance reform.

Elizabeth Fry and Stephen Grellet, both Quakers, began the work of prison reform in 1813.

William Allen (1770–1843) and Joseph Lancaster (1798–1838), both Quakers, were strenuous workers for the extension of education.

Quakers played a prominent part in the agitation for the abolition of capital punishment for trivial crimes. As a result of this agitation the death penalty was abolished for one hundred and sixty offences.

Peter Bedford, a Quaker (1780–1846), originated soup kitchens, clothing clubs, and other means of aiding the poor.

A Quaker, William Tuke (1732–1822), commenced the movement for humane treatment of lunatics. The York Retreat, an institution for the insane, was founded by Quakers in 1792.

Dr. Dimsdale, F.R.S., a Quaker, played a prominent part in popularising inoculation for smallpox.

A study of the history of Quaker families shows that their business ability is often inherited. In some of the instances quoted the son or grandson has shown greater ability than the original founder of the firm. It has sometimes happened that individuals of later generations have shown intellectual or literary ability rather than business aptitude. It might not be anticipated that a small community of religious fanatics should produce men of sufficient scientific ability to be elected Fellows of the Royal Society of London in greater proportion than the non-Quaker population. But, contrary to such anticipation, this is what has actually happened.

The following table shows the total number of inhabitants of Great Britain elected F.R.S. from the foundation of the Society in 1663 to the year 1900. The numbers of Fellows who were Quakers or of Quaker descent are shown separately. The numbers for the Quaker population are (in two cases) guesses based on the statistics and facts previously given.[1]

Period.	Elections to Royal Society of		Average population of Great Britain during each period.	Probable average number of Quakers living in Great Britain during each period.
	Non-Quakers.	Quakers or persons of Quaker descent.		
1663–1700	449	4	5,500,000	40,000
1701–1750	728	8	9,500,000	? 30,000
1751–1800	948	6	12,500,000	? 20,000
1801–1850	1,296	17	22,108,000	17,000
1851–1900	817	19	33,642,000	17,000

[1] *Friends and the Learned Societies*, article in *Journal of the Friends' Historical Society*, vol. vii, No. 1, First month (Jan.) 1910, p. 30, and *Record of the Royal Society*, 3rd Edition, 1912 (Oxford University Press).

From these figures we can calculate the rate at which Quakers and non-Quakers have been elected F.R.S. per million inhabitants. The results are as follows:

Period.	Elections to the Royal Society per million inhabitants of	
	Non-Quakers.	Quakers.
1663–1700	81	100
1701–1750	76	? 266
1751–1800	75	? 300
1801–1850	58	1,000
1851–1900	24	1,117

Thus between the years 1851 and 1900, a man had about forty-six times more chances of being elected F.R.S. if he was a Quaker, or of Quaker descent, than was the case if he belonged to the general population. It is obvious that, as the total actual numbers of elections were small, there is no certainty that if ten times or a hundred times as many elections had been made each year, the same proportion would hold good.

Quakers who have had the distinction of being elected F.R.S. include Thomas Young (1773–1829), the originator of the undulatory theory of light; Luke Howard (1772–1864), a pioneer in meteorology; John Dalton (1766–1844), who first made an estimate of the relative weights of the atoms and thereby placed the atomic theory on a secure basis; Joseph Jackson Lister (1786–1869), who made important improvements in achromatic lenses; his son, Lord Lister (1827–1912), the discoverer of antiseptic surgery; and Silvanus Thompson (1851–1916), the well-known electrician. A list of Quakers and persons of Quaker descent who have been elected F.R.S. is appended.

A contrast to the history of the Quakers is afforded by that of the Mennonites. This sect, whose creed was formulated in 1632, had theological beliefs that were almost identical with those of the Quakers. They resembled the Quakers also in their endogenous marriage customs, in their objection to military service and war, in having been subjected to persecution, in their moral and puritanical habits, and in being restricted in numbers. They have spread chiefly in America, Holland, Germany, and Russia. In matters of belief and custom they differed from Quakers solely, so far as available information goes, in lacking most, if not all, of the accessory quasi-rational beliefs that distinguished the Society of Friends. Further research on this point is, however, needed. Though they are known as honest and industrious workers, no reason could be found in the books about them that I have consulted for ascribing to them any special mental ability in business or otherwise.

The foregoing information was chiefly obtained at the Friends' Reference Library, 136 Bishopsgate, London, whose officials gave me most kind and valuable help. The remarkably full records of the history of the sect contained in this library would permit a far more detailed study of the mental development of the Quakers than has been here attempted.

The following list is mainly compiled from an article in *The Journal of the Friends' Historical Society* (vol. vii, No. 1, First month 1910, p. 30). No information is available to me as to whether any Friends have been elected F.R.S. since 1915.

LIST OF FELLOWS OF THE ROYAL SOCIETY OF LONDON WHO ARE QUAKERS OR OF QUAKER DESCENT

Name.	Born.	Died.	Elected.
Sir John Finch	1626	1682	1663
Anthony Lowther	—	1672	1663
Richard Lower, M.D.	1631	1691	1667
William Penn (? Friend)	—	—	1681
Total 1663–1700 ... 4			
Richard Mead, M.D. (books on the plague and on poisons)	1673	1754	1703
Fettyplace Bellers (lawyer)	—	—	1711
John Bellers (social reformer)	1654	1724	1718
George Graham (clockmaker, writer on magnetism and astronomy)	—	1751	1720
Silvanus Bevan	—	—	1725
Peter Collinson (botanist)	1693	1768	1728
Thomas Birch, D.D. (historian; parents Friends)	1705	1766	1734
Richard Brocklesby, M.D.	1722	1797	1746
Total 1701–1750 ... 8			
John Fothergill, M.D. (founder of Ackworth School)	1712	1780	1763
Thomas Dimsdale, M.D. (popularised inoculation for smallpox)	1712	1800	1769
John Coakley Lettsom, M.D. (naturalist)	1744	1815	1773
Jeremiah Dixon (astronomer)	1733	1778	1790
Mark Beufoy	1764	1827	1790
Thomas Young, M.D. (originator of undulatory theory of light and Egyptologist)	1773	1829	1794
Total 1751–1800 ... 6			
Lewis Weston Dillwyn (parents Friends, M.P. for Glamorganshire)	1778	1855	1804
William Allen (chemist and philanthropist)	1770	1843	1807
Robert Willan, M.D.	1757	1812	1809
John Sims	1749	1831	1814
Michael Bland	—	1851	1816
Hudson Gurney	1775	1864	1818
Luke Howard (a pioneer in meteorology)	1772	1864	1821
John Dalton (author of the theory of atomic proportions)	1766	1844	1822
John Scandrett Harford (left Friends)	1785	1866	1823
Richard Phillips (chemist)	—	—	1827
James Cowles Pritchard, M.D. (author of *Natural History of Man*, etc.; left Friends)	1786	1848	1827
William Phillips (mineralogist and geologist)	1775	1828	1827
Joseph Jackson Lister (investigator of achromatic lenses; memoir in *Phil. Trans.* for 1829)	1786	1869	1832
William Allen Miller	1817	1870	1845
William West	—	—	1846
Robert Were Fox (magnetism and geology)	1798	1877	1848
John Fletcher Miller (meteorologist and astronomer)	1816	1856	1850
Total 1801–1850 ... 17			
Charles May	—	—	1854
Isaac Fletcher (left Friends)	—	—	1855

LIST OF FELLOWS OF THE ROYAL SOCIETY OF LONDON—*Continued*

Name.	Born.	Died.	Elected.
Joseph Lister, afterwards Lord Lister (discoverer of antiseptic surgery; left Friends)	1827	—	1860
Daniel Oliver (keeper of Herbarium at Kew)	—	—	1863
William Pengelley (geologist)	1812	1894	1863
Daniel Hanbury (chemist; parents Friends)	1825	1875	1867
Edward Burnett Tylor (anthropologist; left Friends)	—	—	1871
Wilson Fox, M.D. (of Quaker descent)	1831	1887	1872
John Eliot Howard (chemistry of quinine)	1807	1883	1874
Henry Bowman Brady	1825	1891	1874
W. E. Forster (politician; left Friends)	1818	1886	1875
John Gilbert Baker (botanist; late keeper of Herbarium at Kew)	—	—	1878
George Stewardson Brady, M.D.	—	—	1882
Sir Jonathan Hutchinson, M.D.	—	—	1882
Sir Edward Fry	—	—	1883
John Theodore Cash, M.D.	—	—	1887
Silvanus Phillips Thompson (author of works on electricity)	1851	1916	1891
Arthur Lister	1830	1908	1898
Joseph Jackson Lister	—	—	1900
Total 1851–1900................19			
Ralph Allen Sampson	—	—	1903
Frank Wall Oliver	—	—	1905
Joseph Barcroft	—	—	1910
A. S. Eddington	—	—	1914
L. Doncaster	—	—	1915

REVIEWS

A Treatise on Probability. By J. M. KEYNES, Fellow of King's College, Cambridge. [Pp. xi + 466.] (London: Macmillan & Co., 1921. Price 18*s*. net.)

THE subject of probability is one which has attracted the attention of many famous mathematicians. Some have given it only casual notice, whilst others have devoted much time to it. The connection with games of chance is doubtless responsible to some extent for the wideness of its appeal. The mathematician has been frequently tempted, however, to regard probability as a purely mathematical conception and to neglect the logical basis upon which it is fundamentally based. The neglect of this aspect has been responsible for some of the confusion and errors into which mathematicians have at times been led. Probability must in any case be relative to certain premisses, and a modification of these will modify the value of the probability. Thus to take a simple example quoted by Mr. Keynes: "If a chord in a circle be drawn at random, what is the probability that it will be less than the side of the inscribed equilateral triangle?" On the assumption that it is indifferent at what point one end of the chord lies, the probability is $\frac{2}{3}$: if it is indifferent in what direction the chord lies the probability is $\frac{1}{2}$, whilst if it be supposed that the middle point of the chord is chosen at random, the probability is $\frac{3}{4}$. The probability is therefore indeterminate without a more precise definition of the initial premisses.

Another example in which it is not at first sight quite so apparent that the initial premisses are not complete is the following. Mathematicians have generally assumed that the probability of two witnesses speaking the truth, who are independent in the sense that there is no collusion between them, is the product of the probabilities that each of them separately will speak the truth. If, therefore, x and y are the probabilities that two independent witnesses x and y speak the truth, and if they both agree in a particular statement, the probability that the statement is true is generally taken to be $xy/xy + (1 - x)(1 - y)$. For xy is the chance that they both speak the truth and $(1 - x)(1 - y)$ the chance that they both speak falsely. A critical examination of this problem by Mr. Keynes shows that herein it is tacitly assumed that *any* answer to a given question is, *a priori*, as likely as not to be correct, which cannot in general be the case.

As a further example, of a somewhat different nature, of the errors into which it is so easy to be led, we may instance Bernouilli's Theorem, frequently used in problems of statistical inference. This theorem asserts that, if the probability of an event's occurrence under certain conditions is p, then if these conditions are present on m occasions the most probable number of the event's occurrence is mp (or the nearest integer to this), i.e. the most probable proportion of its occurrence to the total number of occasions is p. This theorem has been regarded by some as having universal validity. Mr. Keynes shows, on the other hand, that it holds only under certain conditions, which are generally not fulfilled. For the theorem to be valid, the initial data must be of such a character that additional knowledge as to the proportion of failures and successes in one part of a series of cases is altogether irrelevant to our expectation as to the proportion in another part. This is rarely the case, for the initial probability is generally founded upon experience, and it is liable to modification in the light of further experience.

Mr. Keynes formulates a notation and bases upon it a calculus of probabilities by means of which such problems may be discussed with great facility and the nature of underlying assumptions made clear. The conception of probability is critically discussed, and emphasis is laid on the fact that it is not always possible to assign a numerical value to the probability of a conclusion based upon a certain premiss.

In this volume the logical basis of the subject is kept in the forefront and systematically developed. The ideas which the author advances are applied to such questions as the weights of arguments, laws of error, inference, induction and analogy, and many particular problems of historical interest are discussed in detail. A difficult subject has been handled in so clear a manner that the reader, whilst being made to realise how errors have crept into much of the previous work dealing with questions of probability, is left wondering that these errors could ever have been made. The author has, in fact, provided the most logical exposition of the whole subject that has yet been given. The volume is a masterpiece of clear exposition which should considerably enhance the author's already high reputation. A very complete bibliography on the subject of probability is given at the end of the volume. H. S. J.

Three Lectures on Fermat's Last Theorem. By L. J. Mordell. [Pp. v + 60.] (Cambridge: At the University Press, 1921. Price 4*s*. net.)

In this small book Mr. Mordell has given an interesting account of Fermat's last theorem. The theorem states that if n is a positive integer greater than two, the equation

$$x^n + y^n = z^n$$

cannot be satisfied by integer values of the unknowns x, y, z unless one of them is zero. It is of course well known that the equation

$$x^2 + y^2 = z^2$$

possesses an infinite set of solutions in integers.

The theorem was stated by Fermat in the first half of the seventeenth century. Fermat, in the course of reading a new edition of Diophantus's work which was published 1621, noted down in the margin a number of theorems. It is a tantalising fact that he entered the theorem—now called Fermat's last theorem—in the margin and remarked that he had found a truly wonderful proof of it, but that the margin was too small to contain it. In spite of the efforts of the greatest mathematicians, including Euler, Legendre, Gauss, Abel, Dinchlet, and Cauchy, and in spite of the stimulus of a prize of a hundred thousand marks, no proof for all values of n has yet been found.

In the course of the book, the author deals with various aspects of the problem and treats the special cases $n = 3$ and $n = 4$, 5 and 7. The important work of Kummer, which proved the theorem for values of less than 100 (except of course $n = 2$) is carefully explained. The several other lines of attack due to Libri, Legendre, and Wendt are briefly mentioned.

The substance of this little book was given in London in the form of lectures in March 1920, at Birkbeck College. The subject is, of course, intrinsically one of great interest; but the author is to be congratulated on having seen the opportunity of using the theorem as a focus for examples of different kinds of mathematical reasoning and of constructing so interesting an account of its historical development. D. M. Wrinch.

An Introduction to Projective Geometry. By L. N. G. Filon, M.A., D.Sc., F.R.S. Third Edition. [Pp. vii + 253.] (London: Edward Arnold & Co. Price 7*s*. 6*d*.)

The third edition of Professor Filon's well-known textbook has been seen through the Press by Mr. T. L. Wren, Reader in Geometry in the University

of London, and does not differ materially from previous editions. A new chapter has been added on Point-Reciprocation, which is the special case of polar reciprocation with regard to a conic arising when the conic is a circle. Its importance is due to the fact that by it *metrical* properties may be obtained; also it is considered that the student is most easily familiarised with the general notion of reciprocation by examining this particular case in some detail.

Another novelty is introduced in chapter ii, where Menelaus' and Ceva's Theorems are deduced from the proposition that the continued product of ratios of segments of the sides of a triangle is unaltered by projection.

A useful set of Miscellaneous Examples has been added at the end.

In comparison with such a book as Enriques' *Geometria Proiettiva* the general treatment may seem lacking in breadth, and the book rather over-burdened with detail; the author has in mind, of course, the elementary student, and is somewhat circumscribed by the peculiarities of the syllabuses of the University of London, by which involution is postponed to a relatively late stage; but his book remains one of the best introductions to the subject that there is in English, and may be safely recommended to the serious student.

F. P. W.

Eléments d'Analyse Mathématique. Par PAUL APPELL. Quatrième édition. [Pp. x + 715.] (Paris: Gauthier-Villars et Cie., 1921. Price 65 fr.)

M. PAUL APPELL, in 1898, founded the first edition of this book, a general course of mathematics for engineers and physicists, on his lectures at the École centrale des Arts et Manufactures. The fourth edition, which has just appeared, contains many additions and is practically a new book.

The first impression that one gets on glancing through its pages is how very much better it is than corresponding books in English. Its scope is, of course, a great deal larger than that of most of our practical introductions to the calculus; in fact, one would expect a strike among science students in this country who were required to know about the curvature and torsion of twisted curves and properties of asymptotic lines on a surface. But in the more elementary parts the superiority is obvious. Take, for instance, his treatment of series in chapter vi. Use is made in plenty of our intuitional notion of a curve (e.g. in the proof of the mean value theorem), but when he gets to the question of differentiation and integration of a power series he does not give a shoddy half-proof of his proposition; he assumes it and says so. "C'est là un théorème très important *que nous admettrons*" (italics in the original).

The contents include, as has been said, a fair amount of the elements of differential geometry, in the plane and in three dimensions; there is a chapter on Stoke's Theorem and on Green's Theorem, and five on differential equations. Many examples are worked out in detail in the text, which has the effect of increasing the size of the book and making it rather difficult to read through. There is no doubt, however, that not only the mathematical physicist but also the student of pure mathematics, in his early stages, would benefit by reading it.

F. P. W.

A Study of Mathematical Education, including the Teaching of Arithmetic. By BENCHARA BRANFORD. 2nd Edition. [Pp. xii + 420.] (Oxford: At the Clarendon Press, 1921. Price 7s. 6d. net.)

THIS book is a study, from the point of view of general educational principles, of the methods which should be employed in mathematical education. The author has had a long experience of school and college education, and has combined mathematical knowledge with educational ideas. The importance of mathematical laboratories well stocked with clay, cardboard, wire and wooden models in mathematical education, both at school and at the University, is only now being realised. It is of course evident that a teacher of mathe-

matics who knows nothing about the psychology of his pupils is in a very unsatisfactory position. It is unfortunate that progress in the technique of teaching mathematics is so slow. But the number of people combining an interest and competence in teaching and mathematical knowledge is very small. The book under review is one of a few isolated attempts to draw a picture of the possibilities in the technical development of mathematical teaching, when the history of mathematics and the mathematical ideas themselves are presented with due reference to the psychology of the learners. With an advance in the technique at the disposal of researchers in psychological domains, and the further development of the comprehensive modern hypothesis of the behaviour of the mind, we may expect important results. The field when it is ripe for scientific methods will be a most important one. In the meantime, the book before us will doubtless be a stimulus towards some attempt at the development of mathematical teaching on psychologically sound lines.

This edition contains a large amount of new material.

D. M. WRINCH.

Handbook of Meteorology. By JACQUES W. REDWAY. [Pp. 294.] (New York: John Wiley & Sons; London: Chapman & Hall, 1921. Price 24s. net.)

THIS work is divided into two parts. In Part I the general facts of the meteorology of North America are set out, together with sufficient theoretical physics for a general understanding of the processes which give rise to everyday weather; Part II contains descriptions of instruments and observational methods, the treatment of this subject being unusually thorough. In the theoretical portions of the work there are good expositions of the formation and importance of "lids" in the atmosphere, due to inversions of temperature, and also of the part played by dust particles in cloud formation—the latter being a field of inquiry in which the author has done original work. The causes of precipitation are less clearly set out, and a correct idea of what has been achieved in this subject is not conveyed. For instance, with reference to unusually heavy falls of rain or "cloud bursts," after pointing out that the atmosphere over any particular place probably never contains sufficient water vapour to produce the rainfall observed on these occasions, it is suggested that "a cloud-burst may be derived from the contents of a waterspout carried inland for a long way and dumped upon the nearest mountain crest which has a temperature low enough to chill it." There is, however, an excellent chapter on cloud forms, abundantly illustrated by cloud photographs taken by Dr. C. F. Brooks.

As an introduction to meteorology for American students this handbook should meet with success; for English students it will be found wanting in that the instruments described are often different from those in general use in this country, while the remarks about weather prediction have little application to the region of the British Isles.

E. V. NEWNHAM.

Principles of Radio Communication. By J. H. MORECROFT, Associate Professor of Electrical Engineering, Columbia University, assisted by A. PINTO and W. A. CURRY. [Pp. x + 935, with 788 diagrams and illustrations in the text.] (New York: John Wiley & Sons; London: Chapman & Hall, Ltd., 1921. Price 45s. net.)

AN excellent general textbook of radio telegraphy and telephony containing all the essential features of similar standard works, with much good additional matter dealing with the modern phases of radio work. In accordance with usual practice in books of this kind, the opening chapters deal with general electrical considerations as far as they affect the main subject, but they differ from most such books in that the electron is considered as the viewpoint for all purposes. The student is thus familiarised at once with modern thought

and ways of regarding electrical phenomena in a manner that cannot but be most helpful at a later stage in his studies when, as when dealing with the thermionic valve, the electron can no longer be disregarded.

The twelve chapters into which the volume is divided show a good balance in the treatment of different sections of the subject. They are illustrated with numerous useful diagrams and curves of experimental data. Throughout the work the experimental side of the subject is by no means neglected, and the importance of such means of verifying theoretical conclusions is everywhere emphasised.

For readers in this country it is necessary to bear in mind that the author is a Professor in an American University, with a consequent underlying natural preference for American apparatus. Some more references and illustrations of types of instruments used in other countries might with advantage have been included, particularly in the last chapter on experiments. This, however, coupled with the rather indifferent printing of some of the half-tone illustrations, is the only point which can be criticised with any seriousness. For convenience in reference, it is perhaps an advantage if the figures are numbered consecutively through the book, but this is a point on which personal opinions may differ.

After giving the general theory underlying radio methods as leading up to "spark" telegraphy, the author considers in great detail the thermionic vacuum tube, and the other methods of producing continuous waves for telegraphic and telephonic signalling in Chapters VI to VIII, while the Theory and Design of Antennæ, Wavemeters, and Amplifiers are dealt with in the following chapters (IX to XI). Particular mention may be made of the last chapter (XII), which is devoted entirely to descriptions of various laboratory tests that can be carried out by students in conjunction with their ordinary studies in this subject.

PHILIP R. COURSEY.

The Scientific Papers of the Honourable Henry Cavendish, F.R.S. Vol. I: The Electrical Researches, edited by JAMES CLERK-MAXWELL, F.R.S., Cavendish Professor of Experimental Physics in the University of Cambridge. Revised by SIR JOSEPH LARMOR, F.R.S., M.P., Lucasian Professor of Mathematics. Vol. II: Chemical and Dynamical, edited by SIR EDWARD THORPE, F.R.S., with contributions by DR. CHARLES CHREE, F.R.S., SIR FRANK DYSON, F.R.S., SIR ARCHIBALD GEIKIE, O.M., F.R.S. and SIR JOSEPH LARMOR, F.R.S. [Pp. xxvii + 452, xii + 496.] (Cambridge: at the University Press, 1921. Price £6 net.)

IN these two handsome volumes we have at last the most complete account possible of the work of Cavendish. The Electrical Researches, which were published in 1879 under the editorship of Clerk-Maxwell, are reprinted in volume i, as revised by Sir Joseph Larmor. Volume ii deals with the chemical and dynamical researches. It must have been a task of peculiar difficulty—as it was indeed found by Maxwell originally—to obtain a satisfactory arrangement and exposition of Cavendish's work as shown by his manuscripts; for Cavendish was exceptionally prone to postpone any preparation of his work for the Press, under the impetus of some new line of thought which was beginning to take shape in his mind. In certain important respects, volume i has brought Maxwell's account more up to date, by the addition of references and footnotes. The headings of the chapters and sections and the headlines of the pages give a distinctly better view of the nature and contents of Cavendish's electrical work. Moreover, as is pointed out in the preface, an account of his theory of a universal electrical fluid acting upon and with material substances solely by attraction is of very considerable interest at the present day, if only as a contrast to the more modern views of the ether and its relation to molecules capable of polarisation. This volume contains an interesting portrait of Cavendish, which is believed to have been constructed

from surreptitious sketches at a dinner of the Royal Society Club, the original being in the print-room at the British Museum.

The first chapter abounds with interesting biographical details of Cavendish. The notes by Maxwell which are reprinted at the end of the volume contain many pieces of work of peculiar value in electrostatic theory which are not readily to be found elsewhere. The general impression derived from reading the work is an admiration for the enormous care and accuracy of Cavendish as an experimenter, and if an example might be quoted we would cite the extraordinarily accurate determination which he gave of the capacity of an electrified circular disc. This, of course, has often been quoted before, but it is in no way more remarkable as regards accuracy than many other experiments mentioned in this volume.

Volume two opens with a photograph of Cavendish's house at Clapham. Most of the contents have been derived from manuscripts preserved at Chatsworth. The work was begun several years ago, but the printing has been much delayed. The greater part of it deals with chemical subjects, but, as the editor points out, the general impression that Cavendish was pre-eminently a chemist is erroneous, although his achievements in chemistry are undeniably very great. For in these volumes it becomes clear that he made important contributions to every branch of physical science then in existence. The treatment in the present volume has been arranged as nearly as possible on the lines of Maxwell's treatment of the electrical researches. There is a valuable commentary showing the relation of his work to the general state of knowledge in his time and to the subsequent advances which have taken place. Several of his papers, previously unpublished, indicate that Cavendish anticipated in some cases the results of later experimenters. For example, there is now no longer any doubt that he was the real discoverer of arsenic acid, and that his method is the method in use at the present day. He also was obviously aware of some of the laws which lie behind the phenomena of gaseous diffusion. A point which stands out rather prominently is the clear nature of his views regarding the Conservation of Energy and the degradation of energy into heat. He indeed anticipates much of Helmholtz's famous essay of 1847. Sir Joseph Larmor has made clear the relation of his ideas to those of Newton and Daniel Bernoulli and other writers to whom Helmholtz expressed his obligations.

Sir Frank Dyson points out the completeness of Cavendish's knowledge of the tidal retardation of the diurnal rotation of the earth and the principle of rotational torque and energy exchange which are relevant. He was indeed aware of the amount of deviation of a ray of light passing near the sun according to the Newtonian corpuscular theory as extended by Mitchell to include gravitation of the corpuscles.

Dr. Chree gives an account of the magnetic work of Cavendish. Maxwell devoted only a very short summary of this part of his work. Dr. Chree shows that Cavendish anticipated many subsequent observers in, for example, his determination of the best form of dip-needles, by tracing the influence of bending the needle on the observed value of its inclination, and as regards other sources of error.

Sir Archibald Geikie indicates the extent of Cavendish's contributions to Geology.

These two volumes are a very notable addition to the rapidly increasing list of scientific memoirs published by the Cambridge University Press.

D. WRINCH.

The Reign of Relativity. By VISCOUNT HALDANE. [Pp. xxiii + 427.] (London: John Murray, 1921. Price 21*s*. net.)

FRANKLY, this is not a book for those who still hope to see the meaning of Einstein's new work put into popular terms. The theme is that of the

relativity of all knowledge, and the discussion roams over the whole realm of science, religion, politics, and literature.

It is possible to find some analogy between the mathematical theory developed by Einstein and the theory of knowledge here presented. In the realm of physics matter, objective or absolute, is that which persists in all possible views of it. The theory of relativity in physics might well be called a theory of the absolute. For the laws of physics are taken to be just such laws as are true, no matter what system of describing them may be adopted. In the general theory of knowledge developed by Lord Haldane, knowledge, the absolute, is to be inferred from a consideration of all possible points of view. If that which is said of knowledge from one point of view contradicts that which is said from another point of view, then there is inaccuracy somewhere. Science and religion, for instance, cannot be contradictory, though they may not say the same thing; they are approaching experience from different aspects. Here comes in a difference between the case of physical relativity and that of the general problem considered by Lord Haldane. In the former we are dealing with the same phenomena looked at from various points of view. In the latter, the various points of view correspond to the selection of different groups of phenomena. Religion is the consideration of one field of human experience, politics of another. These fields overlap, but are not co-extensive.

"The final and complete truth cannot be less than a systematic whole of knowledge within which all particular and partial outlooks have their places as levels or degrees in knowledge. It is therefore from above, and not from underneath, from what is concrete and individual, and not from abstractions only derivative from it, that we must seek to inquire, if we would strive to realise the ideal of bringing the whole under a final and adequate conception, and of so obtaining the whole truth."

The consideration of such an ideal is clearly related to a highly developed view of the State as a community organised for the allowing of the fullest opportunity for the development of all kinds of thought and activity in harmonious co-operation. The healthy community is that in which all contribute their best to the organic whole. An autocracy cannot be consistent with such an ideal, for it involves the domination of many points of view by one. The leaders in the ideal community will not be men intent on getting something done, but on the development of all the variety of power of thought and action latent within its members. Education of the most liberal type will be the dominant note of such a community.

With this thought of the relativity of all particular modes of thought, Lord Haldane traverses the various views that have been put forward as to the nature of the real, from the Greeks to the New Realists. We are taught to see the history of philosophy as evolving progressively a lasting view of the foundation of reality, a view remaining substantially constant in varying forms, despite temporary changes due to alterations in modes of approach attributable to periods and circumstances.

This is not the place for a critical discussion of Lord Haldane's philosophy. It is a generous and warm-hearted piece of thought. It finds room for each within its borders as rendering to the whole of knowledge a contribution which no other ran render.

In his discussion of the particular phase of relativity which has brought the word to the notice of the world at large in recent days, Lord Haldane seeks bravely to give his impression of what the mathematicians are talking about, but he does not succeed in extracting the essence of the matter entirely from the husk of mathematical terminology, and, after all, confesses that it is not easy to do so. The mathematicians who have essayed to give a popular exposition, and have received little recognition of the success of their efforts, will take new courage from his words.

E. C.

The Physical Properties of Colloidal Solutions. By E. F. BURTON, B.A., Ph.D. (Monographs on Physics edited by Sir J. J. THOMSON and FRANK HORTON). Second Edition. (Pp. viii + 221, with 18 illustrations.) (London: Longmans Green Co., 1921. Price 12*s.* 6*d.* net.)

THIS excellent monograph is so well known that it is hardly necessary to do more than to draw attention to the publication of the new edition. The work has been subjected to considerable revision. That the author has kept it well abreast of current research is shown, for example, by the treatment of the equilibrium space distribution of emulsoid particles in relation to their electric charge, and the bearing of this on Perrin's classical experiments. It is thus demonstrated that, whilst Perrin's conclusions are quite valid, they apply only to a very limited range near the surface. Similarly, the chapter on the coagulation of colloids gives a *résumé* of the subject so far as it is known and understood in the light of recent research work.

The absolute necessity for a sound physical basis for colloid chemistry is shown by the existence of such fundamental problems as the magnitude of the colloid electric charge, the equilibrium size of colloid particles, the quantitative formulation of the Brownian movement, the optical properties as a function of the nature, size, and shape of the particles, the nature of the Helmholtz double-layer, and other problems.

It is only as progress is made in these fundamental quantities that we can expect our knowledge of the manifold applications of colloid chemistry to emerge from the empirical stage and to develop along rational lines.

W. C. McC. LEWIS.

An Introduction to Biophysics. By DAVID BURNS, M.A., D.Sc. [Pp. xiii + 435, with 85 illustrations.] (London: J. & A. Churchill, 1921. Price 21*s.* net.)

THE object of this book, the author tells us, is to explain physical and physico-chemical terminology and to deal with the physiology of vertebrates from the point of view of the physical processes involved.

To do this adequately would require a knowledge of physics which the author evidently does not possess. His unit of force is "the dyne at 15° C." or "the poundal at 59° F." Small wonder if, starting from such a definition, he goes on to confuse, and even to equate to each other, force and kinetic energy, thereby arriving at an entirely original mathematical relation between certain properties of the molecules of a solid on the point of liquefaction. We next learn that the vapour pressure of a liquid is equal to the gaseous pressure above it, and in a later chapter we find Newton's law of cooling applied to a problem in the conduction of heat.

Not lack of material, but lack of space, makes us refrain from adding to these instances of the author's inadequate graspof the fundamentals of physics. We should have been content to say less about them but that the author claims to view the problems of life through a physicist's eyes and we wish to repudiate the distorted picture. We welcome the book, nevertheless, as an admirable refutation of the idea that physics, if it have the prefix "bio" added to it, thereby becomes essentially a subject for a physiologist to teach.

There is room for a subject which will correlate physics and physiology, but it must come after and not instead of a grounding in physics itself, a grounding so thorough that even the superstructure of biology will fail to shake it.

G. A. SUTHERLAND.

Les Théories d'Einstein Nouvelle Édition Épurée, accrue de notes liminaires. By LUCIEN FABRE. [Pp. 255.] (Paris: Payot et Cie. 1921. Price 7f. 50.)

IN this small volume the author has presented an excellent account of the development of the relativity conception in scientific thought, and of

the work of Einstein in particular. His account is non-mathematical, and will therefore appeal to the lay reader; it does not suffer, as do so many popular accounts, from looseness of argument and inaccuracy in detail. Though the language is precise, and the development of the argument strictly logical, the author possesses an excellent style which makes the volume anything but difficult to read. The most important inaccuracy occurs in connection with the discussion of the displacement of spectral lines by a field of gravitation. The author states that Fabry and Buisson have experimentally verified the existence of the displacement: no reference is made to the adverse evidence obtained by St. John. A somewhat false impression is therefore liable to be created with regard to the present state of the experimental evidence bearing on this crucial phenomenon. We also notice that Morley is throughout spelt Morlay. But these are small defects in what is, on the whole, an admirable account. The revolution produced by Einstein's theory in many of our fundamental conceptions is so great, and interest in the theory is therefore so general, that accurate and elementary accounts such as the one under review are invaluable in enabling the large majority of those interested, who would be unable to comprehend the rigid mathematical exposition, to obtain an intelligent understanding of the theory. H. S. J.

Biological Chemistry. By H. E. ROAF, M.D., D.Sc., etc. [Pp. xvi + 216, with 47 diagrams.] (London: Methuen & Co., Ltd. Price 10*s.* 6*d.* net.)

ACCORDING to the preface the aim of this book is to give a readable account of the chemical processes that take place in living organisms. The book is divided into three sections, of which the first, entitled "Chemical and Physical," is devoted to a brief description of the main groups of organic substance found in cells together with the outlines of physical chemistry applicable to living cells, etc. The second section deals with Anabolism and contains an account of the photo-synthetic process, as well as a chapter on the interconversion of carbohydrates, fats, and proteins. The third section on Catabolism occupies the remaining half of the book and is very naturally illustrated, chiefly from animal physiology, although it must be stated that there is a laudable tendency throughout the book not to ignore the plant physiological aspect, which is more than can be said of many a larger book on the same subject. The book is replete with information, and our only criticism is that it attempts to do so much in so small a compass, as the attempt not infrequently tends to undue condensation and brevity which may lead to confusion in the minds of those not already familiar with the subject. P. H.

A French-English Dictionary for Chemists. By AUSTIN M. PATTERSON, Ph.D. [Pp. xvii + 384.] (New York: John Wiley & Sons; London: Chapman & Hall, 1921. Price 18*s.* net.)

To those who have used this author's German-English dictionary for chemists this companion volume will need no introduction. The particular virtues of the dictionary are its conciseness, the inclusion of many technical words and phrases not met with in other small dictionaries, and the custom of giving pride of place to the chemical rather than the literary meaning of a word. For example, chicane is rendered in the usual type of dictionary as cavil, evasion, quibble, etc., but in this volume as baffle, baffle plate, obstacle, chicanery. Few dictionaries would translate *framage* as crucible stand, or *petit cheval* as donkey-engine. The general get-up of the book is excellent, but the price is perhaps rather more than many are prepared to give. O. L. B.

Chemical Disinfection and Sterilisation. By SAMUEL RIDEAL, D.Sc., and ERIC K. RIDEAL, D.Sc., M.A. [Pp. vi + 313.] (London: Edward Arnold & Co., 1921. Price 21*s.* net.)

THE need for books dealing with what may be called "border-line" subjects is particularly great, as the literature of such subjects is, from their very nature,

usually very scattered; an attempt to collect the available information into one volume calls, therefore, for sympathetic treatment. In *Chemical Disinfection and Sterilisation* the authors have rendered great service to those interested in this question both from its bacteriological and chemical aspect. The book is divided essentially into two parts, the first dealing more particularly with the practical side, such as the disinfection of air, sterilisation and preservation of food, sterilisation of water, wood preservation, etc., and the second with the chemical substances used in disinfection. A chapter is added on bacteriological methods of standardising disinfectents, and the value of the book is increased by a bibliography at the end of each chapter.

Valuable though the book may be as a work of reference, it unfortunately shows many signs of haste in composition, more particularly in the earlier chapters. The authors make a habit of splitting the infinitive, and careless wording is far from rare. Such expressions as "the expense is four or five times less"; "if tinned fruits show a strongly marked crystalline appearance on the interior surface, they are unsafe to be eaten"; "mercuric chloride has even been proposed to be introduced"; "preservation in suitable (officially improved) cold storage rooms" may raise a passing smile, but others are incomprehensible, as, for example: "It has been stated that one part of bleaching powder with two parts of sulphuric acid of specific gravity 1·53 and enough water to cover the powder, evolved three times as much chlorine as when hydrochloric is used. This may be due to the heat generated by the sulphuric acid, as the amounts yielded are theoretically the same. . . . If the insoluble, and therefore solid, sulphate of lime keeps back less chlorine than does the deliquescent calcium chloride, the difference in the yield might be explained."

It is to be regretted that a book, so excellent in other respects, is marred in this way, and it is to be hoped that when the necessity for a new edition arises a careful revision will be made. O. L. B.

Petrographic Methods and Calculations. By A. HOLMES, D.Sc., A.R.C.Sc., F.G.S. (Pp. xx + 516, with 4 plates and 83 text figures). (London: Thomas Murby & Co., 1921. Price 31*s*. 6*d*. net.)

MUCH of the petrographic work which has been published in the past few years tends to show that the subject is undergoing a transition, so far as its methods are concerned, from the qualitative to the quantitative stage, and this book is no less indicative of the change. While most previous treatises on the subject have been mainly devoted to a description of the optical methods used for the determination of minerals, in the present instance, only two chapters are occupied with this branch, most of the remainder being concerned with more quantitative aspects of the science. The scope of the book may be seen from the chapter headings, which are as follows: Petrology, its Scope, Aims and Applications; Specific Gravity of Minerals and Rocks; Separation of Minerals; Optical Examination of Minerals; Examination of Detrital Sediments; Preparation of Thin Sections; Microchemical and Staining Methods; Examination of Thin Sections; Textures and Structures of Rocks; Chemical Analyses and their Interpretation; Graphical Representation of Chemical Analyses. While, in the chapter on Specific Gravity, the treatment of the subject is very full, and even includes a detailed description of Day and Sosman's apparatus for high temperature work, the omission of any reference to recent work in the thermal expansion of rocks is noticeable. In the section on the optical examination of minerals a brief account of the petrological microscope ought to have been included, in order to render the book more or less self-contained. The description of the methods of examination of detrital sediments is largely based on Boswell's well-known works on Sands, and must be regarded as giving a good account of a subject which, until recently, was somewhat neglected. The discussion of mechanical analyses, however, is rather inadequate, one notable omission being any

reference to Schöne's work. The treatment of rock texture contains a commendable criticism of the idea of "order of crystallisation," which has permeated petrographic work for so long; it cannot be too strongly emphasised that, owing to the fact that minerals in rocks consolidate through temperature ranges, and that the order in which they appear, in solid form, depends on the physico-chemical conditions prevailing, there can be no such thing as a "normal order." In this chapter, however, the discussion of crystallites is misleading, since it is based on Vogelsang's classification, which is now generally regarded as defective, while the statement on page 352 that in graphic granite, quartz and felspar are present in eutectic proportions likewise requires correction, as it has been shown that this structure cannot be a eutectic. While the chapters on analysis contain no description of the chemical methods, a large mass of somewhat heterogeneous information is included. The discussion of equilibrium diagrams and mineral systems is too brief to be of much utility, while the reference to the necessity for the determination of titanium dioxide (p. 380) is rather belated, as this oxide has been generally determined in rock analyses for the past decade.

While the author in places has perhaps gone beyond the limits of his title, the book can unhesitatingly be recommended as a mine of information on the subject, the lucid treatment and clear discussions greatly enhancing its value from the point of view of the student. In future editions the rather large number of misprints might be corrected, the most notable being the omission of a line on p. 484, while some of the mathematical symbols, notably the signs for square root on p. 473 and 492, are by no means clear. A. S.

Catalogue of the Fossil Bryozoa (Polyzoa). The Cretaceous Bryozoa (Polyzoa). Volume III: The Cribrimorphs. Part I: By W. D. LANG, Sc.D., F.G.S. [Pp. 269 + cx, with 8 plates and 115 text-figures.] (London: British Museum Natural History, 1921. Price £1 10s.)

IT is difficult, within the limits of a short review, to do justice either in appraising or criticising a detailed work of this nature. The second of these can be done quite briefly, for it mainly concerns matters of preference. We think it is rather a pity that the term Bryozoa is used instead of the more familiar Polyzoa. While it is not fair to imply that recent forms are not considered in this work, yet at the same time we cannot help feeling that, in the literature of the Polyzoa in general, and perhaps here also, too sharp a separation is kept between living and fossil forms.

The book is divided into two portions: I. Introductory, and II. Systematic. The second part is a very careful list of the large collection of Cretaceous Cribrimorphs in the possession of the British Museum. Of each species the author gives a full synonymy, the diagnostic characters, a description, the distribution, a list of the individual specimens in the collection, and frequently also critical remarks. Useful keys to the sub-families, genera, and species are provided. These, with the full index and splendid drawings of Miss G. M. Woodward, add much to the value of the work. This portion of the volume has evidently involved a great deal of labour. As this is the first work that deals with the Cretaceous Cribrimorphs as a whole, it is sure to be in great demand by all students, and can well serve as a model for workers in allied fields of study.

The introductory part of the book is also valuable, and should be read by anyone engaged in the difficult task of a detailed systematic study of any group of animals. It may be that experts on these matters will differ from the author in regard to some of his interpretations or of his application of certain theoretical principals to particular cases. This much, however, is certain: the author has done his utmost to set forth the ideas that have governed his treatment of the subject, and has succeeded in doing so clearly

and concisely. All technical terms have been carefully defined and the various procedures followed lucidly explained. It will hardly be possible in the future to wonder, when the author used a certain term, whether he meant one thing or something different; the matter can be decided by reference to the introduction, and thus one of the pitfalls of systematic work has been avoided from the outset.

With this introductory part are also included a full bibliography, a detailed note on the place and stratigraphic horizon from which all the specimens have come, a series of lists of the zonal distribution of the species as accurately as they can be determined, and a list of all the genera, with a reference to the description of their genotypes.

The author is to be congratulated upon producing such a valuable volume, noteworthy, not only for the facts it contains, but also for the distinct advance that it makes in our knowledge of the systematic relations of the members of this group. It will no doubt stand for long as a classic, and deservedly so, since it represents a great deal of original research and embodies many features that could be adopted with advantage by all workers in systematic biology.

C. H. O'D.

Report No. 1 for the Year 1920: Fisheries and Marine Biological Survey. By J. D. F. Gilchrist, M.A., D.Sc. [Pp. v + 111, with 9 plates and 2 charts. Cape Town, South Africa, 1921).

Under the joint auspices of the Provinces of the Cape and Natal and representatives of the fishing industry, it was found possible to appoint a Fisheries Survey Committee to conduct an investigation into the fishing possibilities of the coastal waters of the Union of South Africa. A vessel, Survey Ship *Pickle*, formerly a whaler, was acquired and adapted as far as possible for the purpose. During the period May–December explorations were carried out from both Cape Town and Durban, and they resulted in a considerable addition to the previous information on the habitat of certain fishes of more or less economic importance. The committee were fortunate in being able to secure the services of Professor Gilchrist as director of this survey work, for he combines wide training with an extensive knowledge of the South African Marine Fauna. The present report gives a fairly detailed account of the results of the year's work, and must be gratifying to the committee. From the tone of some of the remarks, it would appear that the establishment of the Survey is not upon a permanent basis. We sincerely hope that the funds and government support necessary for its continuance will be forthcoming, for it is obvious that not only will the results be of value to science in general, but they will be particularly valuable to the industries in South Africa. We should like to congratulate Professor Gilchrist on the good start he has made, and wish the enterprise every success.

C. H. O'D.

Across Mongolian Plains: A Naturalist's Account of China's Great North-West. By R. C. Andrews. [Pp. xxiv + 276, with a map and 39 illustrations by Yvette Borup Andrews. (D. Appleton & Co., New York and London, 1912. Price $5.00 net.)

As will be remembered, the American Museum of Natural History in 1916–17 sent an expedition along the frontiers of Tibet, Burma, and Yün-nan in China, and this added considerably to our knowledge of the animal life of this not readily accessible region of Asia. This policy of the exploration of unknown Asia was continued in 1918, when Mr. and Mrs. Andrews went farther north into Mongolia. The present volume is an eminently readable and well-illustrated account of their year's trip.

It seems almost a sacrilege to take anything so prosaic as a motor-car across these vast plains that once re-echoed to the thunders of the horsemen

of Genghiz-Khan, yet such was the means of entering this land adopted by the expedition. The modern invention was used in a very striking way to test the speed of some of the animals on the hard, level plains. It was found, for example, that the wolf, even in its opening burst of speed, did not travel faster than thirty-five miles an hour as registered by the speedometer. On the other hand, the antelope (*Gazella gutturosa*), can keep up for some time a speed of between thirty-five and forty miles an hour, and then, when frightened, by being fired at, can increase this to between fifty-five to sixty miles, although this is only maintained for a mile or so.

The book also contains information on a number of other mammals and birds, particularly on the Argali and the Wapiti. It is also useful for the account it gives of the ordinary life of the peoples of this little-known land, and forms most enjoyable reading from cover to cover. C. H. O'D.

Typical Flies. A Photographic Atlas. By E. K. PEARCE. Second Series [Pp. xiv + 38.] (Cambridge: at the University Press, 1921. Price 15*s*. net.)

THIS atlas is supplementary to the one published in 1915, and consists of a series of 125 half-tone illustrations of British Diptera, either in their adult or immature stages. Many of the figures are evidently reproduced from remarkably clear photographs. The latter must have often been executed under difficulty, since it is almost impossible, with the magnifications used, to get all parts of an insect in sharp focus simultaneously. The book should prove a help to the beginner to sort out some of the members of this difficult order into their respective families. It needs, however, to be used in conjunction with a textbook (that of Williston being almost the only one in the English language), or the student will soon find himself relying solely upon appearances and will remain ignorant of the characters upon which the families are based. The work of Williston should at least have been quoted in the list of books given in the preface.

Mention is made of species to be added later, which suggests a third series of *Typical Flies*. In the event of an additional part being produced it is to be hoped that a reduction in price will be possible. The sum of twenty shillings, for the two parts already issued, is a rather high price to pay for a series of photographs of insects without any, except very meagre, letterpress relating to the order and families to which they belong. A. D. IMMS.

Insect Transformation. By PROF. G. H. CARPENTER, D.Sc. [Pp. xi + 282, with 4 plates.] (London: Methuen & Co., 1921). 12*s*. 6*d*. net.

PROF. CARPENTER'S book describes in clear language the varying degrees of change among insects during their growth from the time they issue from the eggs until they appear as perfectly formed adults. It deals with an abundance of material of interest not only to the entomologist, but to any intelligent person upon whose thoughts natural phenomena have some hold. In order that these changes may be fully appreciated, an introductory account of the form and structure of a typical insect is desirable, since all development tends towards this adult state. With this object in view, Prof. Carpenter devotes about twenty-four pages to the elements of insect morphology. In the succeeding chapter he explains first the simple and afterwards the more complex cases of insect metamorphosis. In a later chapter he discusses those insects that are wingless: some primitively so, and others secondarily so, the latter often through adopting a parasitic mode of life. The primitively wingless forms are of importance in that they provide an insight into how metamorphosis first arose as a definite, although inconspicuous, process of change in the course of growth. Another chapter is devoted to the bearing of metamorphosis upon classification, and the importance which is ascribed to it in our present-day grouping of the different orders of insects. The sur-

roundings of growing insects are treated at some length, and Prof. Carpenter describes many life-histories of great interest, and emphasises the remarkable adaptive modifications that are found in relation to differences of environment. The final chapter is devoted to a discussion of some of the problems of metamorphosis. The book is not an advanced and highly technical exposition of its subject; nevertheless, it is not so elementary that the biologist can afford to neglect it. It is thoroughly up to date, and incorporates the results of much recent research in its pages. The more inquisitive general observer of animal life will learn from Prof. Carpenter the significance of the factors underlying metamorphosis, while the entomological student will learn to view the facts, that he has so often observed, in their true perspective as parts of a continuous whole. It may be added that the book is well printed, clearly illustrated, and issued at a remarkably low price.

A. D. IMMS.

Origin and Evolution of the Human Race. By ALBERT CHURCHWARD, M.D., M.R.C.P. [Pp. xv + 511.] (London: George Allen and Unwin. Price 45s. net.)

DR. CHURCHWARD'S extraordinary volume appropriates the distinction of proclaiming man's birthplace (in the same terms as, but with no acknowledgment to, Charles Darwin), of setting up an unprecedented classification of the human species, demolishing Professors Sollas' and Schimper's calculations concerning the Great Ice Age, exploding all the prevailing notions concerning Piltdown Man, and, in brief, giving "irrefutable" answer to most of the major and minor questions of Anthropology and Ethnology.

Despite the author's assurance that "his work gives the key to the world, which hitherto has been lost," his data insufficiently demonstrate the unique claims advanced. The attempted explanation of man's distribution by imaginary land connections is too gross to demand refutation; Egypt as the "source of culture" has been put forward elsewhere, in a scientific form, by Professors Elliot Smith and Perry.

It may be frankly stated of Dr. Churchward's book that throughout he has stoutly maintained his individuality and independence of thought and theory. No respecter of persons, he is equally caustic whether proclaiming his "gnosis," revealing "agglutinated" languages, condemning national politics, or putting private persons "right."

The spelling is not always careful; amongst other errors, "Reisner" persistently becomes "Meisner," and "Sumerians," "Sumarians." The essential criticism of the work, however, does not lie in its grammatical errors, its wandering literary style, and the wounded spirit it portrays; but in its neglect of what is valid in our present knowledge. Little advance toward truthful conceptions can be achieved by categorical contradiction and vituperation.

The tedium of the diatribe is relieved somewhat by copious illustration with drawings and photographs—the sources of which are not always stated. What there is in the work of noble metal is so debased by extraneous dross, that the specie our author has to offer seems little likely to find currency, either in popular approval or scientific acceptance.

RAYMOND A. DART.

The Direction of Human Evolution. By EDWIN GRANT CONKLIN, Professor of Biology in Princeton University. [Pp. xiii + 247.] (London: Oxford University Press. Price 12s. 6*d*. net.)

THE earlier part of the work is entirely valuable as a terse, popular statement, conservative in type, of man's evolutionary history. One might have expected the recent work in his own country of Guyer and Smith—and perhaps, too, the less generally known genealogies published by Redfield—would have entailed some modification of the author's uncompromising adherence to Weissmannism.

As against his hypothesis that "increasing size of brain . . . leads to

mental and physical instability and disharmony," one does not discover elephants and whales to be neurasthenic; while his further assumption that the human brain is so "highly specialised" that "intellectual evolution has virtually come to an end" will scarcely find universal acceptance.

The analysis of Evolution and Democracy is from the teleological standpoint of "the larger freedom of society," based, however, "on reason and ethics, rather than upon tropisms and instincts." Yet the author's attitude to so-called "instincts" is not clear. "Instincts and emotions," in one case (p. 153), are causative of class, racial, and national hatred; while elsewhere (p. 155) they afford "the firm foundation upon which democracy rests."

Conventional too, is the author's insistence that "reason and consciousness" are the "most distinctive of human traits"—"they have revealed to us a world of evil as well as good, a world of struggle and failure, of suffering and sorrow . . ."; - and that "religion answers to a real human need." The chapters on Science and Religion expand these conceptions. No attempt is made to examine critically the basal postulates; first, whether man's consciousness does differ fundamentally from that of other animals; or, second, what is the origin of the "good and bad" antithesis to which religion provides the answer.

The series of essays is dominated by an expectant and open spirit of inquiry. Their keynote is the liberal doctrine that "love of man is more fundamental than love of country." The work is to be welcomed, not only as the statement of the attitude of mind of one of the foremost of American embryologists, but also because it responds to the call for pertinent biological knowledge in the attempted solutions of human problems.

RAYMOND A. DART.

Exploration of Aïr. Out of the World North of Nigeria. By CAPTAIN ANGUS BUCHANAN, M.C. [Pp. xxiii + 258.] (London: John Murray, 1921. Price 16s. net.)

THIS book is a narrative of a journey to Aïr, an unknown region lying some 600 miles due north of Kano. Although visited by Dr. Barth seventy years ago, the flora and fauna were quite unknown until Captain Buchanan went there on behalf of Lord Rothschild. The value of the collections made on this journey is evident from the large number of species and subspecies, new to science, of mammals, birds, butterflies and moths, lists of which are given in the Appendix at the end of the book.

The surprisingly large number of new things discovered in and around Kano itself shows that little interest can be taken by the Nigerian officials in Natural History!

Captain Buchanan gives us an excellent description of the country, which appears to be not unlike the coastal regions of the Horn of Africa and the Red Sea littoral, and this is further borne out by the mammals, practically all of which have their local prototypes and representatives in the coastal regions of East Africa.

I do not think that many British officers will agree with Captain Buchanan when he says on p. 147 that the mess-room at the fort at Agades, with its furniture made of packing-cases, and cigarette tins serving as salt and pepper pots, and drinking glasses made from wine bottles cut down, is "a fair sample of the humble extent that civilised people can improve upon" the natives' ideas of comfort "when thrown entirely on the scant resources of a wilderness."

The Tuareg's knowledge of the birds and beasts and plants of his country is shared by most of the nomadic races and not a few of those tribes who live mainly by agriculture. This faculty has astonished more than one traveller, who knows only too well the complete ignorance of the subject among the majority of his own countrymen. It is, however, not surprising that a native shepherd who spends the day out in the bush with his herds of camels, sheep, and goats must soon begin to recognise and know by heart the habits of the

various forms of animal life by which he is surrounded. Furthermore, he will early learn to appreciate the fattening value of the trees, plants, and grasses devoured by his flocks and herds, and likewise those of little or no value or of a poisonous nature.

The total lack of appreciation of beauty in nature, I fancy, is common to all the African races. Another peculiarity found in most native races is their inability to distinguish between a pleasant and a disagreeable odour.

Captain Buchanan is to be congratulated on the additions he has made to our knowledge of the fauna of Africa. The extent and value of his collections alone, in a country where animal life is so scanty, are a fair index to the hardships he underwent, his keenness and the toughness of his constitution.

R. E. DRAKE-BROCKMAN.

A History of the Whale Fisheries. By J. T. JENKINS, D.Sc., Ph.D. [Pp. 336, with 12 plates.] (London : H. F. & G. Witherby, 1921. Price 18*s*. net.)

THIS is the third book, by the same author, which the present reviewer has had the pleasure of reviewing in these pages ; it is also the best. The book is divided into eight chapters, as follows : " Whales and their Classification "; " The Economics of Whaling "; " The Early History of Whaling " (to 1623) ; " The Dutch Whalers Predominant " (1623–1750) ; " The Bounty System "; " The Southern Fishery "; " The American Whale Fisheries," and " The Last Phase of Whaling," followed by a copious bibliography, seven appendices, and an index. It is also liberally annotated with many footnotes, and there are twelve excellent plates.

Originally whales were hunted for their oil (train oil). The word has nothing to do with railways, but is derived from the Dutch " traan," a tear, i.e. a drop. But early in the nineteenth century it was gradually displaced by other illuminants, and the discovery of petroleum in 1859 sealed its fate as a means of lighting. A superior kind of oil was derived from the Sperm Whale, as also was spermaceti and ambergris, which latter is worth its weight in gold. "Whalebone" was at one time the most important product of the fishery. Certain species of whales carry some 300 plates of this material, which hang from the upper jaw, acting as a strainer in detaining its food, their length being from 10 to 12 feet in the Greenland Whale. Most of the whalebone goes to Paris, where it is used for stiffening silk fabrics. The oil is used for soap-making, the lower grades chiefly for the manufacture of lubricants, and during the War large quantities were used in the manufacture of glycerine for the making of explosives.

The meat of most species, when fresh, can be eaten, and some is canned for sale as human food ; in fact, in a modern factory, practically everything is used except the "spout"—to borrow a well-known Chicago aphorism. The author humorously suggests that, had Mark Twain seen a whale that had been dead for a few days, he would have compared the smell of it to Limburger cheese.

In the historical sections, students will find much rare and interesting data with which to amplify their notes. One learns that the privateering of the *Alabama* embraced whalers. But one would like to have heard something more about the *Mayflower* and its connection with the whale fisheries. One also learns that there was a whale bounty system, on the lines of the better-known herring bounty—certainly the terms "blubber" and "bounty" are euphonious!

The early methods of whaling were brutal, but with the invention of the harpoon gun in 1860, improved later, modern methods with instruments of precision are much more expeditious and, shall we say, humane ?

But the deadly perfection of the instruments now employed will practically exterminate some of the species pursued unless legislation such as outlined by the author is adopted.

We learn from the Press that the writer has granted permission for the book to be translated into the French language. A. W.

A Textbook of European Archæology. By R. A. S. MACALISTER, Litt.D., F.S.A. [Pp. xv + 610, with 184 illustrations.] (Cambridge: At the University Press. Price 50*s.* net.)

THIS book is the first volume of a series which the author proposes to publish. The present work is composed of eleven chapters, and deals with what is known of prehistoric man from the period when Professor Macalister considers it proper for him to appear on this planet—to the cultural phase just preceding the Neolithic civilisations—which mark the close of the Stone Age. Its compilation must have necessitated much reading and a great deal of tedious labour, but the book is obviously the work of one who has not had much actual experience of finding and examining flint implements, and, in consequence, the statements it contains are, in some cases, unreliable. In the short space at my disposal I would wish to comment upon the author's opinions of those earliest efforts of man to fashion flints, which are preserved for us in what are known as eoliths. It would seem that Professor Macalister tries to pour scorn upon these artefacts, and upon those who have collected and examined them. In scientific matters it is not a usual practice to "make fun," in this way, of one's opponents. Not only is it, in itself, regrettable, but it generally leads to reprisals on the part of others who, in like manner, fail to realise the seriousness of the problem as to when man first appeared on this planet. I would first deal with the Kentian Eoliths. The illustrations of these specimens are quite inadequate, and are produced from very indifferent drawings. As Professor Macalister ought to know, these implements have been described and excellently figured by several eminent scientific people, and to publish drawings of this order would not seem to be treating his readers fairly. The illustration of a rostro-carinate specimen is, however, in every way satisfactory. This particular implement is figured in Evans' *Ancient Stone Implements* (Fig. 444), and was accepted as of human origin by Sir John Evans, an investigator who Professor Macalister regards, quite rightly, as a reliable authority in such matters. No illustrations appear of the other types of Sub-Crag (Pliocene) implements, some of which have now been accepted by Professor Sollas in England, and Professor Breuil and Dr. Capitan in France. And again, in this case, Professor Macalister regards, and rightly so, these three archæologists as reliable guides. I regret to notice that in regard to the Sub-Crag implements, the ridiculous objection to their acceptance is put forward that, as these specimens are now found beneath a marine deposit, their makers "must have been at least amphibious, if not actually mermen." If the author had been aware of the extensive terrestrial fauna and flora found in association with these fashioned flints he would not, I think, have made himself responsible for a statement of this kind. In reference to certain experiments in the natural fracture of flint, it is stated that the fractures produced in these experiments differ from those resulting from the action of *bona fide* natural forces. But no satisfactory reason is given for this opinion. It is incorrect to state that to produce delicate retouching upon the edge of a flint, it is necessary to use a "number of special tools." I notice that the old argument (that flint implements occur only in districts where flint abounds) used many years ago against the idea of the human origin of the river-drift paleoliths, is now launched against the eoliths. Both this argument and that of the alleged uselessness of eoliths are threadbare. I can conceive of many more uses for sharp, edge-trimmed eoliths and for rostro-carinates than I can, for instance, for a small, twisted St. Acheul ovate. But all these curious arguments and objections, apparently so dear to the heart of Professor Macalister, are merely academic, and are, it would seem, the outcome of a lack of practical experience in the subject about which he writes. It seems to me incredible that Professor Macalister really regards the well-finished implements, illustrated on p. 226, as representing the first efforts of man in fashioning flint. And it is still more incredible that these specimens should be described as

shapeless flint chips (italics mine) discovered by Professor Commont. It is clear that the author does not understand the nature of a flint-borer, which is so called because it is fashioned by "reverse" flaking at its functional end. The specimens described as "borers" are in reality "points." The flake illustrated in Fig. 77 shows upon its bulbar surface what is certainly an *éraillure* which is detached simultaneously with the flake itself. (This is, in fact, made quite clear in the description of Fig. 75 bis, Plate XIII in Mortillet's *Musée Préhistorique*, from which Fig. 77 is taken. Yet this specimen is said to be an example with a chip crossing the bulb, as though to reduce its inconvenient thickness, or to roughen the tool, and so to facilitate grasping! Finally, on p. 50, it is stated that "flint is very hard—harder than steel." On p. 235 that early Chellean man "perforated flakes of flint," and on p. 593 that the striæ upon some Neolithic flints "might have been made artificially, as ownership marks or, most probably, to roughen them, and so facilitate firm grasping." Comment here is needless.

The book is generously illustrated and can be recommended to archæologists, so long as they remember that they must use their own judgment as to the soundness of the opinions to which it gives expression.

J. Reid Moir.

A COLLECTION OF BOOKS FROM THE McGRAW-HILL BOOK CO., LONDON AND NEW YORK

Electric Welding. By Ethan Viall, Editor *American Machinist*. [Pp. xii + 417, with 329 figures.] (First Edition, 1921. Price 22*s*. net.)

Gas Torch and Thermit Welding. By the same. [Pp. xi + 442, with 85 figures.] (First Edition, 1921. Price 22*s*. net.)

Copper Refining. By Lawrence Addicks, Consulting Engineer, New York City. [Pp. xii + 211, with 40 figures.] (First Edition, 1921. Price 17*s*. net.)

Elements of Fuel Oil and Steam Engineering. By Robert Sibley, B.S., and C. H. Delany, B.S., M.M.E. [Pp xix + 466, with 248 figures.] (Second Edition, 1921. Price 28*s*. net.)

Technical Methods of Analysis. Edited by Roger Castle Griffin, Director of Analytical Department of the Laboratories of Arthur D. Little, Cambridge, Mass. [Pp. xvi + 666, with 29 figures.] (International Chemical Series. First Edition, 1921. Price 33*s*. net.)

Gasoline Automobiles. By James A. Moyer, Director of University Extension, Massachusetts Department of Education. [Pp. vii + 261, with 212 figures.] (First Edition, 1921. Price 11*s*. net.)

The first five books in this list are of an extremely practical type, packed with information of all kinds, both technical and commercial, which is likely to be of use to persons actually engaged, or about to become engaged, in the large-scale working of the various arts described. They are particularly well illustrated with diagrams and photographs of plant, and are well indexed.

The two books by Mr. Viall form a complete treatise covering every aspect of welding; either by oxy-hydrogen, oxy-acetylene, or by the latest process, oxy-thermalene (thermalene is a gas obtained by the action of water on calcium carbide in the presence of crude oil; it is safer in use than acetylene, and gives a slightly hotter flame—6,500° F. as against 6,300° F.), by thermit, by the carbon or metallic arc, or by the electrical resistance method. A brief historical account of the development of each method is first given, and then a description of the necessary lay-out, the procedure, and the class of work for which the process is suitable.

The book on Copper Refining is, apparently, a reprint of a series of articles which originally appeared in *Chemical and Metallurgical Engineering*.

Only the electrolytic method is dealt with, and the processes described are almost entirely based on the author's personal experience.

The book on Steam Engineering is intended as a complete treatise on steam for the use of those actually engaged in the generation of steam with oil fuel. It contains chapters dealing with the elementary laws of thermodynamics, the steam tables, the measurement of temperature, etc.; but the treatment of these theoretical parts of the subject is necessarily very slight, and the book gains its value from the very thorough description of the modifications of ordinary practice rendered necessary by the use of oil, *e.g.* for its chapters on oil-burning appliances, power-plant design, and oil-fuel tests.

The next book contains a representative selection of the analytical methods which have been adopted as standard procedures in a large commercial laboratory engaged in technical analysis. Certain classes of work have been omitted as being of interest mainly to specialists, *e.g.* the analysis of mineral rocks, vitreous materials, drugs, alkaloids and medicines. With these exceptions, the book is remarkably complete, including, as it does, chapters on metals, fuels, paints, oils and soaps, paper, textiles and foodstuffs. The directions given are such as could be readily followed by anyone familiar with analytical technique, but no attempt has been made to give experimental data indicating the accuracy of the methods or to explain the theory underlying them.

The last book in this collection belongs to a different category. It is written for the owner, or prospective owner, of a car, and aims at familiarising him with the various parts of a standard machine so that he may not be entirely at the mercy of the repairer when repairs have to be carried out. The diagrams and descriptions are very clear; the book is of reasonable length, and should serve most excellently for the purpose for which it was written.

Scientific Theism *versus* **Materialism; the Space-Time Potential.** By Arvid Reuterdahl, Dean of the Department of Engineering and Architecture, the College of St. Thomas. [Pp. 298, with numerous diagrams.] (New York: The Devin-Adair Company, 1920.)

In some respects it is a welcome sign of the times that practical men of science show an active interest in those more speculative problems which constitute the domain of philosophy. Only the other day Dr. N. R. Campbell, of the General Electric Co., published, in his imposing volume on *Physics*, a lengthy discourse on some of the logical and philosophical foundations of physical science. And now another electrical engineer, Mr. Arvid Reuterdahl, goes one better and plunges boldly into problems of theology as well as of general metaphysics. The two writers are markedly different in style and in temperament. Dr. Campbell is a heavy writer and a very cautious thinker; Mr. Reuterdahl has a brisk, almost snappy manner, and a most sanguine way of brushing difficulties aside, or even ignoring them altogether. Both authors, however, have this in common—they are both dissatisfied with the current views on the basic assumptions and concepts of science, and both are of opinion that this unsatisfactory state of affairs is partly due to the mathematicians who have usurped the rôle of spokesmen of science, in utter disregard of the fact that mathematics is but one instrument, though a most potent instrument, of science. One may sympathise to some extent with this revolt against the more vociferous mathematicians; but the inarticulate experimental scientists have only themselves to blame for maintaining silence on these fundamental questions. The study of the foundations and methods of science can only gain in accuracy and in fruitfulness by the co-operation of the concrete experimentalists with the more abstract mathematicians, logicians, and philosophers.

The principal themes discussed in Mr. Reuterdahl's book are: Some Inconsistent Concepts of Modern Science; Action at a Distance and the Ether Hypothesis; The Problem of a Physical Substratum; The Model of

the Physical Universe According to the Space-Time Potential; Non-Newtonian Dynamics; Electrolytic Ionisation and Cell Action.

Mr. Reuterdahl's chief contention is that it is impossible to sustain a purely mechanistic interpretation of the universe, and that the attempts to do so rest upon question-begging conceptions and arguments. For instance, Mass, Force, and Energy are conceived in such a way that each of them involves the others; not one of them is defined independently, so that in the end none of them is really explained or accounted for. The plausibility of such transparent fallacies is due to the deceptive smoke-screen of intricate mathematics. On a strictly mechanistic basis the whole phenomenon of interaction is quite unintelligible. The actual facts of cosmic activity and becoming necessitate, according to Mr. Reuterdahl, the assumption of a Transcendent yet Immanent Activity Principle (God). The cosmic elements, according to our author, are "Monons" (i.e. primordial, but created activity centres), "Energons" (i.e. gyratory groups of Monons), "Vitons" (i.e. primordial life-centres), and "Souls" (i.e. primordial centres of consciousness). Out of these elements the universe has been constructed by God, who also maintains it in existence and in running order.

Mr. Reuterdahl's critical work is much more satisfactory than his construction. His ideas may be right, but they are expressed dogmatically and rhetorically rather than justified calmly and philosophically. In fact, the tone of the book is sometimes more scholastic than scientific; and a breath of religious propaganda seems to stir among its leaves. Mr. Reuterdahl may be justified in his taunts against that maid-of-all-work, the ether, endowed by different men of science with such conflicting properties and functions. He describes it as a kind of glue with which materialistic scientists try to keep the world together. He complains that ether is a materialistic substitute for God. But Mr. Reuterdahl does not appear to do much more than substitute God for Ether—a *deus ex machinâ* who creates and sustains our author's world of "monons," energons," "vitons" and souls, and solves all his difficulties. It may be a sage thing for the layman to say "God knows," whenever he is called upon to account for what he does not understand; but that is not science. Nor is it science to explain the unknown by the Unknown.

At the same time there are many interesting things in Mr. Reuterdahl's book, and those who are interested in the kind of problems which it treats of will find in it much that is suggestive, even if they do not always see eye to eye with the author.

A. WOLF.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

Cours Complet de Mathématiques Speciales. Par J. Haag, Professeur à la Faculté des Sciences de Clermont-Ferrand. Tome II. Géométrie. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1921. (Pp. vii + 662). Price 65 frs.

Applied Calculus. An Introductory Textbook. By F. F. P. Bisacre, O.B.E., M.A., B.Sc., A.M.Inst.C.E. London: Blackie & Son, 50 Old Bailey, 1921. (Pp. xv + 446). Price 10*s*. 6*d*. net.

New Mathematical Pastimes. By Major P. A. MacMahon, R.A., D.Sc., Sc.D., LL.D., F.R.S., St. John's College, Cambridge. Cambridge: at the University Press, 1921. (Pp. x + 116.) Price 12*s*. 6*d*. net.

La Loi de Newton est la Loi Unique. Théorie Méchanique de l'Univers. Par Max Franck. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1921. (Pp. 138.)

Introduction to the Mathematical Theory of the Conduction of Heat in Solids. By H. S. Carslaw, Sc.D. (Camb.), D.Sc. (Glasgow), F.R.S.E., Professor of Mathematics in the University of Sydney. Second edition, com-

pletely revised. London: Macmillan & Co., St. Martin's Street, 1921. (Pp. xii + 268.) Price 30*s*. net.

Multilinear Functions of Direction and their Uses in Differential Geometry. By Eric Harold Neville, Professor of Mathematics in University College, Reading. Cambridge: at the University Press, 1921. (Pp. 80.) Price 8*s*. 6*d*. net.

Étude Géométrique des Transformations Birationelles et des Courbes Planes. Par Henri Malet, Ingénieur des Ponts et Chaussées. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1921. (Pp. viii + 261.) Price 32 frs. net.

Principles of Geometry. By H. F. Baker, Sc.D., F.R.S., Lowndean Professor of Astronomy and Geometry, and Fellow of St. John's College, in the University of Cambridge. Cambridge: at the University Press, 1922. (Pp. xi + 183.) Price 12*s*. net.

Practical Least Squares. By Ora Miner Leland, B.S., C.E., Dean of the College of Engineering and Architecture and the School of Chemistry, University of Minnesota. New York: McGraw-Hill Book Company; London: 6 and 8 Bouverie Street, E.C., 1921. (Pp. xiv + 237.) Price 15*s*. net.

The Emission of Electricity from Hot Bodies. By O. W. Richardson, F.R.S., Wheatstone Professor of Physics, King's College, London. Second Edition, with diagrams. London: Longmans, Green & Co., 39 Paternoster Row, 1921. (Pp. viii + 320.) Price 16*s*. net.

Mémoires sur l'Électromagnétisme et l'Électrodynamique. Par André-Marie Ampère. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1921. (Pp. xiv + 110.)

Rays of Positive Electricity and their Application to Chemical Analyses. By Sir J. J. Thomson, O.M., F.R.S., Master of Trinity College, Cambridge, Professor of Experimental Physics, Cambridge. Second Edition. London: Longmans, Green & Co., 39 Paternoster Row, 1921. (Pp. x +234, with 9 plates and 42 figures.) Price 16*s*. net.

Philosophy and the New Physics. An Essay on the Relativity Theory and the Theory of Quanta. By Louis Rougier, Professeur Agrégé de Philosophie, Docteur ès Lettres. Authorised Translation from the Author's Corrected Text of " La Matérialisation de l'Énergie," by Morton Masius, M.A., Ph.D., Professor of Physics in the Worcester Polytechnic Institute. Philadelphia: P. Blackiston's Son & Co., 1012 Walnut Street. (Pp. xv + 159.)

La Physique Théorique Nouvelle. Par Julien Pacotte, Docteur ès Sciences. Préface de M. Émile Borel, Membre de l'Institut. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1921. (Pp. vii + 182.)

Éléments d'Électricité. Par Charles Fabry, Professeur à la Sorbonne. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel. (Pp. viii + 199.) Price 5 frs.

Statique et Dynamique II. Par Henri Béghin, Professeur a l'École Navale. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel. (Pp. 208.) Price 5 frs.

Ions, Electrons, and Ionising Radiations. By James Arnold Crowther, Sc.D., F.Inst.P., University Lecturer in Physics as Applied to Radiology, University Demonstrator in Experimental Physics in the Cavendish Laboratory, Cambridge. Third Edition. London: Edward Arnold & Co., 1922. (Pp. xii + 292.) Price 12*s*. 6*d*. net.

Elementary Chemical Microscopy. By Émile Monnin Channot, B.S., Ph.D., Professor of Chemical Microscopy and Sanitary Chemistry, Cornell University. Second Edition, partly rewritten and enlarged. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. xv + 479, with 160 diagrams.) Price 25*s*. net.

A Textbook of Inorganic Chemistry. By A. F. Holleman, Ph.D., LL.D.,

F.R.A.Amst., Professor Ordinarius in the University of Amsterdam. Issued in English in Co-operation with Hermon Charles Cooper. Sixth English Edition, Revised. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. viii + 513, with 80 figures.) Price 19*s*. net.

Laboratory Exercises in Applied Chemistry, for Students in Technical Schools and Universities. By Dr. Wilhelm Moldenhauer. Authorised Translation by Lawrence Bradshaw, D.Sc., Ph.D. London: Constable & Co., 1921. (Pp. xii + 236, with 36 figures in the text.) Price 12*s*. 6*d*. net.

Introduction to General Chemistry: An Exposition of the Principles of Modern Chemistry. By H. Copaux, Professor of Mineral Chemistry at the School of Industrial Physics and Chemistry of the City of Paris. Translated by Henry Leffman, A.M., M.D., Member of the American Chemical Society and of the (British) Society of Public Analysts. Philadelphia: P. Blakiston's Son & Co., 1012 Walnut Street. (Pp. x + 195.)

Analytical Chemistry, based on the German Text of F. P. Treadwell, Ph.D. Translated and Revised by William T. Hall, S.B., Associate Professor of Analytical Chemistry, Massachusetts Institute of Technology. Volume I, Qualitative Analysis. Fifth English Edition. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. xvii + 597.) Price 23*s*. net.

The Popular Chemical Dictionary: A Compendious Encyclopædia. By C. T. Kingzett, F.I.C., F.C.S. Second Edition. London: Baillière, Tindall & Cox, 8 Henrietta Street, Covent Garden, 1921. (Pp. viii + 539.) Price 21*s*. net.

Organic Syntheses: An Annual Publication of Satisfactory Methods for the Preparation of Organic Chemicals. Editorial Board: Roger Adams, Editor-in-Chief, University of Illinois, Urbana, Illinois; Hans Thacker Clarke, Eastman Kodak Co., Rochester, N.Y.; James Bryant Conant, Harvard University, Cambridge, Mass.; Oliver Kamm, Parke, Davis & Co., Detroit, Mich. Vol. I. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. vii + 84.) Price 8*s*. 6*d*. net.

A Course of Practical Organic Chemistry. By T. Slater Price, O.B.E., D.Sc., Ph.D., F.I.C., Director of Research to the British Photographic Research Association, and Douglas F. Twiss, D.Sc., F.I.C., Chemist, Dunlop Rubber Co. London: Longmans, Green & Co., 39 Paternoster Row, 1922. (Pp. xiv + 239, with 35 figures.) Price 6*s*. 6*d*. net.

Soaps and Proteins: Their Colloid Chemistry in Theory and Practice. By Martin H. Fischer, Doctor of Medicine, Eichberg Professor of Physiology in the University of Cincinnati, with the collaboration of George D. McLaughlin and Marian O. Hooker. New York: John Wiley & Sons; London: Chapman & Hall, 1921. (Pp. ix + 272.) Price 24*s*. net.

Principes de l'Analyse Chimique. Par Victor Anger, Maître de Conférences de Chimie Analytique à la Sorbonne. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel, 1921. (Pp. 224.) Price 5 frs.

Fungi: Ascomycetes, Ustilaginales, Uredinales. By Dame Helen Gwynne-Vaughan (formerly H. C. I. Fraser), D.B.E., LL.D., D.Sc., F.L.S., Professor of Botany in the University of London and Head of the Department of Botany, Birkbeck College. Cambridge: at the University Press, 1922. (Pp. xi + 232, with 196 figures.) Price 55*s*. net.

Farm Management: A Textbook for Student, Investigator, and Investor. By R. L. Adams, Professor of Farm Management, University of California. New York: McGraw-Hill Book Company; London: 6 & 8 Bouverie Street, E.C., 1921. (Pp. xx + 671.) Price 20*s*. net.

Ocean Research and the Great Fisheries. By G. C. L. Howell, Indian Civil Service, Acting Secretary British Fisheries Society, formerly Director of Fisheries, Panjab. Oxford: at the Clarendon Press, 1921. (Pp. 220, with 20 plates.) Price 18*s*. net.

Selection in Cladocera on the Basis of a Physiological Character. By Arthur M. Banta. Washington: Published by the Carnegie Institute, 1921. (Pp. 170.)

L'Hérédité. Par Étienne Rabaud, Professeur à la Faculté des Sciences. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel, 1921. (Pp. 190.) Price 5 frs.

A Textbook of Zoology. By the late T. Jeffery Parker, D.Sc., F.R.S., Professor of Biology in the University of Otago, N.Z., and William A. Haswell, M.A., D.Sc., F.R.S., Emeritus Professor of Biology in the University of Sydney, N.S.W. In two volumes. London: Macmillan & Co., St. Martin's Street, 1921. (Vol. I, pp. xl + 816, with 713 figures; Vol. II, pp. xx + 714, with 503 figures.) Price 50*s*. net.

The Causation of Sex in Man: A New Theory of Sex based on Clinical Materials, together with Chapters on Forecasting or Predicting the Sex of the Unborn Child and on the Determination or Production of Either Sex at Will. By E. Rumley Dawson, L.R.C.P., M.R.C.S. Third Edition. London: H. K. Lewis & Co., 1921. (Pp. xii + 226, with 22 illustrations.) Price 7*s*. 6*d*. net.

British Mammals. By Archibald Thorburn, F.Z.S., Vol. II. (Pp. vi + 108, with 25 coloured plates and 9 pen-and-ink sketches.) Price 10 guineas the complete work.

Monograph of the Lacertidæ. By George Albert Boulenger, LL.D., D.Sc., F.R.S., Vol. II. London: Printed by Order of the Trustees of the British Museum, 1921. (Pp. viii + 451.)

Diet and Race: Anthropological Essays. By F. P. Armitage, M.A., Director of Education for the City of Leicester. London: Longmans, Green & Co., 39 Paternoster Row, 1922. (Pp. 144.) Price 7*s*. 6*d*. net.

Fuel and Lubricating Oils for Diesel Engines. By W. Schenker, Chief Engineer (Diesel Engine Department) Sulzer Frères, Société Anonyme, Winterthun, Switzerland. London: Constable & Co., 10 Orange Street, 1921. (Pp. xii + 114.) Price 10*s*. net.

An Introduction to Engineering Drawing. By J. Duncan, Wh.Ex., M.I.Mech.E., Head of the Engineering Department of the Municipal College, West Ham. London: Macmillan & Co., St. Martin's Street, 1922. (Pp. ix + 158.) Price 4*s*. net.

Switching Equipment for Power Control. By Stephen Q. Hayes, A.B., E.E., Fellow American Institute Electrical Engineers; Switchboard Project Engineer, Westinghouse Electric and Manufacturing Co. New York: McGraw-Hill Book Company; London: 6 and 8 Bouverie Street, E.C., 1921. (Pp. vii + 463.) Price 20*s*. net.

The Autonomic Nervous System. By J. N. Langley, Sc.D., Hon. LL.D., Hon. M.I., F.R.S., Professor of Physiology in the University of Cambridge. Part I. Cambridge: W. Heffer & Sons, 1921. (Pp. 80.) Price 5*s*. net.

The Psychology of Medicine. By T. W. Mitchell, M.D. London: Methuen & Co., 36 Essex Street, W.C. (Pp. vii + 187.) Price 6*s*. net.

The Morphologic Aspect of Intelligence. By Santa Naccarati, M.D., Sc.D., Ph.D. Archives of Psychology, edited by R. S. Woodworth, No. 45, August 1921. New York: Columbia University Press; London: G. E. Stechert & Co., 1921. (Pp. 44.) Price $1.10.

The Psycho-Analytic Study of the Family. By J. C. Flugel, M.A., Senior Lecturer in the Department of Philosophy and Psychology, University College, London. London, Vienna and New York: The International Psycho-Analytical Press and George Allen & Unwin, 1921. (Pp. x + 259.) Price 10*s*. 6*d*. net.

Benign Stupors: A Study of a New Manic-Depressive Reaction Type. By August Hoch, M.D. Cambridge: at the University Press. New York: The Macmillan Company. (Pp. xi + 284.) Price 14*s*. net.

Considérations sur l'Être Vivant: l'Individu, la Sexualité, la Parthénogénèse et la Mort, au point de vue Orthobiontique. Par Charles Janet. Deuxième Partie. Beauvais: Imprimerie Dumontier et Hague, 23 rue Jules-Miche et, 1921. (Pp. 196.)

Technique of the Teat and Capillary Glass Tube. Being a Handbook for the Medical Research Laboratory and the Research Ward. By Sir Almroth E. Wright, M.D., F.R.S., Principal for the Institute for Pathology and Research, St. Mary's Hospital, London, W. With the collaboration of Leonard Colebrook, M.B., B.S. London: Constable & Co., 10 and 12 Orange Street, Leicester Square, W.C. (Pp. xxvi + 384, with 151 figures and 3 plates.)

Mentally Deficient Children: Their Treatment and Training. By G. E. Shuttleworth, B.E., M.D., Fellow of King's College, London, and W. A. Potts, M.A., M.D., Medical Officer to the Birmingham Committee for the Care of the Mentally Defective. Fifth Edition. London: H. K. Lewis & Co.; Philadelphia: P. Blakiston's Son & Co., 1922. (Pp. xviii + 320, with 21 plates.) Price 10*s*. 6*d*. net.

Essai Philosophique sur les Probabilités. Par Pierre-Simon Laplace. Paris: Gauthier-Villars et Cie, 55 Quai des Grands-Augustins, 1921. (Vol. I, pp. xi + 101; Vol. II, pp. 108.)

A History of the Association Psychology. By Howard C. Warren, Stuart Professor of Psychology, Princeton University. London: Constable & Co., Ltd., 1921. (Pp. ix + 328.) Price 16*s*. net.

A New System of Scientific Procedure: Being an Attempt to Ascertain, Develop, and Systematise the General Methods Employed in Modern Inquiries at their Best. By G. Spiller. London: Watts & Co., 17 Johnson's Court, Fleet Street, E.C., 1921. (Pp. ix + 441.)

The Calendar: Its History, Structure, and Improvement. By Alexander Philip, LL.B., F.R.S. Edin. Cambridge: at the University Press, 1921. (Pp. xi + 104.) Price 7*s*. 6*d*. net.

The Belief in God and Immortality. A Psychological, Anthropological, and Statistical Study. By James H. Leuba, Professor of Psychology in Bryn Mawr College. Chicago and London: The Open Court Publishing Company, 1921. (Pp. xxviii + 333.) Price $2.50.

Priestley in America, 1794–1804. By Edgar F. Smith, University of Pennsylvania. Philadelphia: P. Blakiston's Son & Co., 1012 Walnut Street. (Pp. 173.)

À Propos de Bergson, Remarques et Esquisses. Par Firmin Nicolardot, Docteur ès lettres, Ex-chargé de Cours à l'Université de Lille. Paris: Librairie Philosophique. J. Vrin, 6 Place de la Sorbonne, 1921. (Pp. 173.) Price 4.50 frs.

Conduct of the Understanding in Philosophy and Physiology, and How the Abuse of Nature affects Development of Mind. By Timothy O'Mahoney. With an Introduction by L. G. Taylor, M.A. (Oxon). London: George Routledge & Sons; New York: E. P. Dutton & Co. (Pp. xi + 146.) Price 6*s*. net.

An Introduction to Applied Geography. By Alexander Stevens, M.A., B.Sc., Lecturer in Geography, University of Glasgow. London: Blackie & Son, 50 Old Bailey. (Pp. x + 309.) Price 6*s*. net.

The Microscope. A Simple Handbook. By Conrad Beck. London: R. and J. Beck, 68 Cornhill, E.C., 1921. (Pp. 144, with 131 figures.) Price 2*s*. 6*d*. net.

The Evolution of Consciousness. By A. Wyatt Tilby. London: T. Fisher Unwin, Adelphi Terrace. (Pp. 255.) Price 15*s*. net.

La Fonte (Élaboration et Travail). Par le Colonel Jean Rouelle. Paris: Librairie Armand Colin, 103 Boulevard Saint-Michel, 1921. (Pp. 192.) Price 5 frs. net.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

INDEX TO VOL. XVI (1921—1922)

I. ARTICLES, ESSAYS, ESSAY-REVIEWS, CORRESPONDENCE, AND NOTES

II. AUTHORS OF ARTICLES

III. AUTHORS OF BOOKS REVIEWED